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INDIAN AGRICULTURAL
RESEARCH INSTITUTE, NEW DELHI.

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PROCEEDINGS
OF THE
AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

VOLUME 83

1940



THE AMERICAN PHILOSOPHICAL SOCIETY
PHILADELPHIA
1940

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POST-NATAL DEVELOPMENT OF THE HEAD*

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* Supported in part by a grant from the Penrose Fund of the American Philosophical Society.

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ABSTRACT

This is a study of cephalic changes in size and in various proportions, made upon several hundred children, chiefly utilizing longitudinal series. Noteworthy is the action of gravity in flattening the head as the child begins to sit up and to walk. Indeed, the vertical cranial dimension increases slowly or not at all during juvenility, perhaps again also due to gravity. The decrease in head height is accompanied by increase in width, while girth increases regularly. The vertical dimensions of the face grow more irregularly than those of the cranium due to enlargement of sinuses and tooth alveoli and perhaps also to masticatory muscle pull. Before birth the cephalic proportions are modified to fit the lower part of the uterus. The proportions of the head change rapidly for some weeks after birth due to cranial deformation at birth. After the first year, growth in length is much greater than in width in dolichocephalics, and the reverse holds for brachycephalics. The post auricular segment of the head tends to increase more rapidly than other elements of length and this despite the backward migration of the auricular opening. Up to 4 years as the head width is increasing, the transverse frontal index decreases, but after that, as the forehead widens, the index decreases.

In relation to total face height, height of the nose increases most rapidly after birth (largely at the expense of the upper lip) and that of the chin next. The interpupillary distance increases absolutely as the head enlarges, but decreases relatively to bizygomatic width. The mean angle between the pupils, with apex at the interporal line, decreases from mid-gestation to 9 or 10 years and thereafter increases slightly. During the first 2 post-natal years the annual increment of the cephalic module falls from 150 to 15, the head growth is nearly completed in this period.

Absolute cephalic and facial measurements are less in the female than male, in relation to her smaller size. The mean cranial capacity is less in girls than boys, absolutely, and in relation to stature and body modulus. The relative height of the lower jaw and the ratio of face height to face width are also less in girls than boys.

The head dimensions grow fastest in the standard series, least in the mongoloid dwarfs, and cranial capacity in relation to stature and body modulus is least in the standard series—it has advanced farthest from the infantile relations. The cephalic index is greatest in the mongoloids, while relative width of forehead to maximum head width increases roughly with intelligence. The distance between inner eye angles is large in mongoloids in childhood owing to low nasal root and epicanthus, and becomes relatively small at adolescence.

Among races the Negroes have the greatest head length and head girth, while the Nordic children have the largest post auricular head segment and the largest head height/length ratio. The Nordics have the relatively widest foreheads. The physiognomic and morphologic face heights, lower jaw heights, and relative face widths of the Negro are largest.

Individual curves of head dimensions are strikingly variable. They increase to puberty, with adolescent accelerations. The individual growth of the post auricular part of the skull and the head height/length ratio follow diverse types. The relative forehead width tends to increase with age in all individuals studied, while cranial capacity increases with age absolutely but not relatively to body size. Chin height grows diversely in different individuals probably under the influence of special glandular activities. The other elements of facial height also vary with individual differences in tooth and sinus development.

Three pairs of monozygotic twins were subjected to anthropometric analysis during development. Most of the curves of growth and change with age lie close together in any pair, in other cases the curves lie further apart but run parallel courses. In dizygotic twins the curves are usually far apart and far from parallel. The head growth of individuals of 4 types of dwarfs was followed, also that of 2 acromegalics and a microcephalic.

PREFACE

The development of the general form and changes in proportion of the head have been studied by many authors of diverse training: embryologists, obstetricians, pedologists, educationalists, psychologists. To few has come the opportunity of studying the development of the head of the same individual for a number of years, especially from childhood to puberty. This paper will, it is hoped, be regarded as a reconnaissance in this unexplored field.

The study has been made possible by the cooperation of many collaborators, whose participation in this series of studies has been recognized in my 1939 paper, especially the Normal Child Development Study of Columbia University (Dr. Myrtle B. McGraw), the Brooklyn Orphan Asylum, Letchworth Village (Superintendents Little and Storrs and Directors Humphreys, Martz and Pettit), and the Brooklyn Home for Children. The Carnegie Institution's Department of Embryology and the New York City Morgue have been utilized in the mass studies.

For statistical work I have been indebted at every stage to Miss Catherine Carley, Mr. William Drager and Mr. Wilfred D. Hallock and for literary work to Mrs. Inger M. Andersen. Especial mention must be made of the collaboration in this study of Miss Olive Renfro of Letchworth Village who took roentgenograms of about 100 children repeated during a number of years. These series are, I believe, unique, certainly nothing has been published based upon such series.

The work of obtaining and collecting the data for this book has been rather expensive. The expense has been met mostly by the Carnegie Institution of Washington, in part by Columbia University and Letchworth Village, State of New York, by a grant, especially appreciated since it came at a critical time, from

Mr. Samuel S. Fels; and more recently by a grant from the Trustees of the Mary W. Harriman Fund which was especially esteemed since through this Fund the late Mrs. Harriman still speaks with gentle voice, sound judgment and tenacious purpose and because the present product is the fruition of a work which her relation to Letchworth Village, as one of its Board, made possible. The grant from the Penrose Fund of the American Philosophical Society came as the manuscript was about completed.

Abbreviations

In the following pages, including the text figures, the following abbreviations are used.

BOA,	Brooklyn Orphan Asylum
LVD _I ,	Letchworth Village Development Series I
LVD _{II} ,	Letchworth Village Development Series II
I,	Idiots
M,	Mongoloid dwarfs, from various institutions
a.p.	Antero-posterior
f,	female
m,	male
p a.	Per annum
5 7	years signifies 5 years and 7 months

Davenport '39, or '39, refers to a paper "Post-natal Development of the Human Outer Nose," published in the *Proceedings of the American Philosophical Society*, Vol 80 (1939).

I. INTRODUCTION

The human head is an extraordinary organ both on account of its relatively great size and because it encloses man's relatively large brain and carries his relatively reduced face.

The head is, in its origin, a plastic organ. The brain is laid down early and at about the same time the eyes and otic vesicles appear on the sides of the head. The stomodeum breaks through to form the mouth and gullet. Mesenchyme penetrates around all the soft parts and already at 10 weeks from fertilization the membranes around the brain begin to form bony plates, while the base of the brain is still cartilaginous. Even at birth the bones of the cranium (the paired frontal and parietals, the occipital and the laterally placed temporals) still are separated by membranous spaces, the fontanelles, which permit movement of the bones in

various ways, even to overlapping. Some peoples have taken advantage of this condition of the bones of the skull in infancy to cause deformation of the skull by binding, as among several tribes of Amerindians. Extremely flat occiputs arise from the custom found in certain nomadic people of placing the infant in a carrying basket with the head bandaged against a flat board. Also, the cradle of the Armenians is of such a nature as to cause similar flattening of the occiput. It follows that one must indeed be cautious in ascribing differences in head form solely to heredity.

The cranium of the fetus and infant, made up of membrane and thin sheets of bone, covers the brain, and with the growth of the brain becomes molded upon it. On the other hand, the cranium and its enclosed brain are subject to molding while the fetus lies in utero, to a deformation during parturition (usually very temporary) and to a certain deformation by gravity in early post-natal life.

To learn more in detail about these post-natal changes "longitudinal" measurements were made on 34 babies from near birth to 2 or 3 years of age, upon about 10 normal children of a children's home from 3 to 6 years, upon some 100 boys and 50 girls of the Orphan Asylum of Brooklyn at between 5 and 15 years of age, and upon about 250 "feebleminded" boys and girls of Letchworth Village between 5 and 20 years of age. The following discussions are based on the data thus collected.

The nearest approach to this study is one by Saller ('28) who utilized data collected by another in an Oberrealschule in Lubeck on boys measured repeatedly during 5 years or more, to the number of 9 at ages 10-14, 15 at ages 11-15, 22 at ages 12-16, and so on down to 13 at ages 15 to 19 years. The results obtained by Godin from his classical studies on 100 boys followed individually from 13 to 18 years seem not to have been fully published. (Cf Godin, '23.) Six of the dimensions of head and face discussed here are considered by Saller. Goldstein ('39) has published a study of change in head length, head width and cephalic index of Jewish boys and girls measured repeatedly during 2 to 6 years, and in this study has included certain of the data on these dimensions obtained by the Harvard Growth Study. Data on head length and width taken repeatedly on 1500 boys and girls in the Harvard Growth Study, but not summarized, are given by Dearborn, Rothney and Shuttleworth ('38).

II. ABSOLUTE DIMENSIONS

1 *Growth of Head Length*

General—The growth of the head in length has been traced, generally by obtaining averages, by a number of observers: West ('94), Reuter ('03), Ranke ('05), Rose ('05), Schwerz ('10), Tschepowkowsky ('11), Godin ('23), Saller ('28), Bayley and Davis ('35) and Bayley ('36), and others. The results show, as might have been anticipated, owing to the well-known racial differences, that head lengths of a Swiss sample (Schwerz) and a German one (Ranke) do not coincide.

In general, in these males, the mean head length is about 12 cm at birth. Growth increases very rapidly during the first two years to about 16.5 cm, and then slows down, reaching at 19 years about 18.5 cm. That is, the speed of growth is about 4.0 cm. per annum the first year, 1.0 cm. p.a. the second, 0.4 cm p.a. the third, 0.3 cm p.a. the fourth, gradually diminishing to 0.1 cm at about 9 to 10 years and thereafter. Apparently head length may continue to grow to middle life, partly due to increase of the frontal sinus. Cf. Pfitzner ('99), p. 364.

Owing to the great racial and familial differences in length of the head the "longitudinal" study of head dimensions is especially important but has not hitherto been made for a prolonged period, or at least not reported.

Methods and Material.—Head length was measured with the compass calipers, of which the apex of one leg was held lightly on the skin at the glabella while the other was passed along the skin in the sagittal plane at the occiput, seeking the maximum reading. In a second technique the occiput was placed in contact with the wall, head held with the Frankfort horizontal placed and held horizontally. The distance from wall to glabella was measured horizontally with the depth measurer of Hermann, Rickenbach & Sohn (Zürich). The measurement with calipers was, on the average, about 2 mm. less than that with the depth measurer. This difference was due to the fact that the hair of the occiput formed a pad of about 2 mm. thickness above the scalp. That is, the caliper tip comes in contact with the scalp at two points, the depth measurer at only one. Naturally, this difference varies with the thickness of the occipital hair. The horizontal glabellar distance from wall was used in comparison with the horizontal distance of tragon and orbit from wall.

It may be noted that to compare head length in the living with skull length, on the average 11.5 mm. is to be subtracted (Wilder and Wentworth, '18, p. 102)

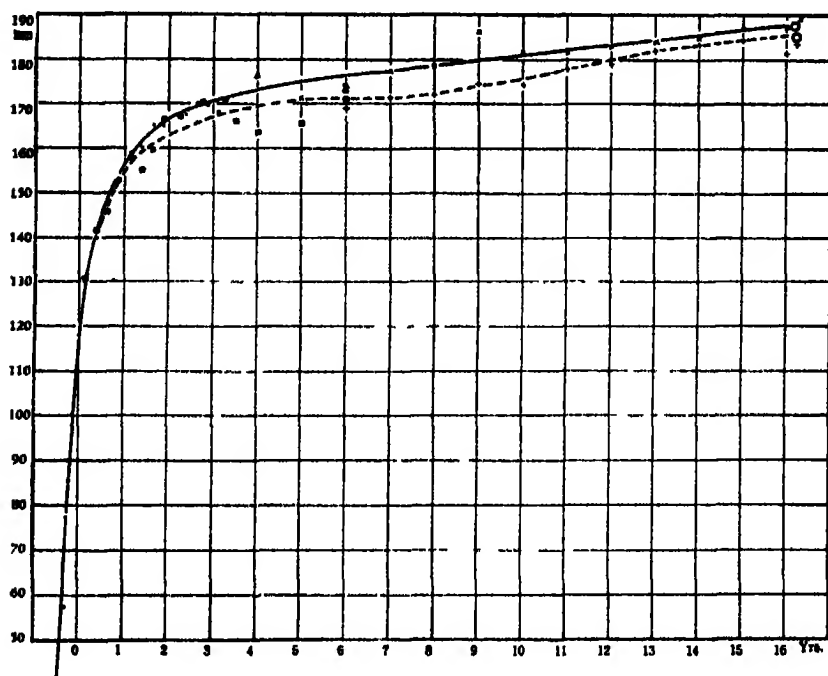


FIG 1 Mass curves of growth of Head Length, embryo to maturity, U S white, male and female, based on data of Carnegie Embryological fetuses, Babies Hospital, Brooklyn Home and BOA Prenatal dots Babies male, dots, female, (circles Brooklyn Home male, triangles, female, squares BOA male, X, female, +

Sexual.—Fig 1 gives the mean head length found in my measurements at different ages. The pre-natal part of the main curve is from white embryos at the Department of Embryology (Carnegie Institution of Washington, kindness of Dr G. L. Streeter, Director). The first 3 post-natal years are based on measurements made by me in the Normal Child Development Study, Columbia University, at the Babies Hospital, New York. Nearly all were Nordics. From 3 to 16 the children measured were mostly school children residing at the Orphan Asylum of Brooklyn, about 95 p.c. Nordics.

Starting from 4.5 cm. at a little later than the mid-gestation period (end of the 5th calendar month) our curve of growth of head length in the male increases to 12 cm. at birth, or at a rate of 195 mm. per annum. After birth the rate diminishes as follows:

	Mm	Rate p a
Birth to $\frac{1}{2}$ yr	25	50
$\frac{1}{2}$ yr to 1 yr	10	20
1 yr to 2 yrs	10	10
2 yrs to 3 yrs	3 8	3 8
3 yrs to 5 yrs	3 4	1 7
5 yrs to 16 yrs	14	1 3

Calling the adult head length 190 mm. then the fraction of the adult size attained at various ages is as follows

	%		%
Birth	63	3 years	89
End 6th month	76	5 years	91
1 year	82	10 years	95
2 years	87	15 years	98

Hardly another dimension of the body is so precocious in its development as to be three-quarters finished at 6 months post partum. Stature is only about 40 per cent finished at that time.

TABLE 1
MEAN HEAD LENGTH OF NORMAL BABIES, BOTH SEXES, MM

Month	Bayley		Present Study	
	Boys	Girls	Boys	Girls
	mm	mm	mm	mm
1	131 0	127 4	130 64	129 81
S E	11	10		
2	134 3	130 9	133 50	134 10
3	137 0	132 9		
4	139 4	135 6	140 38	141 77
5	143 1	137 3		
6	143 8	140 8	147 92	145 62
7	146 7	142 0		
8	148 5	144 2		
9	150 6	146 6	152 92	152 50
10	153 0	148 1		
12	156 8	152 8		
15	160 4	155 0	158 70	155 08
18	162 3	157 6	160 00	160 00
24	166 4	163 0	165 00	
30	170 0	164 8	166 74	
36	172 2	167 8	169 00	
48	174 6	170 3	168 30	

While among our babies the difference in the average head length between the sexes is small, at least to the middle of the third year, the smoothed data reveal an excess of mean head length for the male. Thus my findings agree with those of Bayley ('36,

p. 4, 5). Her differences are rarely half the corresponding standard deviations. Yet the trend is consistent. The total number of the children measured by her was about 50, by me 47 at the start, 18 at 36 months.

Kugler ('32) finds a mean head length for Swiss babies at birth of 117.4 mm in males, 115.7 mm in females, an excess of 1.7 mm. in the male. Bayley's sex difference at birth of 3.6 mm., or 3 p.c., seems somewhat too large, especially as the excess of the stature of male over female at birth is only between 1 and 2 p.c. (cf. Martin, '28, pp. 281-286). The mean head length at birth of 120 mm found for the well babies in my study is perhaps too small. For Nordic stock the mean head length for girls at birth is about 125 mm. and for boys about 127 mm., with a normal range of about 10 mm. above and below the mean.

After the first postnatal year the head length of the male grows more rapidly than does that of the female, at puberty they tend to approach, just as statures do, at from 12 to 14 years, and then to separate slightly (Fig. 1). The mean dimension from adolescence on is about 5 mm. greater in the male.

Social —The growth of head length was secured for 5 groups of Nordic males, BOA (standard), LVD_{II}, LVD_I, I (Idiots) and M (Mongoloids) in decreasing order of intelligence. Except for a reversal in the positions of I and LVD_I the mean of head lengths is smaller with decreasing intelligence at each age from 6 to 16 years. The first 2 series tend to converge toward 16 years. At 16 years the mean head lengths are BOA, 188 mm., LVD_{II}, 187.5 mm., LVD_I, 182 mm.; I, 184 mm., M, 170 mm. The mongolian idiots are notoriously short headed. The curve of the LVD_{II} female approaches that of the LVD_{II} male at 13 years.

Racial —The growth of head length was obtained for males of 4 racial types, all of the two LVD series. The Negroes have at all of the given ages the longest heads, the Italians the shortest. Thus at 14 years the series runs Negroes, 184, U.S. Nordics, 180.5, Jews, 177, Italians, 175. The growth in length of the adolescent Negro skull in our series increases at the rate of 1 mm. p.a. until 13 years, and then at the rate of 2 mm. p.a. to 17 years, when it is 190 mm. Weninger ('27) gives 191.35 as the average of French West African Negroes. The Nordic head lengths increase only 0.5 mm. p.a. at 9 to 12 years, then 0.8 mm. p.a. to 16 years; and the Jewish head lengths increase at about the same rate.¹ The

¹ Cf. Goldstein's ('39, p. 203) data.

Italian head length increases from 7 to 18 years 15 mm., or at the rate of 1.4 mm p a. This short head length is associated with Italian short stature. In Filipino children, from 6 to 16 years, the head length grows on the average 1.6 mm per year—much more than in Nordics (Bean, '15, p. 525).

The great length of the skull of Negro children has been noted repeatedly. Hrdlička in 1900 (p. 52) found a mean excess of around 5 mm. over the white children, and Rouma ('21, p. 76) found the Negro children of Cuba to be 1 to 4 mm. longer headed at various ages from 6 to 14 years than the corresponding whites of Spanish origin. Herskovits ('27, p. 303) has measured a series of "Negro" children, largely from New York City, but also from the South. His series runs uniformly above mine, which suggests a difference in technique. But the Herskovits measurements were made by different persons and partly by von Luschan, whose technique is not known. Herskovits' series rises to 195 mm. at 19 years. The series of Day's ('32) measurements of head length in adult full-blooded black males (students and others) gives a mean of 191.31 which again suggests that a different technique was employed by some of the Herskovits group. Todd ('29, p. 63) finds for head length of Western Reserve standard Negroes 193 mm., for Western Reserve whites, 188 mm. He includes head lengths of 109 Negroes of Howard University (data sent him by Herskovits) which give a mean length of 198 mm., again exceptionally high.

The superior length of the Negro head is, of course, based chiefly on superiority of skull length. Thus Todd ('28, p. 114) finds a mean length of 100 American "Negro" skulls of 186.2 ± 0.5 mm. as compared with that of 100 white skulls of 181.42 ± 0.43 mm.

Individual —The curve of averages is apt to be distorted by the coming into the curve, midway, of a single markedly abmodal individual or by the dropping out of one that has been included at earlier ages. Individual curves are, on the other hand, usually smooth. Fig. 2 gives the growth curves of head length (also width and height) of a white boy (3) and a colored boy (1) at Letchworth Village. At the top, the head length curve of a second Negro (2) is introduced.

Considering only the 3 top curves at this time, one notes that they all rise, from left to right. They lie at different levels, that of the white boy lowest. The latter curve rises about 0.5 mm. per year from 8 to 14, then the slope increases to 1.5 at the time of the

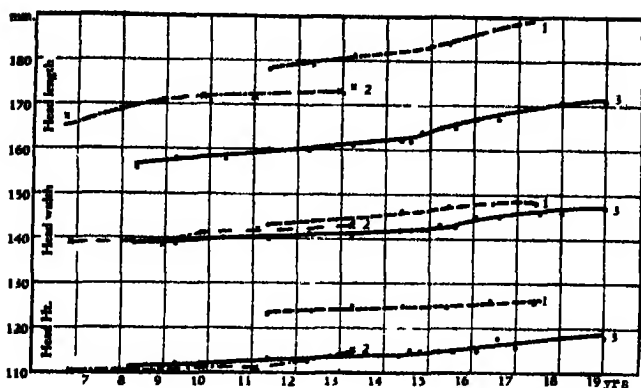


FIG 2 Individual curves of growth in three main cranial dimensions for three boys 1, FB, No 48, colored, 2, HT, No 3, colored, 3, MH, No 2 LVD_I series

adolescent spurt. After 18 years the slope falls to 0.5 again. The colored boy (No 1) at 14 years has a 20 mm longer head than the white boy. It increases about 0.6 mm p.a. from 2 to 14, then to 1.5–1.3 after 15 years.

Still other individual curves of length, width and height of 3 boys, 2 Nordics, Nos 1 and 2, and one Italian, No 3, are shown

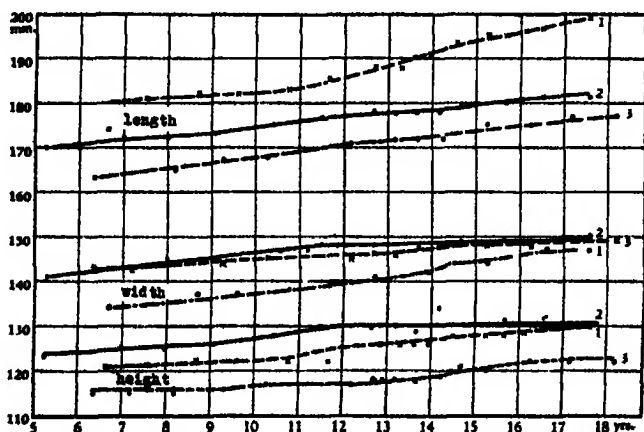


FIG 3. Individual curves of growth in three main cranial dimensions, maximum length, maximum width, ear height, for three white boys 1, SB, No 55, 2, CH, No 83, 3, DF, No 57 LVD_I series

in Fig. 3. In the length curves, No. 1 advances most rapidly from 0.5 mm. p.a. to 1.5 mm. p.a. at 13 years. The height curve of No 2, on the other hand, rises more nearly uniformly at the 0.5 rate from 6 to 17 years. Incidentally, one notes that the boy whose

head length increases the most rapidly has the narrowest and next to the highest head of the three. The heads undergo (relative) changes of proportion. Of our 3 cases the longest head increased most rapidly in length, but with larger numbers Goldstein ('39, p. 207) finds the reverse to be more generally true.

The extreme dimensions of the cranium found by us in medium grade feeble-minded boys are shown in Fig 4. V E. (No 59) has a

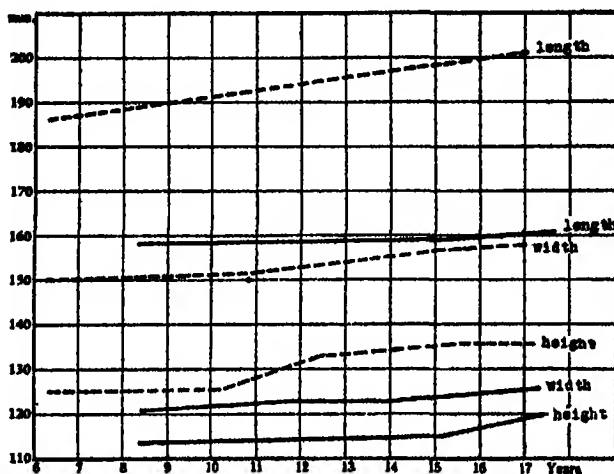


FIG 4 Individual curves of growth in the three main cranial dimensions for a long headed boy, V E, No 59 ----, and a short headed boy, E H, No 79 ———

head length that advanced from 187 mm at 6.4 to 202 mm at 17 years, giving a mean increase of 1.4 mm. p.a. At age 12 he had an I.Q. of 80. E H (No. 79), on the other hand, has a head length that advanced from 158 mm. at 8.5 to 161 mm. at 17.4, giving a mean increase of 0.3 mm p.a. At age 12.8 he had an I.Q. of 58. The latter boy's head length was the smaller by 31 mm. at 8.5 years and the smaller by 41 mm. at 17 years. Thus the early difference has become exaggerated. The head of No. 79 is, as the other curves of Fig. 4 show, extraordinarily narrow and low. The cranial capacity of No. 59 at 16 years is 1583 c.c., of No. 79 at 16 years, 1040 c.c. The relative cranial capacity (cran. cap. ÷ stature in mm.) at 16 years for the same 2 boys is 93 p.c. and 71 p.c. respectively. Since the intelligence quotient of these boys at 12 years is, as stated, 80 and 58 respectively the relation between length of head and intelligence is in this case marked.

Familial.—War. family. Fig. 5 shows the growth curves of 5

boys and 2 girls of the War. family of Letchworth Village. Since at the beginning some were young and others much older the curves do not run between the same ages. In none of the 5 boys

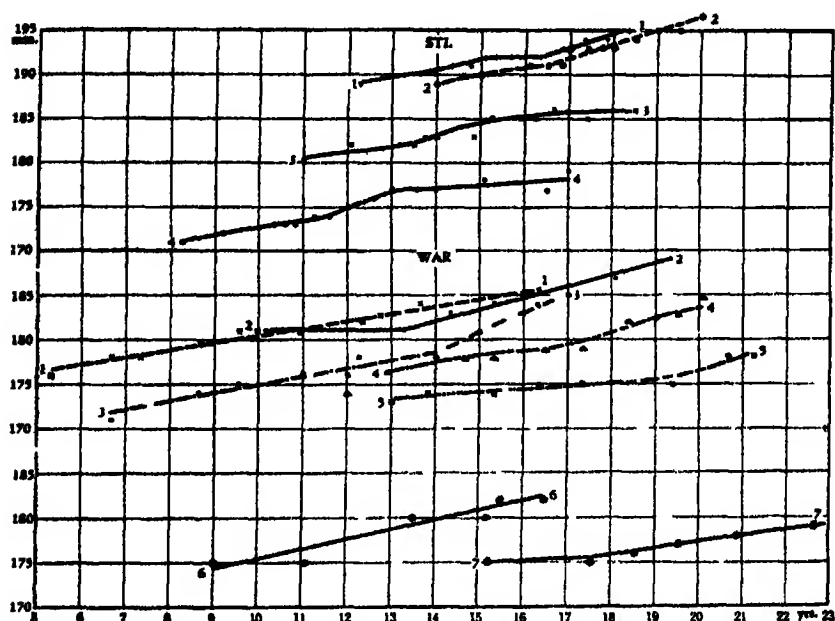


FIG 5 Individual curves of growth of Head Length of members of the Sti (above) and War (below) fraternities Sti 1, LS m, 2, CS m, 3, ES f, 4, MS f War 1, GW m, 2, FW m, 3, SW m, 4, WW m, 5, FW m, 6, MW f, 7, BW f Three different sets of ordinates

(middle) did the curves coincide, although those of No 1 and No 2 agree within a millimeter. What is striking is that all 5 curves run parallel (or nearly parallel) courses—i.e. growth of the brain proceeds with nearly equal steps, at the average rate of about 0.7 p.a. Breaks there are, in two boys at about 14, in 2 boys as well as one girl, after puberty. Before the break the rate of growth had been slow. In the case of No. 2 no growth occurs for 3 years and then growth at the high rate of 1.5. So too is the case of No. 7 (below), from 15.3 to 17.7 years growth was at the rate of 0.25, after 17.7 at the rate of 0.7 mm. p.a. The measurements suggest that something partly inhibited longitudinal growth of the brain (head) for a time and, on the release of the inhibition, growth occurred with enhanced vigor.

Sti. family. In this family the curves of head lengths of the two boys lie above those of the two girls. All run nearly parallel courses and the two boys close together.

Mea. family. Fig 6 gives the curves of growth of head length for 4 boys and 6 girls of the Mea family of Letchworth Village. In the case of two boys, Nos 2 and 3, the curves run practically

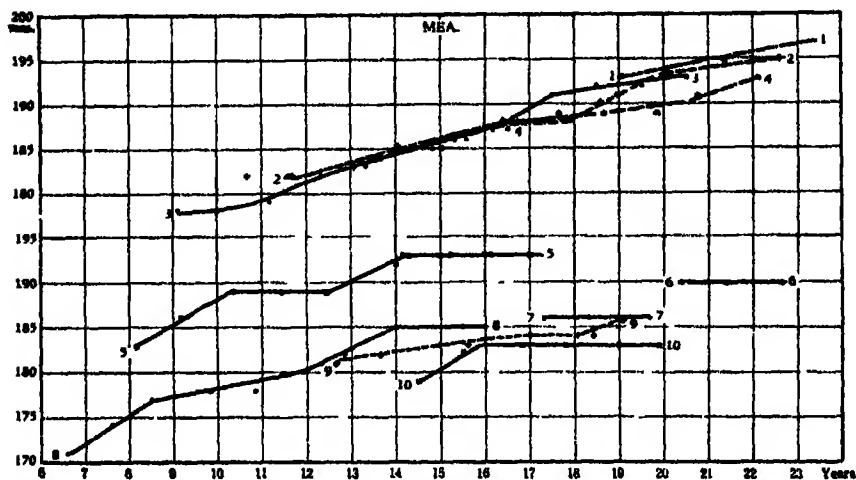


FIG 6 Individual curves of growth of Head Length in 10 members of the Mea fraternity 1, L M m, 2, W M m, 3, G M m, 4, H M m, 5, R M f, 6, M M f, 7, E M f, 8, B M f, 9, N M f, 10, Ma M f Two different sets of ordinates.

the same course. They are drawn a little distance apart to distinguish them. After age 16 the curve of No 3 rises abruptly, probably in connection with his adolescent spurt which occurred in the 17th and 18th years. The curve departs from No 4's, whose adolescent spurt appears to have been delayed until 18-21 years. No 2's spurt occurs at an age between those of No 3 and No 4. The separation of the curves after 16 years is partly due to differences in the age of the adolescent spurt. The rate of advance in the curve of No 2, from 12 to 16½ years, is 1.6 mm. p a. The head length is still increasing at about the same rate after 20 years.

In the lower part of Fig 6 are given the growth curves of head length for the sisters. At 16 years the head lengths for Nos 8, 9 and 10 differ by only 2 mm. No. 5 lies altogether outside the field of the other sisters. The rate of advance in the curve of No 8, from 6½ to 16 years, averages 1.87 mm. p.a. After about 15 years the growth of head length has practically ceased in these sisters.

Special Cases Twins—In Fig 7 are shown some curves of growth of head length in monozygotic twins, the monozygosity having been determined by general physical resemblance, finger

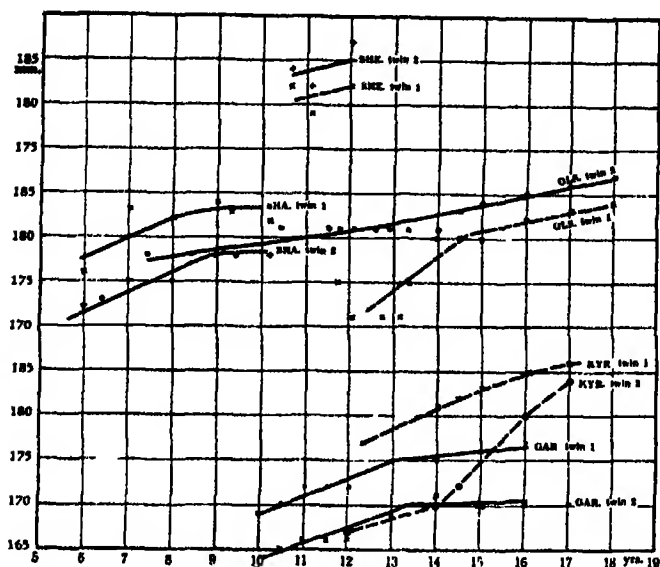


FIG 7 Individual curves of growth of Head Length for five pairs of twins Three sets of ordinates

and palm prints In general, the curves of the pair do not occupy identical positions, but are about 3 to 5 mm apart But they do run roughly parallel courses From some observations made by Bayley ('36, pp 8, 10) it is possible that these differences in head length are in part due to differences in activity during early infancy, leading "the inactive child" to lie "inert on the back of the head for many more hours than the active child," thus perhaps becoming more brachycephalic The actual correlation found by Bayley between activity and brachycephaly was 39 (See also page 73) One cause of dissimilarity in growth of head length in twins is apparently the presence of non-genetical factors in development of the frontal sinus.

In Fig. 8 are presented individual curves of head length, width and height for special cases as described The largest head (No. 3) is that of an achondroplastic; the shortest (No. 4) that of a microcephalic.

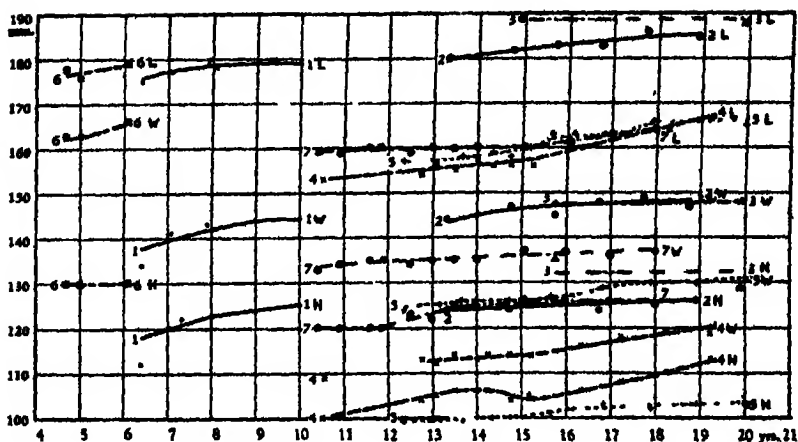


FIG 8 Individual curves of growth of Head Length (L), Head Width (W) and Head Height (H) for seven special cases 1, A A f, cretin, 2, C B f, cretin, 3, M C f, achondroplastic, 4, V R m, microcephalic, 5, A W f, atelotic dwarf, 6, H L m, cretin; 7, L S f, achondroplastic dwarf One set of ordinates

Summary.—The head length is a dimension that increases absolutely before birth (from 4 to 10 lunar mo at the rate of 148 mm. p.a.)—more rapidly than any other bodily diameter in a plane perpendicular to the trunk axis. On the other hand, growth of head length after the first post-natal year increases very slowly, and almost imperceptibly after 3 years. It practically completes its course earlier than any other dimension of the body. The dimension is uniformly smaller in girls than in boys, corresponding to their smaller stature. It is larger in the Negroes than Whites of North European origin, and small in Jews and Italians. In the case of certain white individuals that have been compared the head length and intelligence are associated. In close relatives the development of the head length runs parallel courses, but usually a few millimeters apart. This is true even in monozygotic twins. Posture in bed in early infancy has been suggested as a cause of difference in head length, but it also appears that the genetic causes are primary.

2. Growth of Maximum Head Width

General.—This is the maximum width taken with compass calipers in a line perpendicular to the sagittal plane. The points usually lie above and slightly in front of the ear openings. Measurements of this dimension during the growing period have been

made by Ranke ('05), Schwerz ('10), Rösse ('05), Reuter ('03), West ('94) and others. To transmute on-skin measurements to the surface of the skull subtract, on the average, 11 mm. (Wilder and Wentworth, '18, p 102)

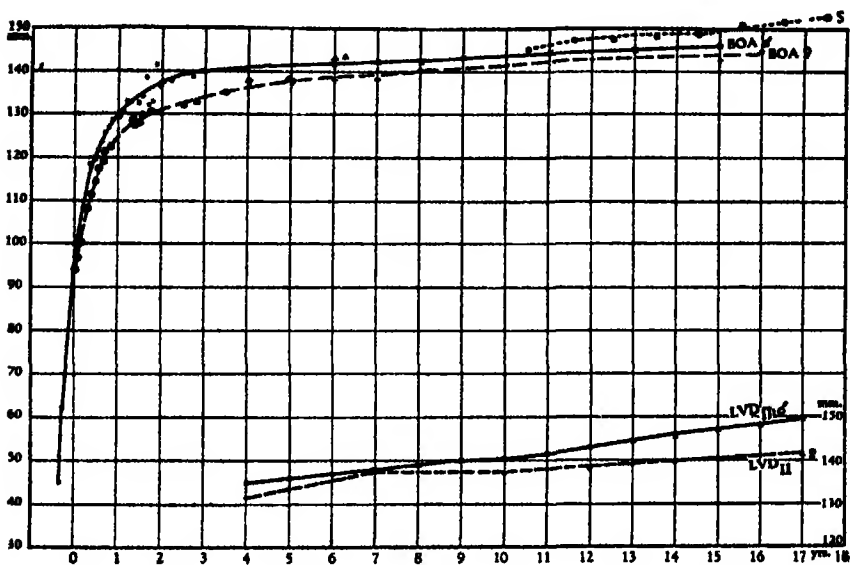


FIG 9 Mass curves of growth in Head Width, embryo to maturity, U S white, male and female Carnegie Embryological fetuses, Babies Hospital (m ——— f — —), Brooklyn Home (triangles) and BOA $\times \times \times$ and $+++$ Also, at S, Saller's ('28) Fehmaraners, m Also LVD₁₁ male and female (right hand set of ordinates)

Figure 9 is a mass curve of mean head width from about 5 lunar months pre-natal to post-puberty. Beginning in the mid-gestational period at 45 mm. width increases with great rapidity to 94 mm. at birth and to 100 mm. during the first post-natal month. Kugler ('32, p 566) finds a mean head width of Zurich males of 92 mm. at 1 day after birth and of 93.1 mm. at 9 days. Ranke finds a mean head width at birth of 99 mm. for males, and 94 mm. for females. The slope of the curve slowly diminishes during the first post-natal year. At the end of 12 months the mean width of the male head is about 131 mm., at 2 years 140 mm., and after that head width undergoes little change—about 5 mm. from 7 to 14 years, or 0.7 mm. p.a. West, Reuter and Schwerz found an increase during that period of 5 mm., Rösse of 4.2 and Reuter of 2 mm. The change in slope from 5 months pre-natal to 14 years is as follows:

AMOUNT OF GROWTH OF HEAD WIDTH IN MM PER ANNUM

Pre-natal .	100 mm p a	1 yr - 2 yrs	9 mm p a.
0-6 mos	50 " "	2 yrs - 3 "	15 " "
6 mos -12 mos	20 " "	3 " -14 "	05 " "

Thus the amount of growth of head width is at first about the same as that of head length, but after 2 years slows up greatly. The early cessation of growth in head width is doubtless associated with the slowing up of increase in brain width. (Cf. Saller, '28, p. 74.)

Sexual.—The head width in the female is, of course, less than that of the male. At birth the difference between the sexes in head width is barely significant, but becomes greater absolutely and relatively with development. Table 2 gives the changes in width with age and sex for the first 4 post-natal years.

TABLE 2
MEAN HEAD WIDTH OF BABIES 1-48 MONTHS OLD

Months	Bayley		Present Study	
	Boys	Girls	Boys	Girls
	mm	mm	mm	mm
1	101.8	98.4	97.76	96.70
2	106.1	103.5	103.32	100.48
3	110.8	107.2	107.93	105.29
4	115.1	111.5	112.13	107.91
5	118.6	111.5	118.30	111.53
6	122.2	117.5	119.15	114.82
7	124.8	121.1	121.18	117.77
8	126.8	121.9	123.73	119.33
9	128.3	123.3	125.90	121.93
10	129.7	125.0	127.00	124.00
12	131.2	126.2	128.00	124.00
15	132.8	127.7	130.50	126.28
18	134.5	129.6		
24	137.6	132.8		
30	138.9	133.0		
36	140.6	134.6		
48	141.9	136.6		

Thus the male diameter exceeds the female by 3 to 4 mm. This confirms the findings of Tschepowkowsky ('11) also.

This difference between the sexes is maintained even to the adult, but becomes reduced to about 2 per cent in the standard series (cf. also Schwerz and others cited by Martin, '28, p. 706).

In the LVD_{II} series the mean head width of the female is uniformly less than of the male from 4 to 17 years except that at

ages 4 to 7 the mean of the female rises sharply and even becomes equal to the male at 7 years (137 mm). Thereafter mean growth is slow to 10 years, increases then a little more rapidly and at 17 years is 8 mm, or less, on the average, in the female than in the male, or about 5 per cent. Stature meanwhile is about 8 per cent less in the female.

Social.—The growth curve of head width has been determined for 4 groups of males of varying mean intelligence. Here the standard BOA group lies at the top, the mongoloid idiots at the bottom. The curves tend to diverge from around 7 or 8 years to about 11, after that there is a slight convergence. During the available periods (7 to 18 years) the mean increase in head height per year is as follows: BOA, 0.61, LVDI, 0.50, I, 0.27, M, 0.49 mm. In general it appears that the two lower grades made a slower increase in head width than the two higher grades. Low intelligence is apparently associated with slower brain growth, in width as well as length.

Racial.—The curves of growth of head width have been determined for 4 racial groups. The curves of which the means are only 3 mm apart at 12 years diverge with age to become 9.5 mm apart at 18 years. The descending order of mean brain width at 9 years is Negro, Jewish, Italian, and Nordic, at 18 years, Nordic, Italian, Jewish and Negro. That is, the order becomes, with age, exactly reversed.

The mean increase per year of the four groups for the years during which they were followed is as follows: Nordic and U. S., 1.08, Italian, 0.80, Jewish, 0.70, Negro, 0.29 mm. (Cf. the data on Jewish children in New York City by Goldstein, '39, p. 203.)

In this series the last (Negro) group is set apart from the others by its markedly slow development of head (brain) width. Since the Negro head length grows faster than that of whites, while the width grows slower, the marked dolichocephaly of the adult Negro is seen to be the result of a special growth process of the head and doubtless of the brain.

Individual.—Figure 10 shows 2 rather extreme cases in the development of head width in two boys, R. Hal. of Nordic stock and I. Stez. a Pole. Although the curves lie at such different levels (at 16 years, 130 mm and 166 mm respectively) yet they run roughly parallel courses, diverging slightly from 9½ to 16 years and then converging slightly toward 18 years. The same

heads differ by about 8 mm. in height Stez is a very large headed boy

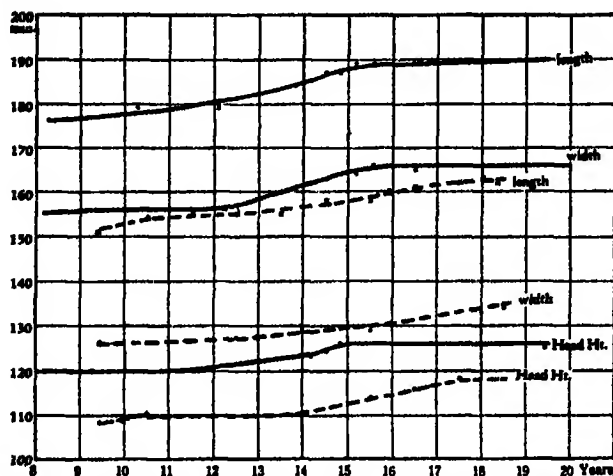


FIG 10 Individual curves of growth of Head Length and Head Width for two boys of LVD₁ series Unbroken line I S, No 93, stature at 16½ years, 167 cm Broken line R H, No 29, stature at 16 years 145 cm, at 17 years 153 cm Single set of ordinates

Babies also are very variable in head width a few days after birth when the babies' individual head form has been regained after the frequently deforming episode of birth Thus 2 babies of the Babies Hospital have linear dimensions in millimeters as follows

No	Length	Width	Height
3504	125	89	72
3507	135	105	96

In the case of No 3504 width is 71 per cent of length, in the case of No. 3507 it is 78 per cent.

Figure 11 shows the individual growth curves of 5 boys. In two, at the top, the growth is exactly alike from 9 to 16 years. Afterwards the curves diverge about 15 mm. The curves advance regularly, but experience an increased slope at 14 years, the time of the adolescent spurt In the next curve below (No. 3) there is a steep gradient (about 1.1 mm. p.a.) from 5 to 12 years. This is followed at 12-14 years by no change, and then a rising slope again, this time of about 0.6 mm p.a. Two bottom curves have slow and gradual slopes with a slight increase at puberty.

Familial.—Figure 12 gives the curves of head width in the Mea. family of Letchworth Village. Two of these curves of the

boys practically coincide in position, though at different ages. They are rectilinear curves, or nearly so, with a slope of 1.5 to 1.9 mm p.a. The curve of No. 3 runs at a 0 slope at 150 mm. from 11 to 14 years, then rises in a 0.5 slope. The curve of No. 4 is level at 148 mm.

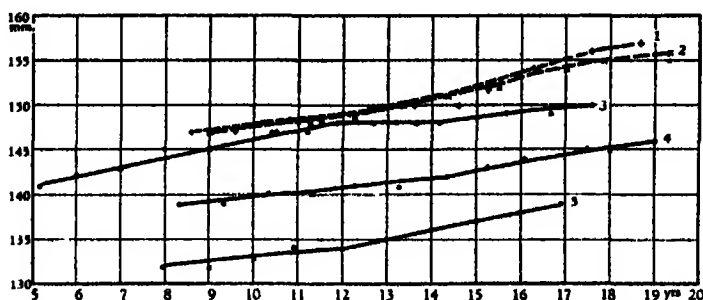


FIG 11 Individual curves of growth of Head Width of five boys of LVD_I series 1, J.C., No. 10, Polish, 2, F.C., No. 28, Irish, 3, C.H., No. 83, U.S., 4, M.H., No. 2, U.S.; 5, G.P., No. 5, Italian

Of the girls the curves of No. 5 and No. 7 run parallel courses hardly a millimeter apart and at a nearly constant slope of about 2.0 mm p.a. No. 6's head width advances after puberty at first 0.3 mm p.a., then after 17 years at 0.7 mm. The other 3 girls, measured only after puberty, show a slow increase, about 0.4 mm p.a.

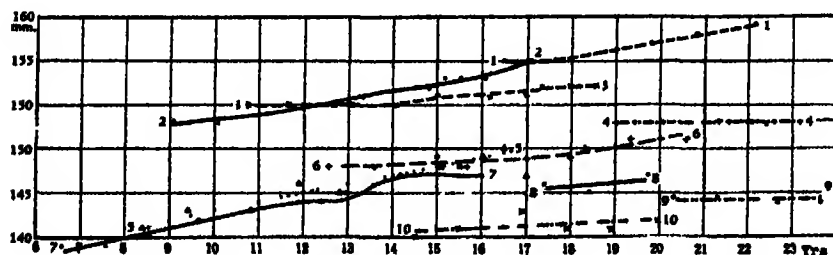


FIG 12 Individual curves of growth of Head Width of members of the Mea fraternity 1, H.M.m., 2, G.M.f., 3, W.M.m.; 4, L.M.m., 5, R.M.f., 6, N.M.f., 7, B.M.f., 8, E.M.f., 9, M.M.f.; 10, M.a.M.f. Two sets of ordinates

In both sexes there seem to be two groups of growth curves, one group with fairly steep gradients, and one with low gradients. The curves are somewhat scattered in position.

Figure 13 shows growth curves of head width for the children of the War. and Sti. families. In both families the curves advance

regularly except for adolescent accelerations. In the War. family two boys have coincident curves of head width growth and the same is true of both boys of the Stl. family. In both families the

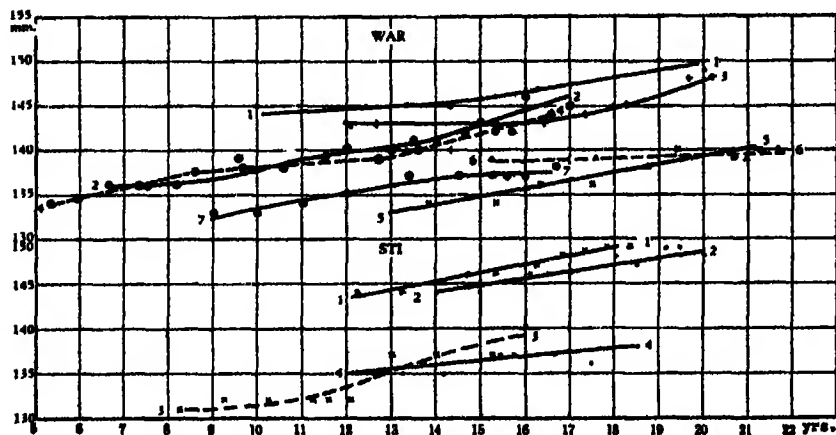


FIG 13 Individual curves of growth of Head Width of members of the War (above) and Stl (below) fraternities War 1, Fk W m, 2, S W m, 3, W.W m, 4, G W. m; 5, Fd W m, 6, B W f, 7, M W f Stl 1, L S m, 2, C S m, 3, M S f, 4, E S f Two sets of ordinates

head width of the sisters differs by only a millimeter or two. There is a coincidence at one point in the curves of the Stl. sisters.

Twins.—Figure 14 shows certain curves of growth of monozygotic twins. The Gar. twins, Nos. 1 and 2, through most of the course of the curves have the same head width. The same is true of the Kyr. twins. The Ols twins differ constantly by about 2 mm. Head width, more than head length, develops similarly in monozygotic twins.

Special Cases —At the top of Fig. 14 are shown growth curves of 3 cretins. They are all characterized by large width dimensions—133 to 150. No. 4, who is part Korean, and cretinous, has at 5 years a head width of 163 mm. An achondroplastic dwarf (No. 3) has a slowly growing head width at 135 to 137 mm., while in the case of retarded No. 5 head width advances rapidly from 125 to 130 mm. Smallest of all is the head width of a microcephalic, V R, which rises from 110 at 10½ years to 119 at 18:2. The slope of V.R.'s curve is steep, 9 mm. in 8 years, or 1.13 mm. p.a. In comparison, Ols. 1 increases 9.5 mm. in 11.5 years, or 0.83 mm. p.a. Apparently the growth of the head width of the microcephalic is retarded before and accelerated after onset of puberty.

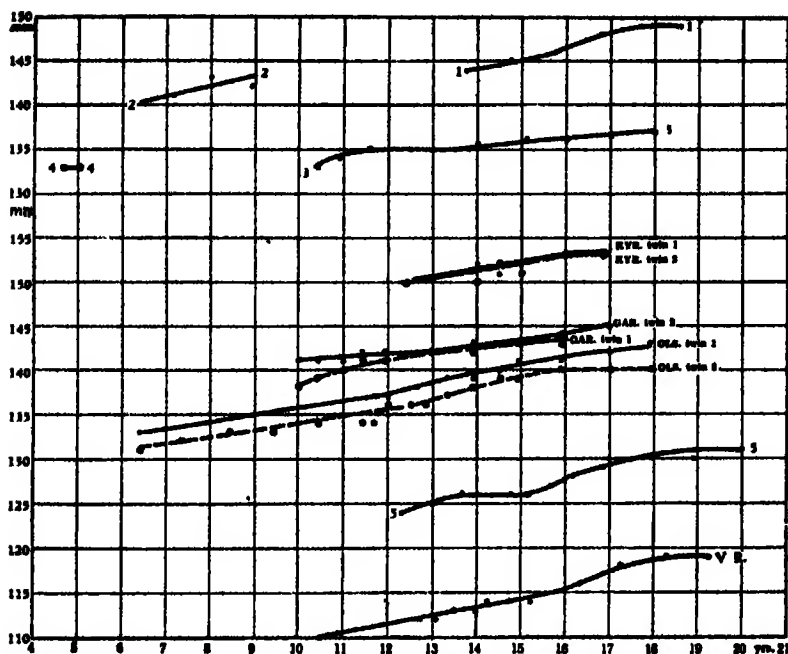


FIG 14 Individual curves of growth of Head Width in three pairs of twins and some special cases as follows 1, C B f, cretin; 2, A A f, cretin, 3, L S f, achondroplastic; 4, H L m, cretin, 5, A W f, atelotic dwarf; V R, microcephalic Two sets of ordinates

Summary—Head width increases, after birth, by the same amount as head length, but after 2 years grows much more slowly than head length. It increases less rapidly in girls than in boys. The more fully developed children in body and mind have the broader heads. The head width of the Negro is apparently precocious in development, but tends after 12 years to be less than that of Whites. The growth of head width in the pairs of monozygotic twins is extraordinarily like.

3. Growth of Head Height

General.—The head height in this study is the distance, perpendicular to the Frankfort horizontal, of the vertex of the head from the upper lining of the external auditory meatus. It is called also "ear height" and dermal portion—apex of Gray and Robinson ('28, p. 327) and of Goldstein ('38, p. 342). This dimension is of peculiar interest because one of the most striking changes in form that the cranium has undergone from *Pithecanthropus* and *Sinanthropus* through *Galilee man* and *Neanderthal man* to the

modern European type is increase in height. Thus by measuring two outlines given in Gieseler ('36, Fig 62) it appears that head height \div gabella-inion distance is in Neanderthal man 40%, in recent Alsatian 64%. It is of especial interest to see how this dimension changes in early child development.

The head height is not easy to measure on the living. I have used the Matiegka head height instrument in all cases except in young embryos.² I used two types of this instrument, an earlier and a later (past 1934) one, and although they calibrated the same yet owing to the fact that in the earlier type the zero point was at the edge of a wedge which easily penetrated between hairs to the scalp in the later type the wedge-edge was replaced by a flat plate which came to rest on a layer of hair, usually about 2 mm from the scalp, the later measurements of the same child gave head height 2 mm more than the measurements taken 6 months earlier, with the other instrument.

Measurements with the same instrument on the same child repeated on the same day did not always agree within two millimeters owing to variations in depth of insertion of the tips of the moveable arms into, and to variations in pressure applied to the upper wall of, the fleshy introitus.

Since few other investigators have used this convenient instrument of Matiegka, and since the result depends to a considerable extent upon the instrument employed, my results are, unfortunately, rarely comparable with the findings of others.

In the case of babies under 1 year of age it was impracticable to use the Matiegka instrument, so measurement was made with the depth measurer from vertex to tragon, if practicable on both right and left sides.

I may add that the method of determining head height by subtracting from vertex height the height of tragon is subject to an error due to the varying value of both of these dimensions as the tone of the muscles that hold the body erect fluctuates.

To transmute ear head height on the living to measurements made on the skull about 11 mm is to be subtracted from the on-skin measurements.

Sexual—Figure 15 shows the growth of the mean ear head height from the 6th fetal month to maturity, as found by me. The fetuses measured are 12 at the Department of Embryology,

² For a discussion of auricular height measurements see Howell, '38

Carnegie Institution of Washington. For the first 3 post-natal years I used the babies at the Normal Child Development Study, Babies Hospital, Columbia University. The means from these groups connected up with the BOA series.

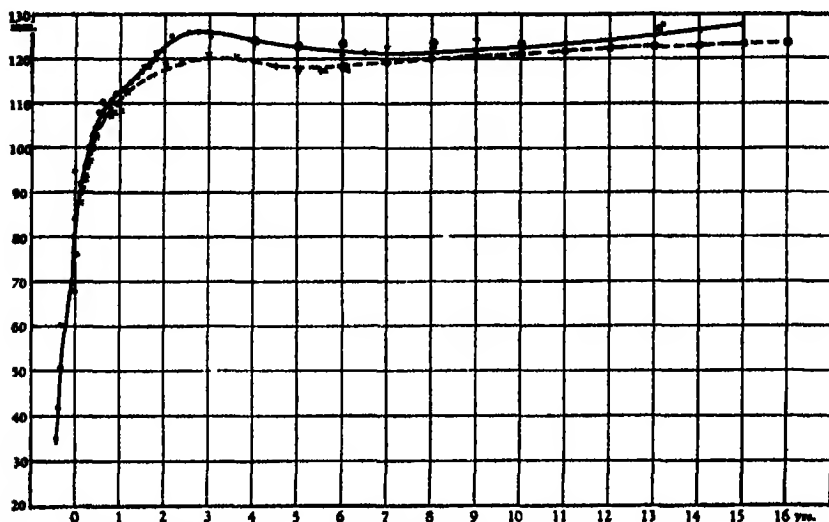


FIG 15 Mass curves of growth of Head Height, embryo to maturity, U S and Nordic, male and female. Dots, fetuses and boy babies and BOA, + in circle, Brooklyn Home, m, X, girl babies, dot in circle, BOA girls, ++ Niggli-Hürthmann. Solid line, m, broken line, f.

The increase in 5 lunar months (20 weeks) is from 35 to 85 mm, or 50 mm, being, in 20 weeks, 2.50 mm. per week, or at the rate of 117 mm p.a. This may be compared with increase in head length during the same period from 54 to 120 mm. or 66 mm in 16 weeks, or 4.11 mm. per week. Also with head width from 45 to 94 mm, or 2.72 mm per week. Thus the head height increases only slowly before birth and after 3 years post-partum almost none at all. This is true also of the Chimpanzee and Gorilla as Krogman's ('31a, p. 107 and '31b, p. 336) studies indicate.

The irregular curve of Fig 15, indicating mean course of growth of head height, is based, from birth to $3\frac{1}{2}$ years, on my measurements at the Babies Hospital. These seem to tie up fairly well with data of Niggli-Hürthmann ('30, p. 86), $4\frac{1}{2}$ to 6 years, and with sparse data of my own from high grade Orphan Asylums.

The wide divergence at 2 to 3 years between the sexes seems

to be a real thing, when one compares children measured in the same group by the same person Niggh-Hürlimann ('30, p. 86) finds, as I do, the same descending slope somewhere between $4\frac{1}{2}$ and 6 years. This is a period when length and width of the head are increasing, though slowly. Apparently there is in the Nordic head a striking change in form at between 3 and 5 years. The head height is always less in the female than the male, but the 2 sexes approach quite closely at about 10 years in connection with the female spurt of growth.

The form of the mass curve from 6 months post-natal to 5 or 7 years is extraordinary and I was for some time in doubt as to its validity. The mass curve of male babies undergoes an inflection at about 1 year and then a second inflexion at 20 months. It reaches a crest of 126 mm. at 30 months and thereafter slopes downward to about 7 years. It thereafter rises slowly in the BOA series to a mean of 127.5 mm. at 15 years

Few series of mean change of head height from birth to 6 years are available. Friedenthal ('14, p. 115) gives a table of increases in Schadelhöhe as measured by him on "kleingemessene Material." This shows a growth from birth to 12 months of from 80 to 95 mm., on the average, as contrasted with our means of from 80 to 112.5 mm. Pfitzner's ('99, p. 366) table of mean head height from birth to maturity proceeds by too great intervals to be comparable with ours. Niggh-Hürlimann's ('30, p. 148) includes only ages $4\frac{1}{2}$ to $6\frac{1}{2}$. The mean increase from $4\frac{1}{2}$ to 5 years is given as 0.1 mm. for classes III and IV, and there is a mean decrease during that interval of 0.7 mm. from classes I and II, and a still further decrease of 0.2 mm. from 5 to $5\frac{1}{2}$ years. This agrees with our findings, though the mean absolute head height of these Zürich kindergarten children is 2 or 3 mm. less than in ours. In North Germans, Saller ('30 and '31) finds the head height about 4 mm. less in girls than boys. From mass studies Saller, Gutbier, Kohe u. Schiereck ('33, p. 93) conclude that head height diminishes with age; but this result *may* be due to selective mortality.

Social —The total change during juvenility and adolescence is so small that it is hard to distinguish differences in the mean of the 4 groups. From 1 year onward the BOA boys have the highest mean head height, the mongoloids the least. The I series stands slightly above the LVD₁ series. The BOA line increases at the rate of 1.33 mm. p.a., the M line at the rate of 1.0 mm. p.a., the I

line at the rate of 0.0 mm p a. In the most intelligent boys growth in head height persists the longest.

Racial —The relative position and the curves of increase in growth for 3 races were found. Of these the Negro stands highest, the Mediterranean lowest and the U. S. and Nordic occupy an intermediate position, being at 12 years about 122.5, 121 and 118 respectively. Just what this order means is hard to say. X-rays of the head indicate an exceptionally thick skin of the Negro's cranium. Negro skulls are said to be among the lowest, around 119 (Martin, '22, p. 796), while the Chinese skull is high. The Schaffhausen (Switzerland) children, measured by Schwerz ('10, p. 26) reached eventually about the head height of our BOA Nordic boys, but at 6 years reached only 116 mm instead of our 121 mm. Likewise his girls at 6 years reached 114 mm instead of our 119 mm. Probably this difference in speed of growth marks a racial distinction.

Individual —Individual curves of growth of 8 babies, selected because of length of the series, are given in Fig. 16. These have a

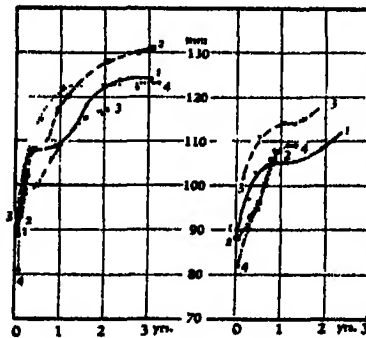


FIG 16 Individual curves of growth of Head Height of eight babies, birth to 3 years, to show flattening of curve at 1 year. Left, males 1, J C No 19, 2, A R No. 38, 3, D R No 26, 4, R F No 33. Right, females 1, A P No 5; 2, C C No 3, 3, F S No 14, 4, B B No 2.

special importance as bearing on the significance of the crest at $2\frac{1}{2}$ years in the mass curve of head height of Fig. 15. One sees the marked reduction of slope at around 1 year. At the right are drawn four typical curves for female babies showing a steep positive slope at between 2 and 3 years.

Figure 4 illustrates the growth of head height in two LVD boys of unlike intelligence. In V.E. the head height took a strong spurt upward during the 11th year and continued to rise to age 17,

attaining 136 mm In E H. the course of development showed little upward trend until the 16th year, when it advanced from 115 to 120 mm. at 17½ years.

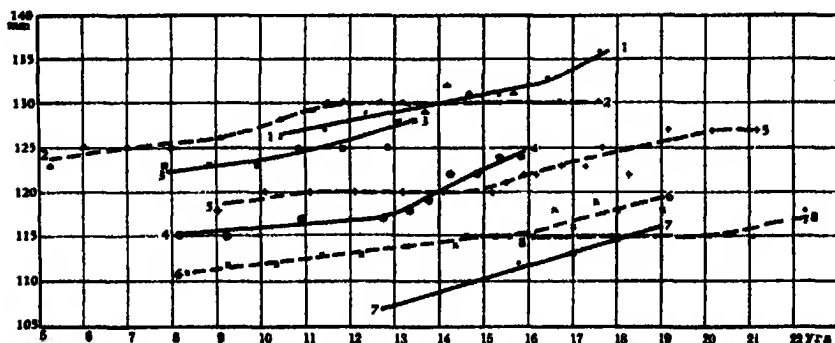


FIG 17 Individual curves of growth of Head Height for various boys of LVD_I and I series 1, WE I No 15, 2, CH LVD_I No 83, 3, JM I No 129, 4, GH LVD_I No 82, 5, CD LVD_I No 30, 6, MH LVD_I No 2, 7, PM I No 10, 8, WW I No 13

In the LVD_I and I series we have some extremes of head height. Thus in Fig 17 one finds at 14 years of age head height ranging from 108 to 130 mm, 126 being about standard for this age. While in cases Nos 1 and 7 the head heights are at 13 years grow-

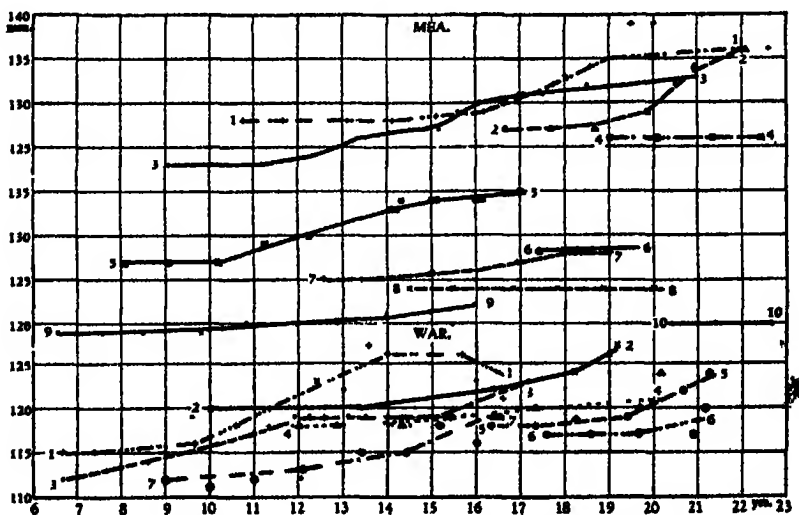


FIG 18 Individual curves of growth of Head Height for Mea (above) and War (below) fraternities Mea 1, WM m; 2, HM m, 3, GM m; 4, LM m, 5, RM f, 6, EM f, 7, NM f; 8, Ma M f; 9, BM f, 10, Mi M f War 1, GW m, 2, Fk m; 3, SW m, 4, WW m, 5, Fd W m, 6, BW f, 7, MW f Three sets of ordinates.

ing at the standard rate of 1 mm p.a., in No 3 the growth is 1.5 mm p.a., and in No 8 growth stopped for 4 years from age 16, which is exceptionally early, and then started up again.

Familial.—Figure 18 gives growth curves of head height for 10 members of the Mea fraternity. It will be observed that 2 boys reach the high point of 136 mm. at 20 years. There are 2 girls (Nos 8 to 10) with a head height of less than 125 mm at 16 years. Three of the fraternity show no change in head height after about 19 years at a time when others are increasing more or less rapidly. The median position of the curves at 16 years is about 127 mm. In the War fraternity all (except No 1) show head height rapidly advancing to the end of the period of observation at 16 to 21 years. The mean position of the curve at 16 years is about 121 mm, strikingly less than in the Mea fraternity. The

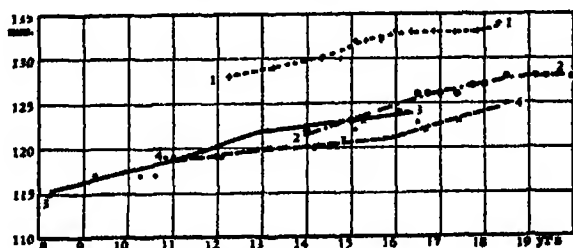


FIG 19 Individual curves of growth of Head Height for Sti. fraternity 1, L S m; 2, C S m, 3, M S f, 4, E S f

curves of head height growth of the Sti. fraternity (Fig 19) all advance steadily to 16–20 years, without obvious acceleration at the adolescent spurt. The mean position of the curves at 16 years is about 126 mm.

Special Cases.—In microcephalics, males, all head diameters are below standard. The distribution of head height of 12 of them is as indicated on Fig 20. No 7a, at 11 years, has a head height that is 101 mm., or 23 mm. below standard, but the rate of increase continues high (0.56) until 19 years, being 1.25 mm. p.a. (standard 1.05 mm. p.a.) Accordingly at 19 years his head height is about 122 mm., or 17 mm. below standard.³

Of the female microcephalics (Fig 21) all the dimensions are below standard. In head height some cases (No. 3) are 30 mm. below standard. For the 3 cases under 21 years of age the percentage departure from standard head height is 15 per cent.

³ Mollison's ('26, p. 111) Mesek at 70 years had an ear height of about 102 mm

The only hydrocephalic I have measured had, at 9 years, a head height of standard size.

Twins.—Figure 22 gives the growth curves for 4 pairs of twins. Pair 1 is of colored girls (Dot) who are not regarded as identical,

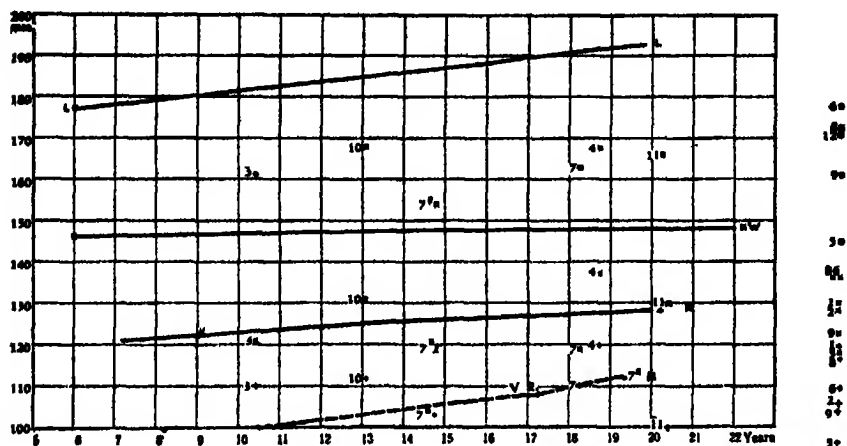


FIG 20 Standard (mean BOA) curves of growth of Head Length (L), Width (W) and Height (H), with growth curve of Head Height of microcephalic, V R (7a). Also, scattered single observations on Head Length, □, Head Width, ×, and Head Height, +, of 11 male microcephalics as follows: 1, A B, 2, G B, 3, R C, 4, G C, 5, L M, 6, F R, 7, F R, 8, P V, 9, I W, 10, F C, 11 M M, all of Letchworth Village, measured at the ages indicated. Those at right margin beyond 22 years of age.

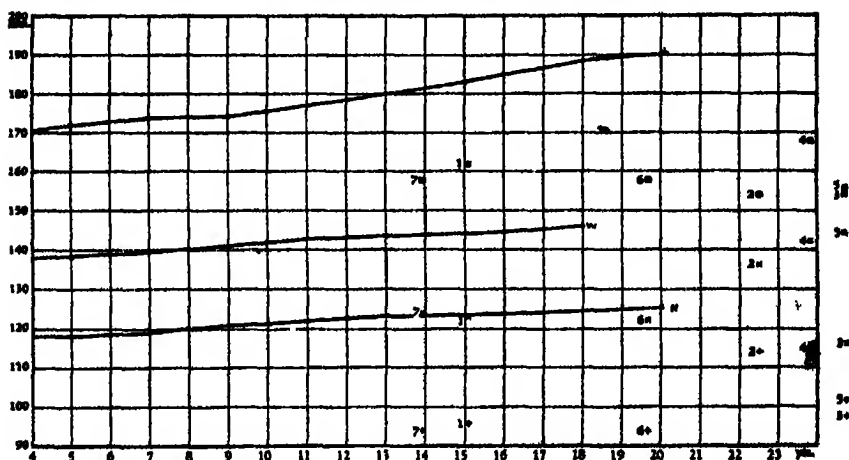


FIG 21 Standard (mean BOA) curves of growth in girls of Head Length (L), Width (W) and Height (H), with extension from Saller ('30), and Gray and Ayres ('31). Also, scattered single observation on Head Length, □; Head Width, ×; and Head Height, +, of 7 female microcephalics, as follows: 1, T C, 2, M C; 3, V C; 4, A L; 5, J M, 6, M R; 7, B S, all of Letchworth Village, measured at the ages indicated. Those at the right margin beyond 24 years of age.

because S is 30 to 20 mm. shorter in stature than E, S has second digit greater than fourth, E has second digit less than fourth. The fingerprint patterns support the view that the Dot twins are dizygotic.

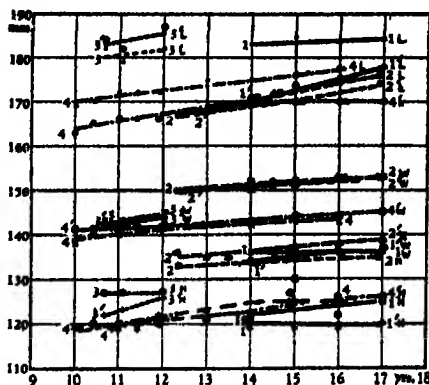


FIG 22 Individual curves of growth in head dimensions (L, W, H) of four pairs of twins 1, S D, 1' E D (colored dizygotic) — 2, G K, 2' M K (monozygotic) — — — 3, D S, 3' M S (doubtfully monozygotic) - - - - 4, A G, 4' M G (monozygotic) — — —

The closeness of the width curves in the case of the Gar. (No 4) twins and Kyr (No 2) twins is striking. These are certainly identical. The Sha. (No. 3) twins are probably monozygotic. Given the same germ plasm, the growth of head (and brain) proceeds along nearly or precisely the same path.

Summary.—The curve of mean head height is not as regular as that of length or even width. Increase of height is much slowed up at around one year of age, a result attributed to gravity. During the second year the height increases rapidly. The head height is less in the female than the male except at about 9 years when the means are nearly the same. Again, children with more stunted development have the smaller head width. Also, the Nordics have the absolutely highest heads. In monozygotic twins head heights are closely alike.

4. Growth of Minimum Frontal Width

General.—There is a dimension in the taking of which considerable errors may occur. We have tried to follow Martin's ('28, p. 182) definition. In Davenport, Steggerda and Drager ('34, p. 274), this measurement was made by the present author with the relatively large error of 2.2 per cent, doubtless due to the

difficulty of giving a uniform contact. In transmuting on-skin measurements to skull-surface measurements subtract, on the average, 13 mm. (cf Torok and László, '02, p 502). In graphing the results, considerable smoothing has been necessary. Averages of this measurement have been determined for children of school age and over by Schwerz (Swiss), Saller (Fehmaraner and Probst); Reuter (Hinterpommerens), Vitali ('02) (Italian), Hoesch-Ernst (Zürichers), Godin, '23 (French boys)

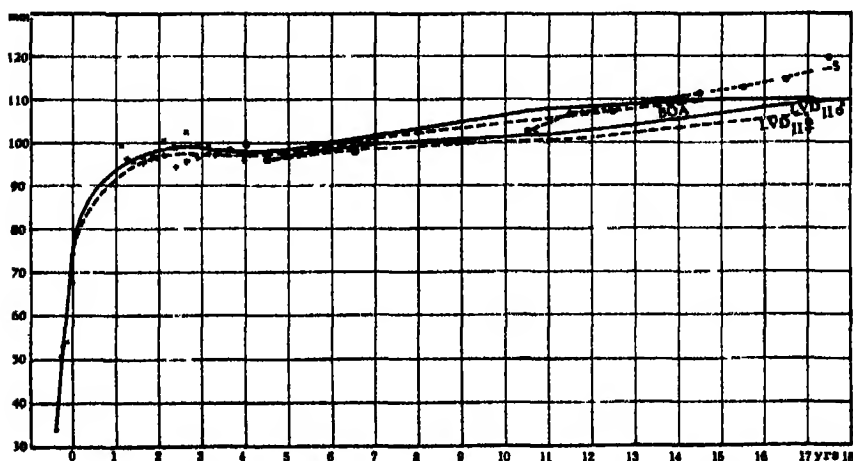


FIG 23 Mass curves of growth of Minimum Frontal Width, males ——— and females - - - - BOA (Standard) and I.V.D.I.I series Also curve (S) of males of Saller's ('28) series for comparison xxx male babies, +++ female babies

Beginning, at 20 weeks from conception (Fig 23), at 34 mm the curve of minimum frontal diameter rises to about 75 mm at birth, or at the rate of 98 mm p.a.

Unfortunately, I have no measurements of this dimension in babies of the first year. The landmarks are not easy to locate in the relatively undeveloped head. These measurements I have of the second year average about 100 mm for boys and 96 for girls.

From 6 years (at 100 mm) onward the minimum frontal diameter increases to 110 mm at 15 years, or at the rate of about 11 mm p.a. or 11 per cent (Fig. 23). It may be noted that in Saller's ('28, p. 75) series the percentage rate of growth of this dimension is the greatest of any of the main cephalic dimensions. In the post adolescent years growth of minimum frontal in his series must exceed that in ours

Sexual—The curve of the mean minimum frontal width of females (BOA) runs very close to, but 1 to 2 mm below, that of males. The two curves depart slightly to 11 years, after which the female curve converges toward the male, probably in connection with the precocious spurt of growth of the female. Great irregularity in the relation of the sex curves at 10–15 years, as figured by Saller ('30, p. 86), must be regarded as of statistical significance only. Taking my data as they come there is evidence that from 2½ years to 4 years the minimum frontal becomes, on the average, no greater, perhaps a millimeter or so less, and then slowly increases to adolescence.

The ~~sex~~ curves of the LVD_{II} group both lie below the curves of the BOA group. The female curve lies below the male by about 1 to 3 mm, diverging to 12 years and then running nearly parallel to the male curve. At 17 years in the LVD_{II} series the female/male ratio is .89. These findings are in confirmation of those of Reuter ('03) for East German children and of Saller ('30, p. 86, 97) for Fehmaraneis.

Social—In general, at age 13, the mean size of the minimum frontal diameter diminishes in order of mean intelligence of the 5 groups measured, except that the LVD_I series has not taken its place before 18 years. The superiority in the mean of this dimension at 8 years of the mongoloids over the LVD_{II} series is remarkable. Not much stress can be laid upon it until confirmed by other investigators. One is reminded that this dimension develops very rapidly in infancy (Fig. 23). In the mongoloids it increases in juvenility rapidly and then grows very slowly.

Racial.—At age 13 the Nordic standard males have the widest mean forehead, 110 mm, the Negroes of the LVD_I and LVD_{II} series come next, 108 mm, 2 to 4 mm wider than that of the Nordics of the LVD_I series. Measurements of adult Yakoma Negroes by Girard (teste Martin, '28, p. 818) gave a mean adult minimum frontal diameter of 108 mm, which is 3 or 4 mm less than our Negro boys. Ours are of course all hybrids.

Individual—As Fig. 24 shows, the range of variation in minimum frontal width is very great. Even among white boys at 16 years the range is from 99 to 118 mm or a total of 19 mm. The slope at 12 to 16 years varies from 0.7 mm. p.a. (No. 4) to 1.7 mm. p.a. (No. 6). A colored boy (No. 1) occupies, as we would expect from the mass curves, a high position with a slope at 16 years of 2.5 mm. p.a.

Among the girls (in upper part of figure) the range of curves drawn is at 13 years from 100 to 106 mm. This range excludes an American Negro girl (No. 1) with exceptionally broad forehead.

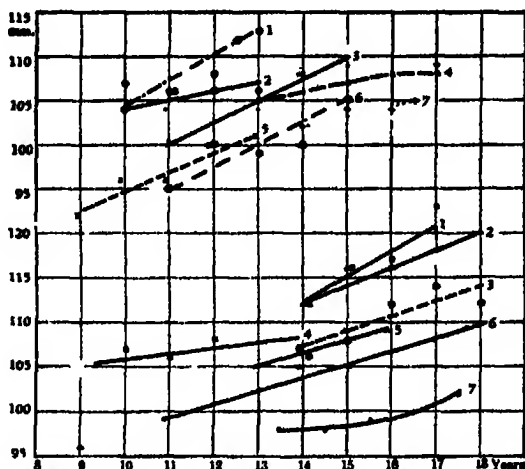


FIG 24 Individual curves of growth of Minimum Frontal Width, LVD males and females, as follows Above, girls, 1, RB col, 2, OD col; 3, MB Ital, 4, CK Scand, 5, CC US, 6, CA US, 7, GH US. Below, boys, 1, R N US; 2, G.W col, 3, CB US, 4, M J US, 5, R C US, 6, R B US; 7, CH US. Two sets of ordinates

The slope of growth for the whites at 11 to 14 years varies from 1 mm p.a. (No. 4) to 2.5 mm. p.a. (No. 3). The Negro girl (No. 1) has a slope of 3 mm. p.a. In all cases, except No. 4 the minimum frontal breadth is still increasing at 16-17 years.

Familial—Figure 25 gives the findings as to minimum frontal width for eight children (4 male and 4 female) of the Mea family of Letchworth Village, also for seven children (5 boys and 2 girls) of the War family, and four children (2 boys and 2 girls) of the Sti. family

The Mea. family is characterized by slow increase (0.5 mm. p.a.) of forehead width until about 17 to 19 years, thereupon follows a more rapid increase, about 1.5 to 4 mm. p.a. No. 8 is an exception.

The War. family is characterized by a change from slow growth (1.0 mm. p.a.) of forehead width to more rapid growth (1.5 to 2.5 mm. p.a.) at about 14 to 16 years.

The Sti. family shows no, or very slight, growth of forehead width; and, indeed, a decrease of this dimension in three of the

children followed by an increase in one. The decrease is of the order of 0.8 to 1.2 mm. p a. In the case of No 3 this decrease continues from 12 to 20 years.

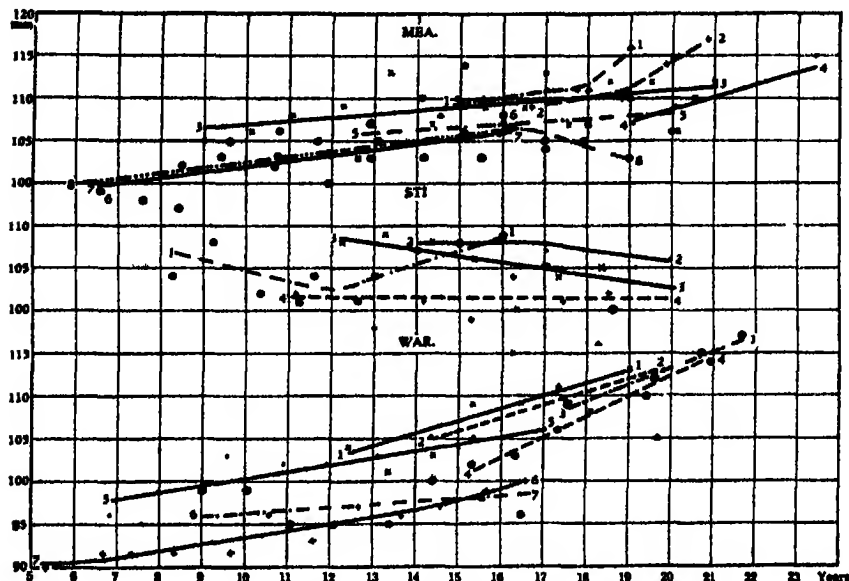


FIG 25 Individual curves of growth in Minimum Frontal Width in three LVD fraternities (high grade) Mea 1, Ma M f, 2, H M m; 3, G M m, 4, L M m, 5, N M f; 6, B M f, 7, R M f, 8, W M m Sti 1, M S f, 2, C S m; 3, L S m, 4, E S f War 1, Fk W. m, 2, W W m, 3, B W f, 4, Fd W m, 5, S W m, 6, G W m, 7, M W f

Thus, while the growth curves do not always coincide (except Mea. family Nos. 5, 7, 8, also War family, Nos. 1, 2, 3, 4) they show family resemblances.

Twins.—Figure 26, middle, gives the course of growth of the minimum frontal width for three pairs of certainly and one (She) of doubtfully monozygotic twins. In the case of the Gar. and Kyr. twins the growth curves run in all four children close together. In the case of the Sha. and She. twins there is a difference of 3 or 4 mm.—somewhat greater than the error of measurement which is, in my case, about 1.9 mm. On the whole, the development of the front part of the brain case proceeds, in twins, in very similar fashion.

Special Cases.—A growth curve of the forehead width of a microcephalic lies at the bottom of Fig. 26. Before adolescence its slope is about 0.8 mm. p.a., after adolescence about 1.5 mm. p.a. It reaches 95 mm. at 19 years. Mollison ('26, p. 112) found 87

mm. in the microcephalic Mesek. The dwarf, No. 5, from 12 to 18 years increased 4 mm., or 0.67 mm. p a. During the period 12 to 16 years the Gar. twins, on the other hand, increased nearly 9

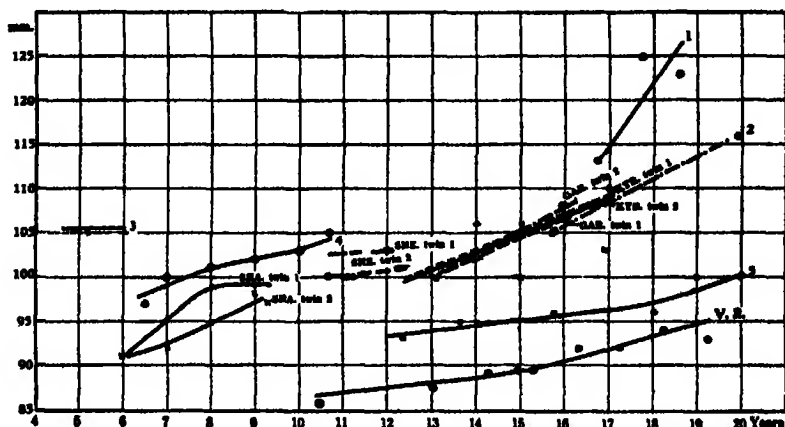


FIG 26 Individual curves of growth in Minimum Frontal Width in twins and special cases, LVI) males and females 1, C B f, cretin, 2, M C f, achondroplastic, 3, H L m, cretin, 4, A A f, cretin, 5, A W f, ateliotic Gar monozygotic twins, f; Kyr monozygotic twins, m, Sha monozygotic twins, m, She probably dizygotic twins f V R microcephalic

mm., or 2.25 mm. p a., while the cretin, No. 1, undergoing some thyroid treatment from 16.9 to 18.6, grew at the rate of 7.4 mm. p a. In No. 3 on the other hand, a cretin not undergoing treatment, the forehead increased from $4\frac{1}{2}$ to 6 years not at all. Finally, No. 2, achondroplastic, grew from 15.9 to 20 years 11 mm., or about 2.6 mm. p a. The head dimensions of this achondroplastic girl grew at more than an average rate.

Summary.—The minimum frontal diameter rises rapidly to nearly its final size at 1 year. A reduction, on the average, appears at 3 to 4 years. Thereafter the rise is slow. The male/female ratio is 0.89. The groups with lower grades of intelligence have the lower position and less steep slopes. The frontal width generally continues to increase, at least up to 18 years. In different families the rate of increase varies greatly. In twins the frontal width develops in similar fashion. In cretins the dimension is sometimes large, in microcephalics small.

5. Growth of Bizygomatic Width

General.—This is the maximum distance between the skin surfaces of the bizygomatic arches. In making my measurements

I followed the directions of Martin ('28, p 183, No. 7). To the zygomatic arches are attached the masseter muscle used in chewing and the zygomaticus which draws back and up to the corners of the mouth, as in the snarling dog

From a distance of 31 mm. at 20 weeks of gestation this dimension increases rapidly to 87 mm at birth, being at the rate of 140 mm p.a. (Fig 27). During the first post-natal year the rate

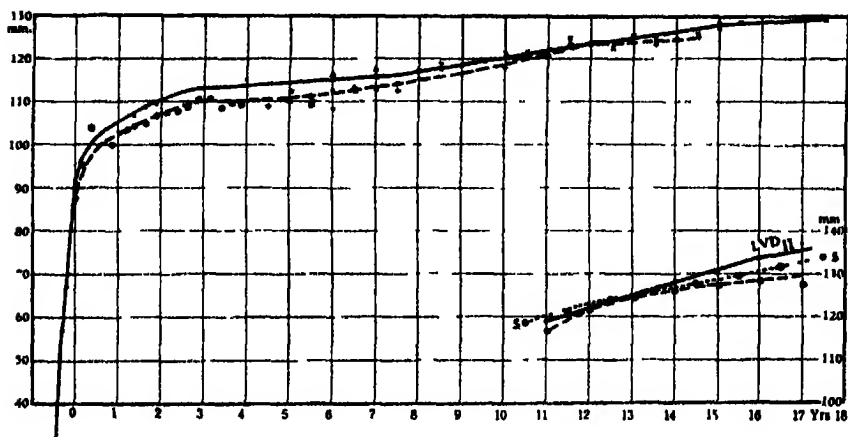


FIG 27 Mass curves of growth of Bizygomatic Width, standard U S white males and females Above, as in Fig 1 Below, LVD₁₁ males, unbroken line, and females, broken line, also data from Saller ('28), S, with scale of ordinates to right

falls from 30 to 18, 12 and 6 mm p a , during the second year to 5 mm., then to 0.8 mm p.a. after 3 years. During school years the growth of this dimension is moderately slow, increasing from 6 to 16 years about 15 mm , or 1.5 mm. p a This is about the same rate as West obtained from the Worcester series, but a little more than Boas finds (14 mm p.a.) for white U.S. males, or Schwerz (13 mm. p.a) for Swiss children In the case of North American Indians Boas ('95), starting at 6 years, finds a rate of 1.7 beginning with a breadth of 123 mm instead of our 113 mm

Sexual —The bizygomatic width in the females, like the other dimensions of the skull, is the less in the smaller sex. The sexual difference is just detectable at birth, increases slightly to about 4 mm at the beginning of the sixth year in the girl and, thereafter, appears to approach zero on the average at 12 years With the adolescent spurt of growth in the male the difference between the sexes is increased slightly (Fig. 27).

For Fehmaraners, Saller ('30, p. 96) finds bizygomatic width of the female about 2 mm. less than of the male to 15 years and thereafter 3 to 8 mm. less. Ernst ('06, p. 127) finds the female face breadth about 2 mm. less than that of male, except at 13-15 years, when it is equal or more. Schwerz ('10, p. 32) likewise finds the breadth in girls less than in boys, except at ages 15-17. However, in Porter's St. Louis series and West's Worcester series the mean bizygomatic width of the girls never exceeds that of the boys.

Social—The more defective groups are all (excepting the LVD_{II} group) below the standard and at 16 years fall below in the order of their average intelligence. In the standard group, mean bizygomatic width, which is 112 mm. at 6 years, increases up to 12 years at the rate of 4 mm. p. a., from 12 years to 16 years it increases at the rate of 2.6 mm. p. a. The LVD_{II} group increases at the rate of about 3 mm. p. a. The mongoloids increase at about 3.5 mm. p. a. from 10 to 14 years (when mean width is 119 mm.) and at about 1 mm. p. a. from 16 to 22 years.

In general, the bizygomatic width is associated with stature. Whatever controls growth of the long bones affects, on the average, the width of the face and its bizygomatic arches.

Racial.—Of the three main groups the Negro has the widest face in childhood, 127 mm. at 12 years, attaining 137 mm. at 17 years. The slope of growth is rapid before adolescence, averaging 2.0 mm. p. a. between 6 and 11 years, then becoming slower to 1.0 mm. p. a. between 14 and 17 years. The Nordic children at 12 years have a bizygomatic width of 123 mm. Between 6 to 10 years the slope of growth averages 1.25 mm. p. a., between 11 and 13 years, 2.75 mm. p. a., 16 to 18 years 1.75 mm. p. a. The curve attains 137 mm. at 20 years. The Mediterranean group runs nearly parallel to the Nordic group, has at 12 years a width of about 122 mm., or 1.0 less than standard. After 14 years the rate of growth slows up to 1.25 mm. p. a. and attains 131.5 at 17 years. Goldstein ('39), however, finds a bizygomatic width for his normal Jewish boys that is above our standard.

For various Nordic peoples as listed by Saller ('30, p. 97) the bizygomatic width runs in the male from 136 to 143 mm. at 20-24 years, whereas our Nordics at 20 years reach only 137 mm. Amerindian boys at 12 years have a zygomatic width of about 132 mm.; at 20 years 145 mm. (Boas, '95). An Eskimo from Smith

Sound, measured by Hrdlička ('10, p 228) had a bizygomatic width of 148 mm. This is the race that has the widest bizygomatic diameter, ascribed by some to the eating of coarse food.

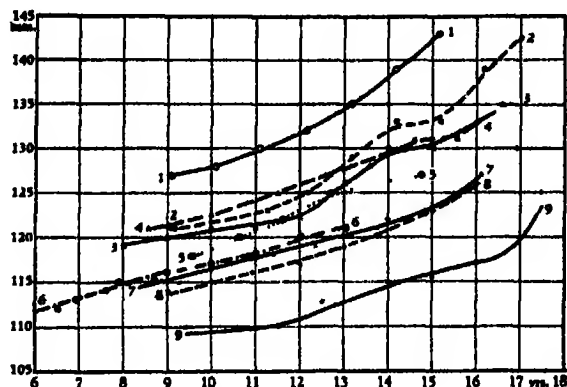


FIG 28 Individual curves of Bizygomatic Width in U S (except as otherwise stated) males of LVD₁ series, as follows 1, CH No 26, col , 2, WS No 16, 3, AM No 7, 4, JC No 10, 5, JB No 54, 6, JG No 114, Ital , 7, MH No 2, 8, GP No 5, Ital , 9, RH No 29

Individual—Figure 28 shows the curves of growth of bizygomatic width in 9 boys of the LVD₁ series. The range is seen to be great, at 14 years from 114.5 to 138 mm. This variation of bizygomatic width is loosely correlated with general body size. Two curves show the influence of the adolescent spurt of growth (at 13 years). Other curves seem little affected thereby. In general transverse dimensions are less affected by that episode than median ones.

On Fig 29 are given curves of growth in bizygomatic width of four LVD₁ boys and 3 BOA girls. It will be noted that No. 7, whose bizygomatic width is small, but whose stature at 17 years is 162.5 cm., shows no spurt of bizygomatic increase, while No. 2, whose bizygomatic width is large and who at 17 years was 166.0 cm. tall shows a marked increase of gradient at 11.3 years. At 17 years No. 2 is broad faced at 142 mm., while No. 7 remains narrow faced, at 120 mm., or has a face of the width of No. 2's at 8 years.

No. 1 is a colored boy, his face width is markedly greater even than No. 2's, although his stature at 16 years is about 158 cm. As we have seen, the Negro face width is much greater than the whites', certainly before 16 years of age. Individual curves of

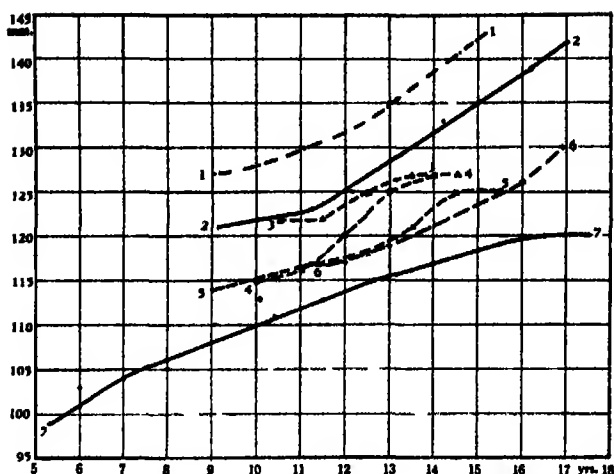


FIG 29 Individual curves of growth of Bizygomatic Width, as follows 1, C' H LVD_I No 26, col, m, 2, WS D_I No 16, US m, 3, BOA No 91 f, 4, BOA No 73 f, 5, BOA No 51 f, 6, G P D_I No 45, Ital m, 7, C' H D_I No 83, US m

three BOA girls are given in the middle of the chart (Nos 3, 4, 5). One notes that in all cases the spurt begins early, at from 11 to 12.5 years. The spurts of the white boys are at 11.3, 12.2 and 13 years, respectively, or 3 to 6 months later.

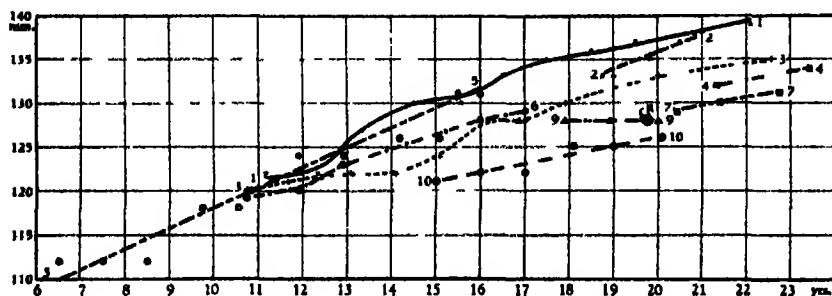


FIG 30 Individual curves of growth of Bizygomatic Width in *Mea fraterut* as follows 1, G M m, 2, H M m, 3, W M m, 4, L M m, 5, B M f, 6, R M f, 7, M i M f, 8, E M f, 9, M a M f, 10, N M f

Familial.—Figure 30 gives curves of growth of bizygomatic width in the case of the *Mea* family of Letchworth Village. In the case of the four boys (Nos 1-4), in as far as they have been measured during comparable periods, the curves run parallel, but 3 to 5 mm apart. In the case of the girls, two have curves that are for the most part not more than 3 millimeters apart

The other three are grouped, at 19-20 years, about 126-128 mm. It is certainly remarkable that two sisters, Nos 5 and 6 measured repeatedly during 5 years should during most of that time have approximately the same width between cheek bones, while in other families the range at 15 years is for white girls from 122 to 132. The efficiency of genetic factors (environmental being constant) in determining the width of the middle head, including the zygomatic arches, is indeed striking.

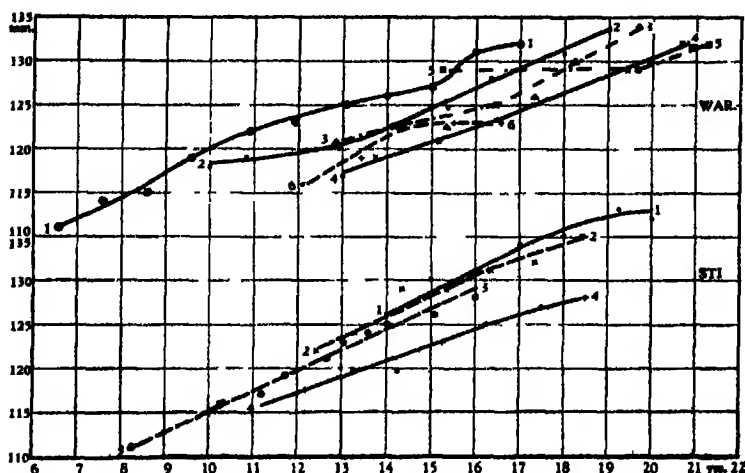


FIG 31 Individual curves of growth of Bizygomatic Width in War and St1 fraternities War 1, S W m, 2, F W m, 3, W W m, 4, Fd W m, 5, B W f, 6, M W f St1 1, C S m, 2, L S m, 3, M S f, 4, E S f Two sets of ordinates

In Fig 31, at the top, are growth curves of bizygomatic width for 6 members of the War fraternity. The mean advance is about 2 mm. p a. No 1, m, shows a marked adolescent spurt at 15 years, and No 3 at 16 years.

At the bottom of the chart are curves of 4 members of the St1. family, 1 and 2 are brothers, whose growth curves coincide. 3 and 4 are curves of sisters which are 2 to 3 mm. apart and diverge slightly with age.

Twins.—In Fig 32 are shown, near the bottom, the curves of growth of bizygomatic width for 4 pairs of twins, all monozygotic except the She pair. The monozygotic pairs run identical courses. Curves are shown (above) of two cretins (Nos 1 and 3), an achondroplastic (No. 2) and an atelotic dwarf (No 4). V R. is the microcephalic.

Summary.—Bizygomatic width increases very rapidly during pre-natal development, slows up temporarily at 4 to 5 years; thereafter proceeds with increased gradient. It is less in girls than

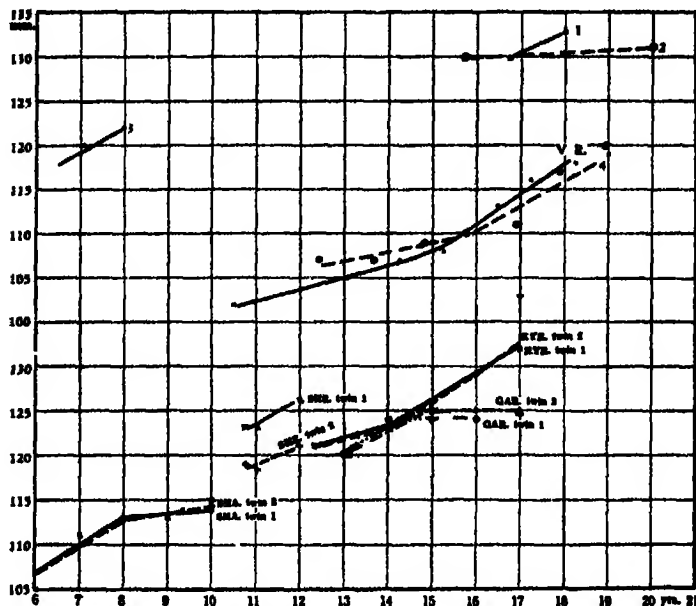


FIG 32 Individual curves of growth of Bizygomatic Width in twins and special cases 1, C B f, cretin, 2, M C f, achondroplastic, 3, A A f, cretin, 4, A W f, ateliotic dwarf, V R m, microcephalic Monozygotic twins Gar f, Kyr m, Sha m Dizygotic twins, She f Two sets of ordinates

boys except at 12 years. The curves of the least developed types, physically and mentally, lie usually below that of the best developed. The curve of the Nordics occupies an intermediate position. The great size of the dimension in Eskimo is believed to be due to the eating of coarse food; and food habits may cause the larger bizygomatic width in different groups of this country. In various individuals of one fraternity curves of bizygomatic width run roughly parallel courses, they are frequently influenced by the general growth-promoting hormones. Monozygotic twins have similar curves, the genetical factors are strong, although their action is modifiable by environment.

6. Growth of Bigonial Width

General—This dimension is the horizontal distance between the gonions, defined as that point of the lower jaw which lies

undermost, hindmost and outermost The skin and connective tissue and muscles are sparse at this point, so the bony distance is probably not more than 10 mm less, though Wilder and Wentworth ('18, p. 102) recommend subtracting 21 mm. Probably this recommendation assumes that the on-skin measurement is made over the masseter muscle.

Since the angle of the lower jaw is used for the attachment of muscles of mastication it tends to become more prominent with age.

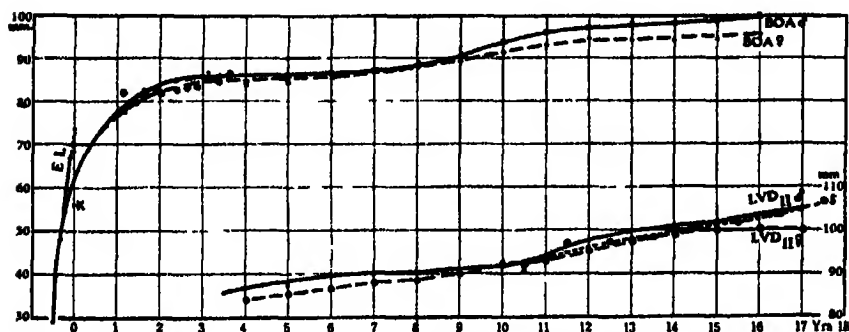


FIG. 33. Mass curves of Bigonial Width, standard (BOA) series, also LVD_{II} male and female, with series (S) from Saller ('28), using ordinate scale to right K, Kugler E L, Embryological Laboratory

Figure 33 gives curves of mass growth of bigonial width from the mid-gestational period to maturity. The pre-natal means run from 29.9 at fourth lunar month to 73.3 at birth (tenth lunar month) But Kugler ('32) finds a mean bigonial width at birth on live babies of 56 mm. Since the fetuses in formalin seemed swollen and rigid the mean that I found at term is certainly too high I have run a hypothetic curve through the 62 mm. point at birth to combine it with the point of babies at 12 months post-natal. The rise is moderately swift to 3 years, thence slow to 7 or 8 years, and thereafter more rapid to 12 years. After this the speed of growth slows up to maturity in the BOA series, though growth still increases in the LVD_{II} series to 17 years or later. The low gradient of the curves from 3 to 5 years is rather exceptional among mass curves of growth of dimensions

Sexual.—In the Babies and BOA series the female bigonial width is less than that of the male, although the curves of the two sexes approach closely at 7 to 8 years (Fig. 33). In the LVD_{II} series male and female curves come in contact at 10 years. The male dimensions are otherwise greater than those of the female of

that series. The excess of bigonial width in the male at 16 years is about 5 per cent, which is less than the excess of stature at that age (about 7.5 per cent).

Social.—The mean growth curve of bigonial width for 3 groups of varied intelligence is greatest in the BOA (standard) series, from 10 to 14 years, reaching 99 mm. After this period the LVD_I series shows a slightly greater jaw width. This width is much the lowest in the mongoloid series—about 5 to 8 mm less than in the BOA series. This is roughly proportional to the small stature of the mongoloids.

Racial.—Taking Nordic as a basis of reference, the Italian and Negro groups seem to have a broader jaw at an early age, 8 to 10 years, and narrower after 14 years. The angle of the jaw develops in them precociously. The Jewish group, on the other hand, has a small jaw width at 9 years and this becomes 1 or 2 mm. greater than the Nordic at 19 years.

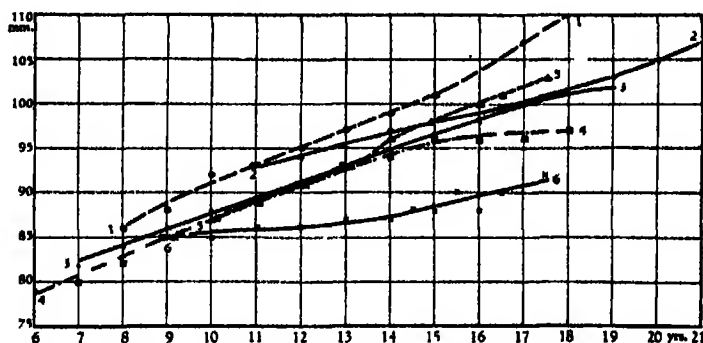


FIG 34 Individual curves of Bigonial Width in LVD_I males, U S and Nordic 1, J B No 54, 2, C D No 30, 3, E H No 1, 4, R O No 110, 5, R G No 84; 6, C H No 83

Individual.—Figure 34 shows curves of growth of 6 boys belonging to the LVD_I series. No. 3 is nearly straight up to 17 years, concave upward are Nos. 2 and 6. No. 4 is concave below and Nos. 1 and 5 are wavy. Where a change of slope upward occurs it is usually at about 14 years of age—the age of general rapid growth, but in No. 4 growth begins to slow up at about this age. In general, as Goldstein ('36, p. 86) points out, mean facial widths do not show adolescent acceleration, but individual widths seem frequently to do so.

The slope of No. 3 is at first 1.5 mm. p.a., it then slows down after 17 years to 1.0 mm p.a. No. 4, which at first has a slope

of 1.7 mm. p a , slows down after 15 years to 0.5 mm p a No 1 is still increasing in bigonial width at 17 years at the speed of 3 mm. p.a

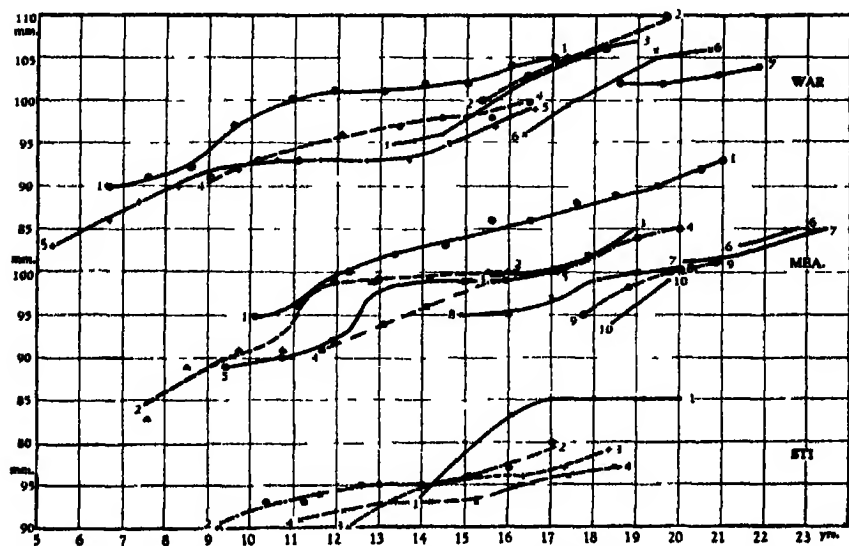


FIG 35 Individual curves of growth of Bigonial Width in members of three fraternities War 1, S W m, 2, W W m, 3, Fk W m, 4, M W f, 5, G W m, 6, Fd W m, 7, B W f Mea 1, G M m, 2, B M f, 3, Ma M f, 4, W M m, 5, R M f, 6, M i M f, 7, L M m, 8, N M f, 9, H M m, 10, E M f Sti 1, C S m, 2, M S f, 3, L S m, 4, E M f Three sets of ordinates

Familial.—Figure 35 shows growth curves of representatives of 3 fraternities at Letchworth Village. In the War fraternity the curves all advance with upward slope, averaging about 1.25 mm. p a. They are centered, at 14 years, at about 97 mm. The more complete curves are all sigmoid. In the Sti fraternity the curves are centered at 14 years at about 94 mm. Three run close together but the fourth, that of a boy, advances rapidly (5 mm p.a.) from 14 to 16 years and ceases to grow after 17 years, at an age when the other curves are sloping upward.

In the Mea fraternity all long curves slope upward with varying degrees of sigmoidity. No 1, that of a boy, has for 9 years a growth curve of bigonial diameter that is nearly straight at 1.4 mm. p.a. No 4, of a boy, is also only slightly sinuous. Nos. 2, 5 and 8 are of sisters all of whom show a brief period of rapid growth at 11, 12 and 17 years respectively. The curves at 14 years are centered at 99 mm.

Thus while there is quite a bit of variation in position of the curves in any one family the mean positions at 14 years are significantly different. The large mean bigonial diameter of the Mea. family is associated with their partly Indian stock. For the Amerindians, like the Chinese to which they are kin, have broad lower jaws.

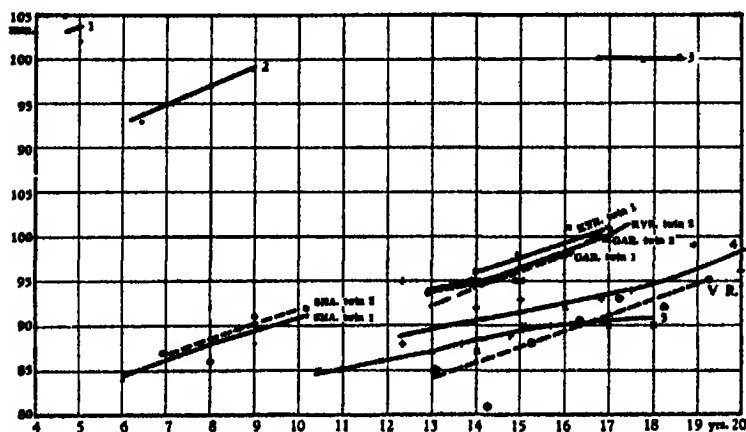


FIG 36 Individual curves of growth of Bigonial Width in twins and special cases 1, H L m, cretin, 2, A A f, cretin, 3, C B f, cretin; 4, M W f, ateliotic; 5, K S f achondroplastic V R m, microcephalic Monozygotic twins Gar f, Kyr m, Sha m Two sets of ordinates

Twins and Special Cases —Figure 36 gives the growth curves of the bigonial width in the Gar, Kyr and Sha. twins. The developmental curves of each pair practically coincide. Curiously enough the curves of the Kyr. boys are also just like those of the Gar. girls

The ateliotic dwarf, No 4, and the achondroplastic No. 5 have small bigonials, corresponding with their dwarf size. The microcephalic, though much larger than the dwarfish girls, had at 14 years a smaller bigonium. The bigonial is large in the three cretins (Nos. 1, 2, 3).

Summary —The bigonial width increases less rapidly, prenatally, than does the bizygomatic width; during juvenility growth is slow, there is an adolescent spurt in growth. The curve of the female lies in general below that of the male except that the two coincide at 8 or 10 years. There is not much difference between the social and racial groups except for the narrowed jaw of the mongoloid dwarfs. In families the growth curves of indi-

viduals are not coincident but the means of different families are slightly different. The bigonial width is alike at all ages in monozygotic twins. It is large in cretins.

7. Growth of Distance from Occiput to Tragon

General.—The large post-auricular portion of the cranium is one of the most characteristic dimensions of man's head as the reduction of the visceral part of the skull is another. In the human embryo of 85 mm length the auditory plate and acoustic ganglion are placed on the sides of the newly infolded neural tube, about two-thirds of the distance from the mid-dorsal line at future occiput to the optic vesicle. Starting from this stage which is common to all Primates, it is of interest to see how in humans this section of the head grows

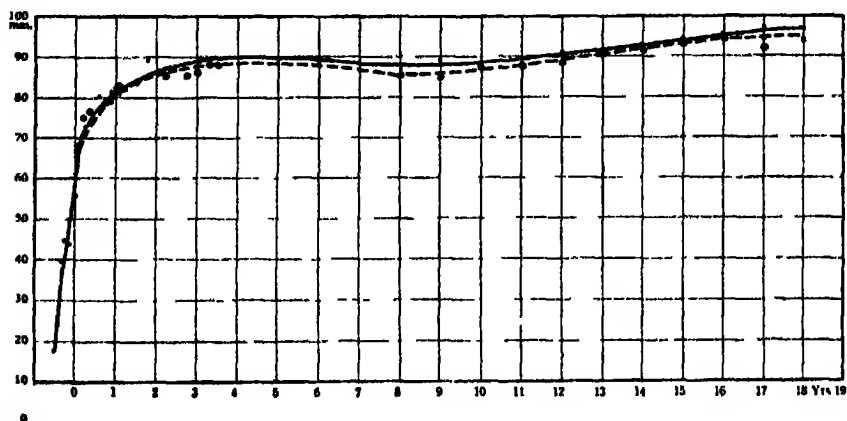


FIG 37 Mass curves of growth of Post Auricular Cranium from fetus to maturity Standard series, unbroken line males, broken line females

Figure 37 reveals the changes in horizontal length of the post-auricular region from a fetus of the fourth month to the adult—from a post-auricular distance of 18 mm to one of 97 mm. During less than 6 months to birth the post-auricular part of the cranium increases 40 mm. or at the rate of about 75 mm p.a. During the first post-natal year the average increase is, in the male, from 59 to 81.7 mm, or at the rate of 22 mm p.a. This year is characterized by a marked bulging of the occipital bone. In the next year the increase falls to 5, and in the third year to 2.5 mm. These additions are only about 10 per cent less than the additions to

total head length, indicating that the post-auricular part of the head is growing with exceptional speed during this first year ⁴

After a rapid growth to the end of the first post-natal year growth slows up greatly and, after 4 years (apparently, from our mean data), absolutely diminishes a millimeter or two until about the 8th year. This would seem improbable had we not other evidence of the changes in head form during childhood. This matter is considered in the next section and in that on cranial proportions (page 94). Holl ('88) and Neumayer ('08) found that the post-auricular fraction of head length shortened itself in Alpine peoples during the growth period and Kenter ('33, p 347) confirmed this with long-headed Schleswig-Holstein children, where the mean post-auricular portion was 46.7 mm at 6-7 years, 45 at 14-15, 43.6 at 25-60 years in males. A similar decrease occurred in females.

After 8 years our curve rises about 0.8 mm p.a. to 92 mm at 13 years, thence more rapidly (1.4 mm p.a.) to 16 years during the adolescent spurt. Thence the rate of growth diminishes again (to 0.8 p.a.) to 18 years. The post-auricular portion of head length attains at 4 years over 90 per cent of its adult size.

Sexual—From birth on, the female post-auricular length is less than in the male, corresponding to the smaller head length (Fig 1). From 4 to 10 years the mean difference is about 1.5 mm. After that the mean difference decreases to about 1 mm at 18 years. In the LVD_{II} series the female's distance perhaps comes to exceed the male's by a millimeter at about 13 to 17 years. But this difference is in part due to sampling, since all the individuals used in getting the 11 year mean are not the same as were used in getting the 15 year mean.

Social—Not sufficient data were obtained on this topic to warrant discussion.

Racial.—As compared with Nordics, the Italians and Jews have the shorter mean post-auricular cranium—the Italians shorter (about 2 mm.) than the Jews. The Negro group has, from 7 to 12 years, the largest post-auricular cranium of all (about 90 mm.), but after 12 years in our series this dimension is smaller than in Nordics, despite the greater mean head length in the Negro. All curves, except perhaps the Jewish, show the reduction in post-auricular cranium at 8 to 9 years.

⁴ For an account of methods used in measuring occiput to tragon see page 93

Individual.—A special study of 11 children measured yearly between ages 6 and 8 or older reveal minima in horizontal tragon as follows (minima in two adjacent years being counted as two cases) at 6 years, 4 cases, at 7 years, 5 cases, at 8 years, 4 cases; at 9 years, 1 case. It will be seen that the minimum in the mass curves lies at the ages of the individual minima. Did these minima fall merely by chance we should expect most of them at the earliest years, *i.e.*, 6 years, on the assumption of a continuously rising growth curve.

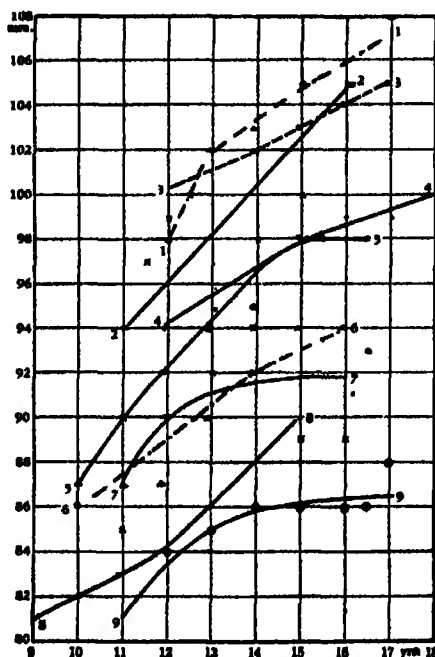


FIG 38 Individual curves of growth of Post Auricular Cranium, LVD₁₁ series, female 1, H O No 80, 2, C K No 58, 3, A G No 48, 4, B A No 3, 5, A G No 44, 6, M G No 45, 7, N B No 11, 8, M B No 8, 9, G H No 52

Figure 38 shows individual growth curves for 9 girls, between ages of 9 and 18 years. In general, the increase of the post-aural cranium is fairly regular. In general, growth is most rapid before 13 years in these girls and slows up thereafter. In No 5 and No 7 (perhaps) growth ceases at 15 years.

Figure 39 gives growth curves of post-auricular cranium for 6 boys. The inaccuracies in measurement appear large on this large scale where 1 mm. is represented by 3.0 mm on the paper.

Accordingly there is a considerable dispersion of points of observation. In one case there is a steady advance in size from 12 to 18 years. But in Nos 2 and 4 there is an apparent decrease in this

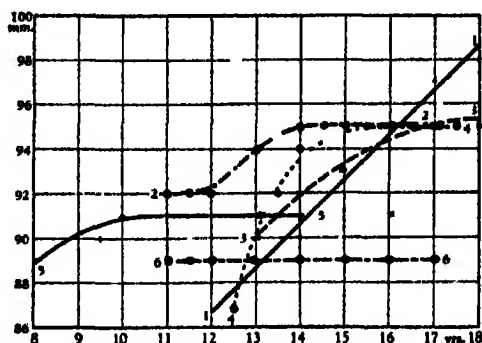


FIG 39 Individual curves of growth of Post Auricular Cranium, LVD₁₁ series, male 1, CB No 10, 2, GR No 88, 3, FC No 28, 4, IG No 49, 5, WF No 41, 6, AS No 99

dimension and in No. 6 there is apparently no growth, upon which however not too much stress is to be laid because of the above mentioned inaccuracies. Still the findings are worth recording, the more so as the head ordinarily changes shape at or about the period of the adolescent spurt

Familial —Figure 40 shows curves of growth of the Kra, Mea and Sti. fraternities. The Kra family (1, 2, 2a and 3) is rather fragmentarily presented. 2 and 2a are dizygotic (dissimilarly sexed) twins, but nevertheless they grew similarly.

3', 4, 5, 6 are members of the Mea family. The first is a girl, the other three her brothers. All are advancing or apparently have advanced at high speed into dimensions of over 100 mm. In the case of No. 6 there has been a failure to enlarge after 16 years, and possibly a reduction of the dimension in later years. The girl's (3') curve has been introduced to give some idea of probable early growth of post-auricular diameter in this family.

No 7 and No 8 are growth curves of this dimension for two Sti boys. They reach the same high point (106/107) at 18 years, though by apparently different courses.

Nos. 9–11 are growth curves of post-auricular dimension in 3 boys of the War. family. The curves all lie at a low value, 90 to 96 at 19 years. The curves do not coincide but run parallel courses.

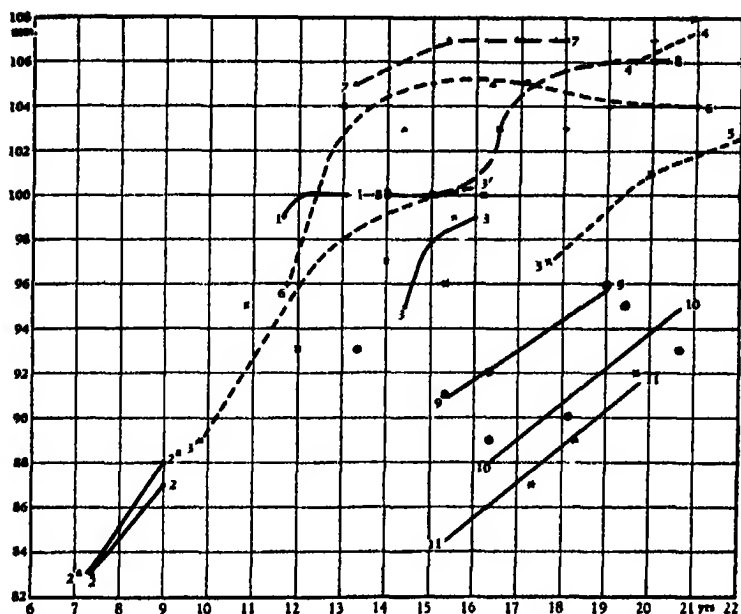


FIG 40 Individual curves of growth of Post Auricular Cranium Representatives of fraternities Kra fraternity 1, D K m, 2, J K m, 2a, M K f, fraternal twin to 2, 3, R K m Mea fraternity 3', B M f, 4, G M m, 5, H M m, 6, W M m Sti fraternity 7, L S m, 8, C S m War fraternity 9, Fk W m, 10, Fd W m, 11, W M m

Twins and Special Cases -- Figure 41 shows the curves of growth of post-auricular depth for the Gar, Kyr, Ols. and Sha twins. In all cases the twin curves run close together from 2 or 3 mm. apart, except in the Gar twins where there is a divergence from

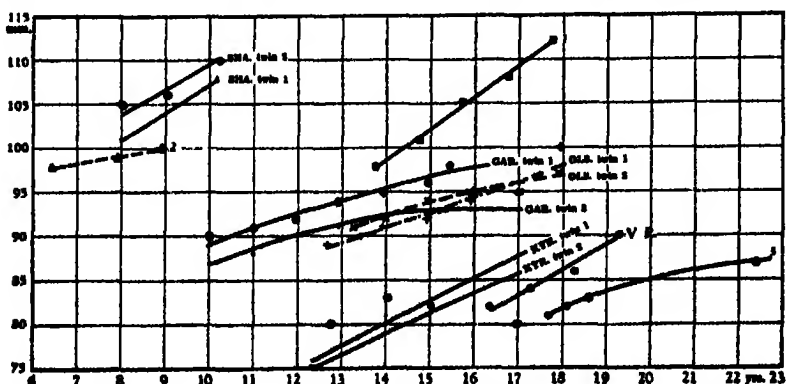


FIG 41 Individual curves of growth of Post Auricular Cranium in monozygotic twins and special cases 1, C B f, cretin, 2, A A f, cretin, 3, A W f, ateliotic dwarf V R. m, microcephalic Female twins Gar 1 and 2 Male twins, Kyr, Ols, Sha

15 years on, to 5 mm, but this may be partly due to limitations of measurement. The low position of the Kyr twins (at 75 to 87) is striking. Of the dwarfs, No 3 is located at the bottom of the chart with a post-auricular depth at 18 years of 82 mm., while the cretin dwarf No 1 stands at the same age, at 113 mm. Cretinous also is No 2. In a hydrocephalic male at Letchworth Village, Ralph W., a post-auricular depth of 115 mm was found at 9 years. In these last cases the great post-auricular depth is associated with the notoriously large head of these types.

8 *The Migration of the External Auditory Meatus*

Both in the study of head height and in the horizontal distance from occiput to porion the porion (at external auditory meatus) is taken as a point of reference. As such it is tacitly assumed to be fixed.

This assumption, as Klaatsch ('08) and Schultz ('17, p. 4) have pointed out, is not justified. The porion, at least between ages 10 to 16, generally shifts its position with reference to the sella turcica. No doubt the sella turcica migrates also, but since the sphenoid bone, of which it is a part, is in turn a part of the chondrocranium which forms the base of the brain and since the sphenoid is relatively fixed between the occipital bone posteriorly and all the other cranial bones laterally and anteriorly, the movements of the sella turcica with reference to other parts of the cranium must be relatively slight.

To measure the direction and magnitude of the movements of the porion I have had available a series of roentgenograms taken laterally by Miss Olive Renfroe of Letchworth Village. She has had great experience in taking such radiograms and the comparison of the radiograms of the same child taken in successive years reveals that the exceedingly difficult task of orienting the head has been, for the most part, especially in later years, satisfactorily done. From tracings of such radiograms I have been able to get the direction of change of the porion from the first position in subsequent years.⁵ The results are shown in Table 4 which gives the frequency of occurrence of movements of subsequent positions of the porion from a first position (face to right) toward the north or vertically directed quadrant, 4 cases, toward the east or anteriorly directed quadrant, 4 cases; to the south or basally directed

⁵ Further details will appear in a subsequent publication to appear in the *American Journal of Roentgenology and Radium Therapy*

quadrant, 8 cases, to the west or occipitally directed quadrant, 18 cases. Accordingly, the prevailing direction of movement of the porion during this period is occipitally and, to a less extent, occipit-basally. This confirms Neumayer's ('08) conclusion, based on comparison of skulls, that in juveniles the porion lies relatively further forward than in adults.

Such a movement, it may be added, would tend to increase the head height measurements, taken from porion. Nevertheless, the total head height as seen in Roentgenograms often diminishes during the adolescent period. It must not be overlooked that the parietal bone (which impinges on the temporal) undergoes change of shape during development as Todd and Tracy ('30, p. 90) point out.

TABLE 4, showing direction and amount of migration of porion from the first position observed in children in early teens, sexes combined. The face is supposed to be facing the east (right)

Quadrant	N	E	S	W
		Anterior		Posterior
No. of cases	4	4	8	18
Average movement in millimeters, p a	19.9	5.4	8.6	17.8

Summary—The distance from occiput to tragon is subject to marked changes during development corresponding to changes in the dimensions of the brain. There is rapid increase to the end of the first post-natal year, when the mean of this dimension has attained 82 per cent of the mean adult size. There is a cessation of growth during juvenility; a slow growth subsequently. The cessation of growth of the post-auricular portion of head length is in part due to a backward migration of the external auditory meatus.

9. Growth of Distance from Occiput to Subnasale

My observations on this topic are confined to relatively few children and these mostly older than 5 years. It appears that the mean horizontal distance of subnasion from the occipital plane is, before 10 years, less than the maximum head length (to glabella), but after 11 years greater, and increasingly so at least to 17 years where it is 7 mm. the greater. This relatively rapid increase is, of course, due to the growth of the maxilla and its contained sinus. This growth, in turn, is rendered necessary to accommodate the molar teeth, as Keith ('21, Fig. 176) has clearly shown. The

visceral part of the skull is developing, from 6 to 17 years, faster than the cranial part. This matter is discussed in further detail on pages 125, 134

10 Growth of Head Girth

General—The head girth is perhaps the single dimension that best expresses cranial size quantitatively, although it does not, like the skull modulus, take height of the skull into account, also it does not measure varying cephalic index

In measuring the head girth, the tape was passed over the glabella and the occiput so as to give the maximum (approximately horizontal) girth

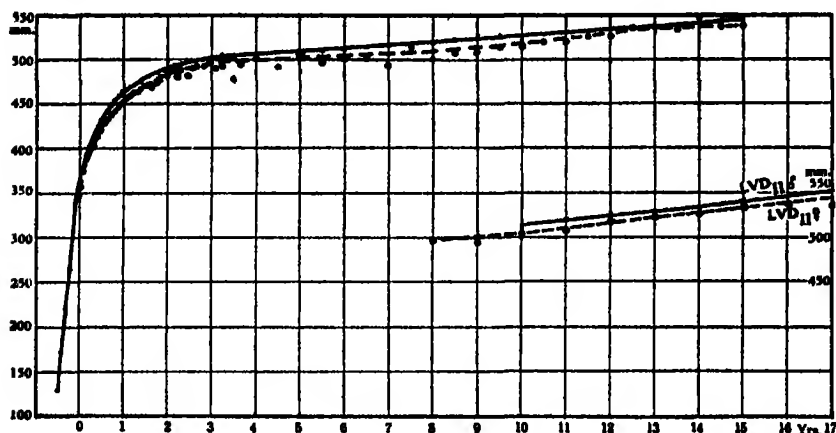


FIG 42 Mass curves of growth of Head Girth, males and females, from embryo to maturity From over 5 years, BOA Also data from LVD_{II} series, using ordinate scale at right

Figure 42 shows the growth of this dimension from 20 weeks fetal to maturity, for standard children During the latter half of intrauterine life the head girth increases about 2 times, as does also the sitting height (crown-rump dimensions) The curve of Fig. 42 agrees, for the pre-natal part, with data of Daffner ('02, p 77) My post-natal girths are larger than his (at p. 323) by about 2 per cent. In my data head girth at birth averages about 355 mm for males, exactly the same as Kugler's ('32, p. 500) mean for Zürcher neonates. At one year my mean is 455, Bondireff's for Russians, 450, Pfaundler's for Germans, 460 mm. My mean series for boys from birth to 36 months agrees fairly well with Bayley and Davis' ('35, p. 37), except that the difference between the means of the

sexes is, in my series, usually of the order of 5 to 10 mm, and theirs of 6 to 20 mm

Data on head girth at various ages have been afforded by Daffner ('02, p. 402), Ranke, Pfaundler, Hoesch-Ernst ('06, p. 92), Schwerz, Quételet ('69), Bonifacy, Bondireff, Weissenberg, Landsberger ('88) and Pfitzner. The mean girth at birth averages about 340, and rises to about 550 at maturity.

Sexual—Figure 42 shows the comparative growth of the head girth as between boys and girls. In the standard series (above) the sexes become measurably different at birth, the mean girth of the boy's head being above that of the girl (463 453 mm) by about 10 mm at 1 year and thereafter, until at adolescence the difference is temporarily expunged (at 538 mm), just as Bondireff ('02) found, but Ernst ('06, p. 93) did not

In the lower part of the figure are curves of mean head girth of boys and girls of the LVD_{II} series. The mean girth of the head of the girls from 10 to 19 years is about 10 mm less than of the boys. The curves approach slightly but do not touch at 14 years. Of course, this smaller head girth of the girls is correlated with their generally smaller dimensions. Ernst ('06, pp. 45, 95) found that head girth of boys or girls of about 125 cm stature was about 41 per cent of stature. I find in both the BOA and LVD_{II} series that the ratio, head girth to stature, is about 5 per cent larger in the boys than in the girls. Also, at a given stature the relative head girth in girls is from 97 to 90 per cent that of boys.

Social—The mean head girth of the BOA group is greatest, those of the LVD_{II}, I and M series, at 14 years of age, decrease in that order. In the three upper curves there is a reduction of slope at sometime between 11 and 15 years. This is followed by an increased slope of the curve at 12 to 15 years. Except in the mongoloids the increase of slope begins the later the smaller the mean head girth. There is acceleration after retardation. Thus the change to increasing slope occurs in the upper curve at 533 mm. at 12 years; in the middle curve at 528 mm. at 13 years, and in the lowest curve at 526 mm. at 15 years. The mongoloid curve seems to advance without change of slope from 10 years onward.

Racial.—The Mediterranean and Jewish groups lie below the Nordic at 14 years, though the Mediterranean was higher than the Nordic at earlier than 14 years. The Negro group exceeds the Nordic at all ages, the excess diminishing from about 6 mm. at

12 years to 2 mm at 16 years Rouma ('21, p. 94) found an excess of head girth in Cuban Negroes over whites of 2 to 7 mm. While our Nordic adults have a head girth of the order 560 mm. at 18 years, Schwerz ('10) finds a head girth in Swiss boys, 18 years of age, of 545, Seaver ('09, p. 10) for New England children finds a head girth at 16 years of 547 mm. for boys and 550 mm. for girls, but it is doubtful if the head girth of the average girl ever exceeds that of the average boy of the same age.

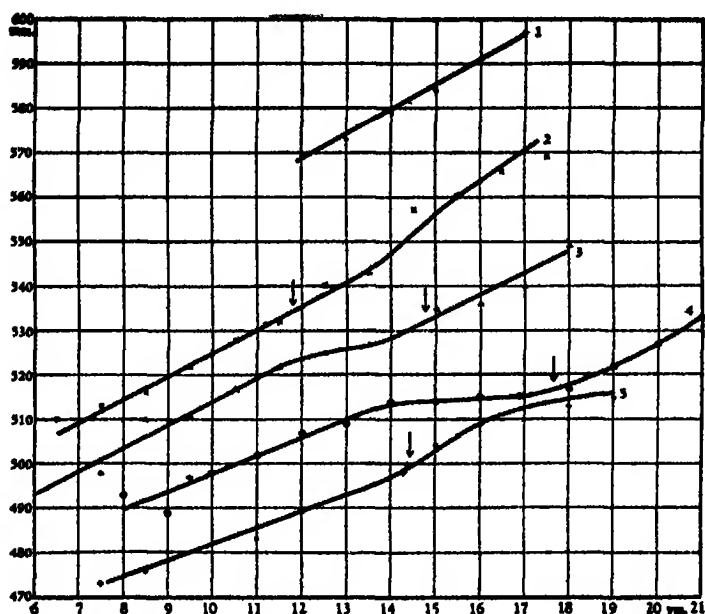


FIG 43 Individual curves of growth of Head Girth, U S and Nordic males, LVD_I and II 1, D_{II} No 79, 2, D_I No 55, 3, D_I No 54, 4, D_I No 30, 5, D_I No 2 Arrows indicate time of spurt of stature growth

Individual.—Figure 43 shows how head girth grows in five individuals. In the longest series, starting from 6 to 8 years, growth is nearly strictly rectilinear. It is so from between 6 and 12 years in all these boys. In three cases a spurt of growth starts at around 14 years. In one case it is delayed until 17 years. The arrows show the period of actual beginning spurt of stature. This is seen to be much delayed in the case of No. 4. Thus even the growth of the girth of the head, including the skull with its frontal sinus, is associated with that of the body as a whole. It will be noted that head girth in these boys is still increasing rapidly at 17

to 21 years Saller ('28, p. 70) finds growth of head is not closed at 20 years.

Familial.—Figure 44 shows, above, the curve of growth of the head girth in four boys in the Hic fraternity. In three of the

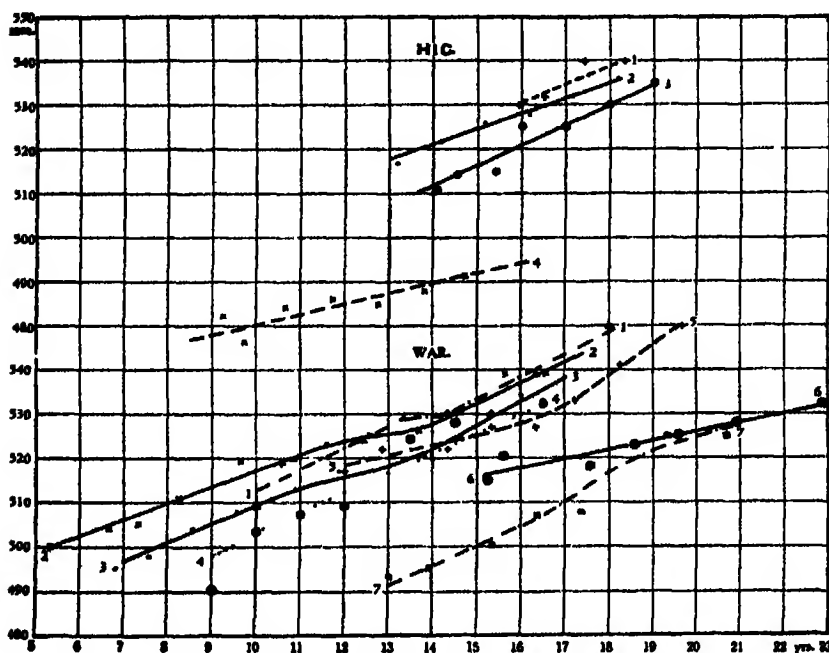


FIG 44 Individual curves of growth of Head Girth in two fraternities Hic 1, F H m; 2, C H m, 3, H H m, 4, C H m War (statures at 16 years are given in cm) 1, Fk W m, 151.0, 2, G W m, 150.2, 3, S W m, 165.7, 4, W M f, 149.6, 5, W W m, 148.9; 6, B W f, 159.3, 7, Fd W m, 152.1 Two sets of ordinates

brothers the growth curves are rather close together, at 16 years not over 10 mm. apart. But one brother, No 4, has a head girth that is 26 mm less than the next smaller headed brother

Below are shown the growth curves of seven members of the War family (5 boys and 2 girls). The growth curves of Nos 2 and 3 run nearly parallel courses. Those of Nos. 1 and 2 are close together at all ages. Also, the curve of No 5 is much like that of No 1, only No 5's head girth runs 5 to 10 mm. less than No. 1's. One boy, No. 7, stands quite apart from the others. The curve of one sister (No 4) lies with the group of the boys, of the other with the aberrant boy, No. 7. In order of stature the children stand as follows at 16 years: No. 3, 165.7 cm, No. 6, 159.3, No. 7, 152.1, No. 1, 151.0, No. 2, 150.2; No. 4, 149.6, No. 5, 148.9. No. 1, who

has the largest head, lies in the half group with the smaller statures, while No. 7 with the smallest head is next above No. 1 in stature. No. 6, who is much the larger girl, has the smaller head of the sisters. Thus, the relation between head girth and stature is, in this family, not close.

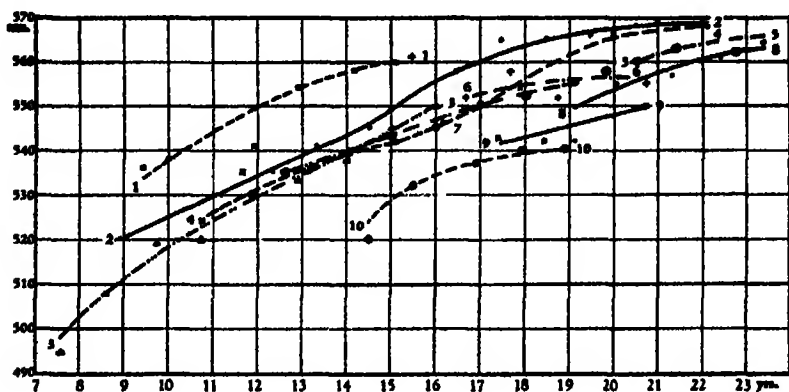


FIG 45 Individual curves of growth of Head Girth for Mea fraternity 1, R M f, 165 5, 2, G M m, 157 0, 3, B M f, 160 0, 4, W M m, 152 0, 5, Ml M f, 6, H M m, 144 0, 7, N M f, 138 0, 8, L M m, 9, E M f, 10, Ma M f, 165 5 The numbers given are stature in cm at 16 years, except No. 1 which is at 15 years

Figure 45 shows the growth curve of head girth for ten children, of the Mea family (4 boys and 6 girls). With two exceptions they are seen, boys and girls alike, at 16 years grouped close to 550. The largest and the smallest heads are both of girls, and girls of the same stature!—a remarkable instance of the independence, in this case, of head girth, sex and stature. The statures at 16 (or 15 years) are given in the figure's legend.

Figure 46 gives the growth curves of head girth for the 2 brothers and 2 sisters of the Sti. fraternity. The curves of the 2 boys are above, about 10 mm apart at 17 years, having run nearly parallel courses, the two girls are 20 mm. apart. Here differences in head girth are associated with differences in stature in the sexes.

Twins and Special Cases—Figure 47 shows, near the middle, the growth curves of head girth of the monozygotic Gar twins. The curves are not at exactly the same level but run strictly parallel. Twin 1's head is of the same width as, but somewhat longer than Twin 2's. The curve of head growth of their brother, No. 4, is also given as a sort of control. The curves of the Kyr. and Sha twins run parallel courses. Scattered over the figures are fragmentary curves of head girth of three cretins and of a microcephalic

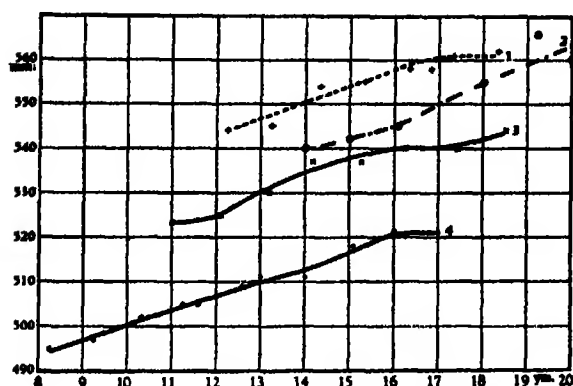


FIG 46 Individual curves of growth of Head Girth for Sati fraternity 1, L S m, 2, C S m, 3, E S f, 4, M S f

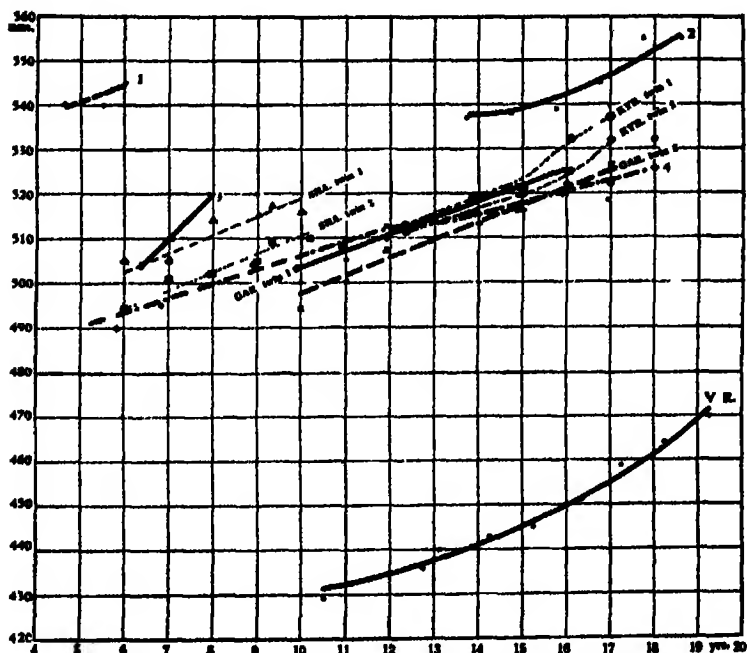


FIG 47 Individual curves of growth of Head Girth of twins and special cases 1, H L m, cretin, 2, C B f, cretin, 3, A A f, cretin, 4, J G m, brother to f Gar twins V R m, microcephalic

(V.R.). In the microcephalic, head girth rises from 430 mm at 10½ to 470 at 19 years. In adult microcephalics girths of from 525 to 375 mm. have been recorded (Tredgold, '15, p. 207) The head girth of Mesek was 562 mm.

11. *Relative Head Girth*

No special statistical study was made of this ratio. Examination of data from a few boys and girls show that the ratio falls rapidly with advancing age and stature. I cite a few examples in Table 3.

TABLE 3

CHANGES WITH AGE IN HEAD GIRTH IN RELATION TO STATURE IN 12 INDIVIDUALS

Age (years)	4½	5	5½	6	6½	7	7½	8	8½	9	9½	10	10½	11	12	13	14
R H ♂ BOA			47		46		44		42		41						
W M ♂ BOA				46		45		43		41		40					
F S ♂ BOA	53		51		48		46		45		43						
R V ♂ BOA			45	44			42		40		40		39				
H B ♀ BOA				42		40		38		37		36					
T H ♀ BOA			50		48				44		42		40				
M M ♀ BOA	46		44		41		40		39								
M A ♂ LVD _{II}														39	38	37	35
J B ♂ LVD _{II}															37	36	34
H A ♀ LVD _{II}														36	35	34	32
B A ♀ LVD _{II}															38	37	36
V R. ♂ Microcephalic Idiot													36			33	34

Summary —The head girth of boys is about 10 mm larger than that of girls. Its size is greatest in standard boys, least in mongoloid dwarfs, and greater in Negroes than in the other races studied. Its increase shows a spurt at the time of the adolescent spurt in stature, but the head girth is not closely correlated with stature.

Relative head girth tends to decrease with age, i e, the legs grow faster than head girth. Also, relative head girth is, as stated, regularly less in girls than boys of the same stature.

In the case of our microcephalic (V.R.) the relative head girth is about 36 per cent of stature instead of about 42 per cent as in normal boys. He is 132.3 cm. tall at 14 years.

III. CHANGE OF PERCENTAGE RATIOS (CRANIAL INDICES) WITH AGE

The growing head is not only changing in absolute size of its various dimensions, but also it is changing its proportions and is gaining a differentiation in form. The changes with age of seven of these proportions will be considered in this part.

1. Cephalic Index

General — This ratio of $\frac{\text{head width}}{\text{head length}}$ is the most commonly determined head ratio. It has been regarded as a good racial diagnostic since Anders Retzius ('43) first showed its importance as such. Its distribution over the earth has been described by Dixon ('23) and more philosophically by Gerhardt ('38). A table of means of this ratio for different races of mankind is given in Martin ('28, pp 775-778). The mean ratios run from 71.8 for inhabitants of Badaga in South India to 89.9 for Californian Indians. There is always a suspicion that these extremes for Amerindians may be partly due to head deformation. Among Asiatics the tribe of Faranchi of Chinese Turkestan gives an index of 89.2

The different sizes of the cephalic index are grouped for convenience under three heads, defined by Martin as follows

Dolichocephaly	x - 75.9
Mesocephaly	76.0-80.9
Brachycephaly	81.0- x

Pfitzner ('99, p 372) writes "Der Langen-Breiten-Index des Kopfes ist während des ganzen extrauterinen Lebens konstant," and Pearson ('02) and Tippet ('23) regarded this index as tolerably constant. On the other hand, Bachmaier ('28, p. 61) found the heads of 8 year olds to be relatively narrower than those of 6 year olds. Keiter ('33, p. 347) and a number of other authors find the mean cephalic index to decrease slightly from 4 to 16 years.

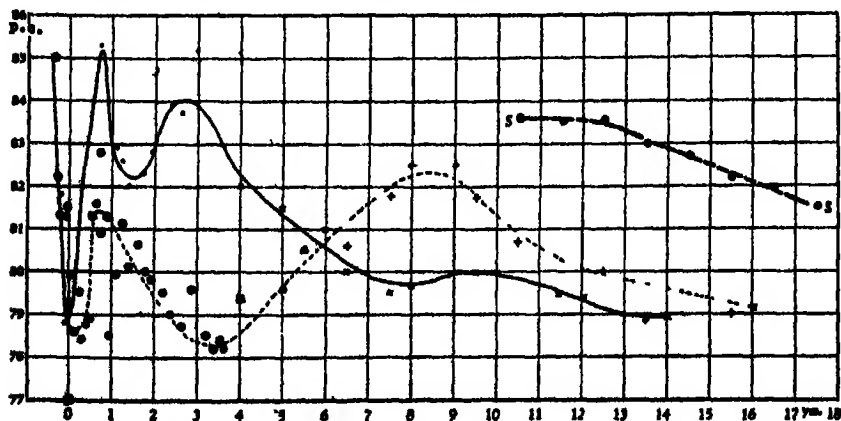


FIG 48. Mass curves of change with age of Cephalic Index, or Head Width/Head Length ratio. Symbols as in Fig 1. S data from Saller ('30)

Figure 48 is an attempt to show the changes in the mean cephalic index with age of a group of European stock from 4 months before birth to maturity. The curve is a complex one and offers many points of interest.

The pre-natal portion deserves careful consideration. It has seemed worth while to assemble here all the (to me known) available data on this part of the curve in Table 4.

TABLE 4

AVERAGES AND RANGE OF VARIATION OF THE CEPHALIC INDEX IN WHITE FETUSES

Age Lunar Mos	Schultz, 1926			Corrado, 1899 Avg	C B D Avg	Scammon & Calkins 1926 Ratios of Means
	Avg	Min	Max			
3d mo 1st half	87.4	81.6	98.0	81.8		74.7
3d mo 2nd half	86.6	77.8	96.9			79.2
4th mo	87.6	76.5	97.2		89.9	79.4
5th mo	84.9	76.7	93.0	73.8	84.0	78.2
6th mo	82.2	74.6	89.1	77.7	82.2	78.0
7th mo	81.3	71.6	92.1	80.2	81.8	78.4
8th mo	81.3	74.0	87.2	85.2	78.8	77.4
9th mo	81.5	73.5	85.3	80.5		79.8
10th mo	79.3	71.2	90.3		79.3	80.3

It seems quite clear that the cephalic index at the fourth month of gestation is very high, about 87 per cent, because width has been growing faster than length until the fourth month. After that the ratio rapidly falls to 81 per cent at the eighth pre-natal month. The mean index for males at birth is 79.3 in my records, 80.1 in those of Freeman ('32, p. 73) and 77.6 (ranging from 66.1 to 86.4) in the extensive series of Kugler ('32, p. 567) for Swiss neonates. Schreiber's ('28, p. 683) mean data for head length and width at birth yield an index of 76.5 for males, 78.8 for females.

Having reached this nadir of dolichocephaly at birth the cephalic index rises rapidly during the first 10 months post-partum, until it reaches a maximum of 85.2 in my mean series, 86.3 in that of Freeman ('33, p. 196) and 86 in that of Bayley ('36). This peak is a result of the very rapid growth of head width toward the end of the first post-natal year, which in turn is conditioned by the reduction in increase of head height through gravity, that occurs at this time. This gravitational effect is temporary, however, passing as the membrane bones of the skull begin to come in contact. Thereafter the cephalic index decreases, at first about 1.5 per cent p. a., at 10 years about 0.2 per cent p. a. (Fig. 48).

Figure 49 gives the changes in mean cephalic index more in detail for my observations on babies, 38 to 8 individuals for each age. The smaller frequencies are for the 3 year olds. I have included also curves based on Bayley's data. All of these curves

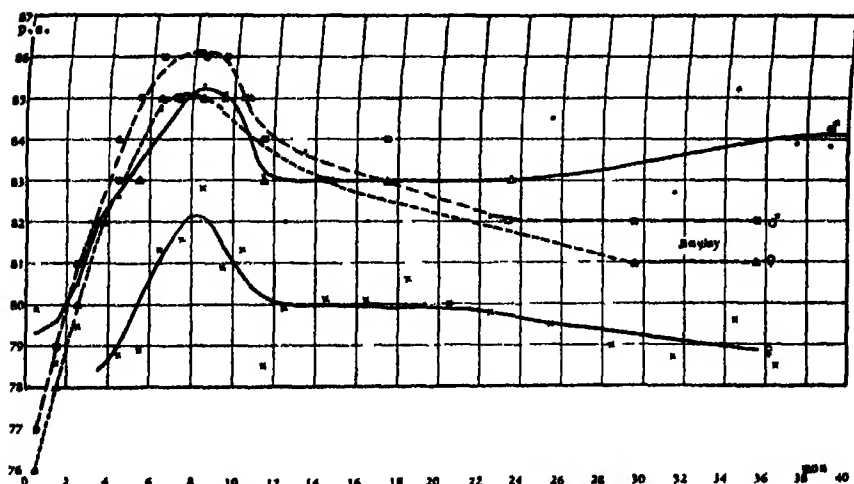


FIG 49 Mass curves of change with age of Cephalic Index, babies only, male and female. Unbroken lines, Babies Hospital, males above, females below. Broken lines, from data of Bayley ('36). Abscissæ in months.

show a rise in mean cephalic index from 1 month post-natal to about 8 months. On the average the change in the index is from around 79 up to 86. Thereafter the mean index decreases rapidly to about 12 months and then more slowly. Freeman ('33) found the same increase, the curve reaching a crest with him at 11 months and thence falling rapidly. (See also individual curves in Fig. 50.)

Sexual.—Already at 4 months post-partum there appears a marked difference between the sexes in the cephalic index (Fig 48). The females' is about 4 points less than the males'. At 3 years the departure is 5 points. The numbers are small in the later months (although not so small as in Bayley's series) and since the male curve does not tie up well with the BOA curve of Fig 48, I am inclined to think the last position of the male babies too high and thus the sex difference too great. The female BOA data were mostly not taken by myself, and may have been slightly influenced by a personal equation of my well trained assistant which possibly tended to make the heads slightly too short and the cephalic index

thus larger than that of the males. On the other hand, the data of Boas and Wissler ('06, pp. 55 and 93) on 8 year old girls, Nos. 1 to 50, show a mean cephalic index of 0.8036 and on 50 8 year old boys of 0.7887—or a difference of 0.0149, as contrasted with a difference of 0.025 in our BOA series. (Cf. Bean, '34, p. 283.) So I think it justifiable to conclude that, at this age, the female head has the larger cephalic index. In adults the average difference is very small, in some racial groups the mean ratio for males is greater than for females, in other groups this relation is reversed.

Social.—In our Nordic group the cephalic indices of the BOA (standard), LVD₁ and Idiot series do not differ markedly, though the Idiots are consistently about 0.5 point below the BOA groups. The mongoloid dwarf group lies consistently 2.5 to 5 points above the others, with an index in the neighborhood of 83 per cent. It has, indeed, been long recognized that their heads have, like those of the Cochín Chinese and Tonkinese, an exceptionally high index. Probably the same internal (glandular) causes that control the horizontal proportions of the cranium act in both cases.

Racial.—Of these 3 racial groups the Mediterranean comprises the heads with highest cephalic index, at 13 years around 81.5 per cent. At 11–16 years the Jewish boys have 1 per cent narrower heads than the Italians. The North European stocks come below with an index of around 80 per cent, but falling below this level at adolescence. The American Negroes within the same points have the smallest index, around 79 per cent.

Individual.—While the cephalic index has been considered a valuable index of race its individual variation is certainly striking and changes in the ratio during development are very great. The brief paper by Kruse ('34) is very important in this matter. Figs. 50 and 51 give individual growth curves for a few boys and girls during the first three post-natal years. The range of the index at birth is great, from 72 to 82. In about 250 males, measured at birth, Kugler found a range from 66.1 to 77.6. Generally, as already seen in Fig. 48 the index increases rapidly for two or three months after birth. Thus in No. 2 (Fig. 50) the increase is from 79.5 to 92.3, although the ratio falls to 86.5 at 32 months. In the case of two girls whose growth curves are shown in Fig. 51, during the first two months a marked decrease in the index, amounting in one case to 7 points, occurs. In general, after the first two months the index reaches a maximum at between four and twelve months and thereafter diminishes.

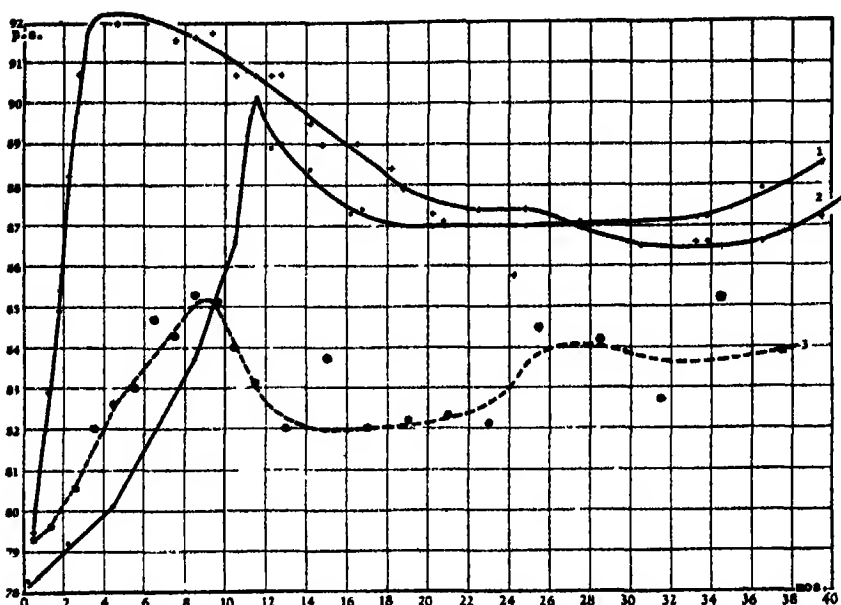


FIG 50 Individual curves of change with age of Cephalic Index in two male babies of Normal Child Develop Clinic 1, No 9 m, 2, No 18 m, 3 (for comparison) mass curve of boys, based on 38 to 5 measurements per point For another interpretation of same mass data see Fig 64, m Abscissae in months

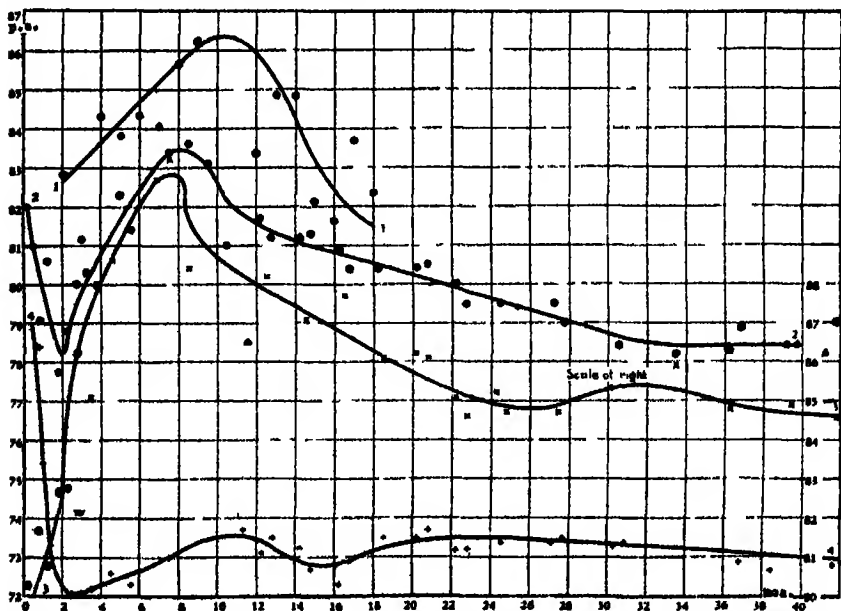


FIG 51 Individual curves of change with age of Cephalic Index in three girl babies 2, CC No 3, 3, MK No 1; 4, RN No 6 Also curve 1 based on mean of Freeman's ('38) data for girl babies. Ordinate scale at right for curve 3 only Abscissae in months

Figure 52 gives changes in cephalic index for five boys of the LVD_I series. All are of U. S. Nordic stock, except No. 5, who is Italian. It will be seen that in all cases except No. 3 the ratio tends

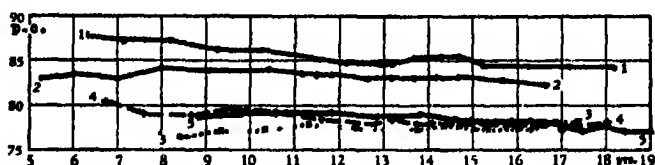


FIG 52 Individual curves of change with age of Cephalic Index of five boys of the LVD_I series 1, CH No 83, U. S., 2, HM No 25, U. S., 3, JB No 54, U. S., 4, DF No 57, Ital., 5, EH No 79, Scand.

to decrease from childhood to puberty, sometimes as much as 3 or 4 points. In the case of No. 3 there is an increase of 1.7 points. But usually the head tends through late childhood, juvenility and adolescence to become more dolichocephalic.

Familial—Figure 53 shows the curves of change of cephalic index in one fraternity measured repeatedly during several years.

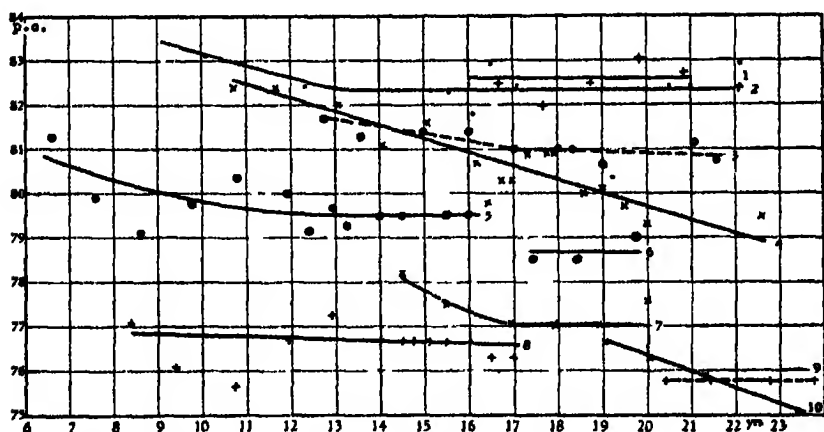


FIG 53 Individual curves of change with age of Cephalic Index in members of the Mea fraternity 1, HM m, 2, GM m, 3, NM f, 4, WM m, 5, BM f, 6, EM f, 7, Ma M f, 8, RM f, 9, Mi M f, 10, LM m.

In this family there is for the most part a steady decrease in cephalic index especially at 9 to 11 years (curves 2, 4, 5). The slope is about 0.3 per cent p. a. Something of this slope is found also in 10, 7 and 3. Mostly in later years, usually after 14, the slope becomes very small and in most cases eventually is zero. In individuals 4 and 10 the head is becoming more dolichocephalic, 0.3

points p.a., even at 20 years The index shows a great range, at 16 years from 76.6 to 82.6 Curves 1 and 2 practically coincide during 5 years, 3 and 4 are close together from 12 to 16 years

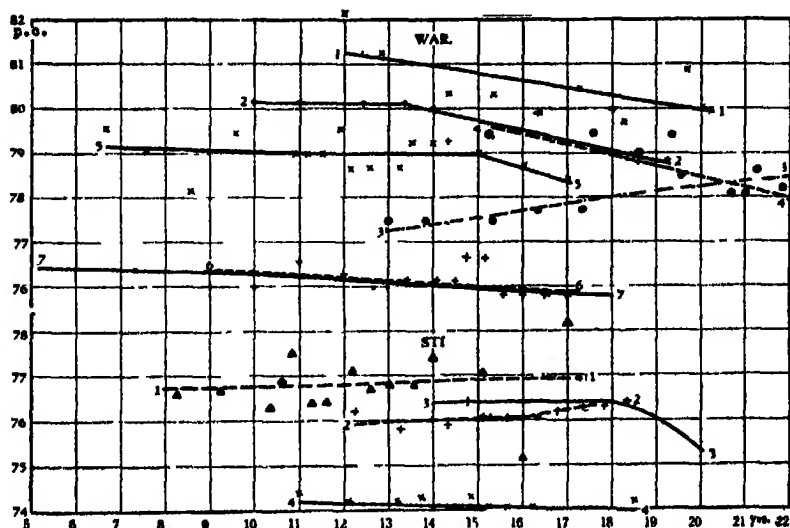


FIG 54 Individual curves of change with age of Cephalic Index in members of the War (above) and Sti (below) fraternities War 1, W W m, 2, Fk W m, 3, Fd W m, 4, B W f, 5, S W m, 6, M W f, 7, G W m Sti 1, M S f, 2, L S m, 3, E S f, 4, C S m Two sets of ordinates

Figure 54 shows on the upper part of the figure the curves of change of cephalic index for the War fraternity The curves for the most part slope downward, but show a much less slope than in the Mea fraternity of Fig 53 There the typical slope was about 0.3 point p a, here about 0.05 point p a, especially before 13 years of age. There is a more rapid increase toward dolichocephaly in later years in contrast with the reduction of slope characteristic of the Mea. fraternity However, in one case (No 3) the head from 13 to 22 years shows a slight increase in the index.

The range in the index at 16 years is from 75.9 to 80.7, or 4.8 points (as opposed to a range in the Mea fraternity of 6 points), and there is a family parallelism in some of the curves, such as Nos 2 and 4, 6 and 7. Indeed, the heads of Nos 2 and 4 have practically similar proportions that change in the same way for at least four years

Figure 54 shows, below, the curves of change of cephalic index in four members of the Sti fraternity All have much smaller

cephalic indices (74-77) than the mean of the War fraternity, yet both fraternities bear English names and come from rural New York. In the Sti. fraternity the indices of the children's heads change very little in the years of juvenility and adolescence. Indeed, in two cases the curves seem to slope upwards so that the children are tending slightly toward brachycephalism.

Twins.—The change with age of the cephalic index is shown in Fig 55 for 3 pairs of twins, by various criteria identical. Thus the

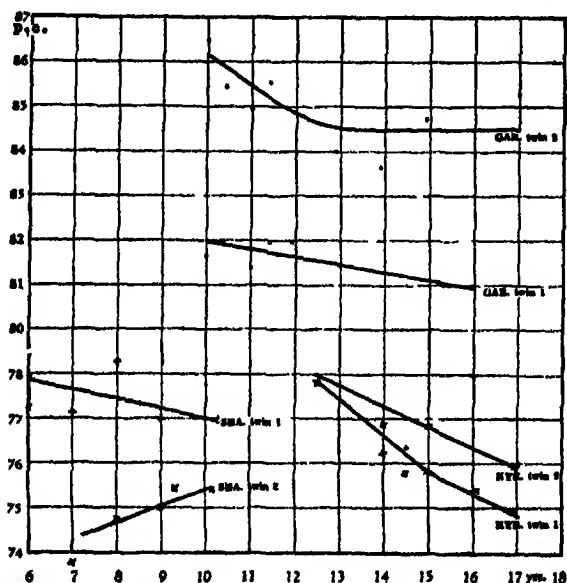


FIG 55 Individual curves of change with age of Cephalic Index in three pairs of Monozygotic twins Gar f, Kyr and Sha m Two sets of ordinates

Sha. twins are both hearing mutes. The Gar. and Kyr. twins have very similar facial features and papillary patterns. In the case of the Gar. twins the width and height of the head of the girls is practically identical (Figs. 14, 22), but the head length differs greatly (Fig 7), and consequently the indices lie 3 or 4 points apart. In the case of the Sha. twins the difference is about 2 points, in the Kyr. twins less than 1 point. In both cases the slopes tend to approach parallelism with age.

Figure 56 gives a comparative view of the size of the cephalic index in a number of special cases. At the bottom (No. 12) lies the curve of a microcephalic. It undergoes little change from 10½ to 19 years, and lies at about 72.2. At the other extreme is the

index of a cretin, No 1, shown with another scale at left It stands at about 91.7. In the cases of other microcephalics — for No. 9 the index is at 77.9, No 5, 79.6, and No. 7, 80.8.

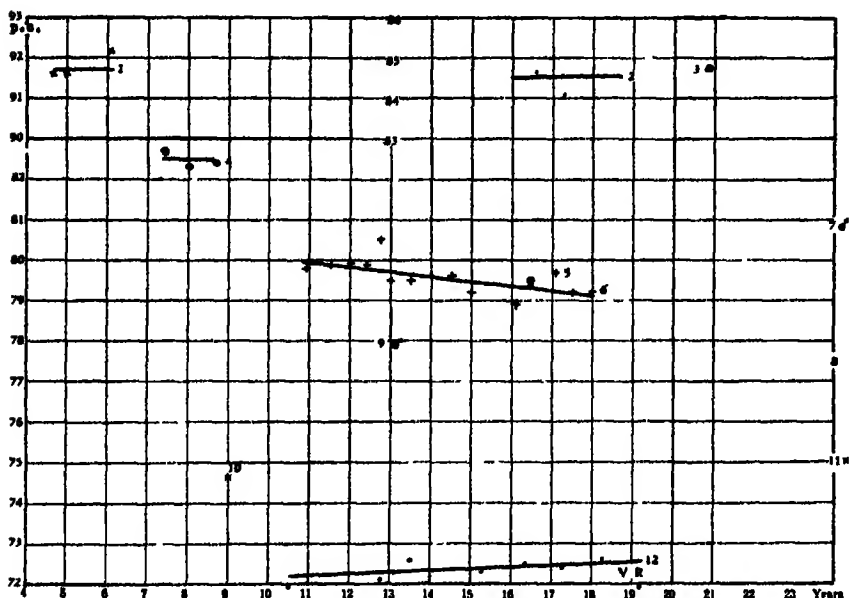


FIG 56 Individual curves of change with age of Cephalic Index in some special cases 1, H L m, cretin, 2, A W f, ateliotic dwarf, 3, P U m, hypopituitary, 4, M K m, hypopituitary, 5, A G f, microcephalic, 6, M C f, dwarf, 7, A T m, microcephalic, 8, D R f, hypopituitary, 9, F K m, microcephalic, 10, R W m, hydrocephalic, 11, H S m, gigantism; 12, V R m, microcephalic Cases located at the right hand margin were over 23 years of age on examination Two sets of ordinates

Hypopituitaries, Nos 8, 4 and 3, have indices at 77.5, 82.5 and 84.7 respectively. Two ateliotic dwarfs, Nos 6 and 2, have indices of about 79.5 and 84.5 respectively

Discussion —To what extent is the form of the individual curve of cephalic index of infants influenced by "birth molding" that is, distortion due to pressure of the muscles of the birth canal and other surrounding tissues? This is a matter that has been investigated by a number of authors and in 1930 by L. J. Tiber, a pupil of Richard E Scammon.⁶

From all this work it appears that the sagittal-oblique (subocciput to bregma) is perhaps the dimension that is most altered (reduced) by the pressure of the muscular (circular cylin-

⁶ Tiber's doctor's thesis has not been published For opportunity to consult it I am indebted to Dr Scammon

dricul) walls of the birth canal in the case of normal vertex presentations. There is a mean reduction in this dimension of about 9 mm. (Scammon and Calkins, '29, p 98) This is restored in the course of the next ten days. The head width (biparietal diameter), on the other hand, seems to be increased in the birth process. It becomes reduced in the next two or three days post partum before growth processes start it increasing again. On account of the decrease of the sagittal-oblique and increase of head width the head of the neonate, born with vertex presentation, is rendered temporarily more brachycephalic. But quickly the genetic form is restored. Thus in 161 of the babies measured by Kugler ('31, p 564) at 1 day and 9 days after birth, the cephalic index had in nine days decreased in 58 per cent of her cases, increased in 39 per cent, and remained the same in 3 per cent. Thus in about one week after birth, on the average, the head had become more nearly dolichocephalic in nearly 60 per cent of the cases. It is probable that the restoration of the "molded" form is not always completed in seven days. The time required for complete remolding is probably dependent upon the degree of deformation in the process of birth. Some deformation may occur by precocious molding of the head through intrauterine pressure and this may be more tardily smoothed out after birth (Budín, '76).

Whether the deformation at birth affects the form of the head in later life is a doubtful point. The obstetrician, C H Stratz ('22, p 122), states that the displacements of the cranium are usually smoothed out in a few days, though traces of them in the form of slight asymmetries often persist throughout life. Mueller ('07) believes some of the head distortions to be persistent to a certain degree.

If the birth molding renders the head more nearly brachycephalic, then the cephalic index taken nine days after birth should be less than on the day of birth, as Kugler found. In my studies of babies measured at 1 or 2 days post partum and again three weeks later, all had a change of cephalic index, respectively as follows. Rhoda N., 79.9 to 78.4, Charlotte C., 82.0 to 79.2, Lionel LaJ., 80.9 to 79.7, Adrienne LaP., 83.9 at 2 days to 77.2 at 39 days.¹ The last is truly a great difference (6.7 points) even though at the later measurement the baby is over five weeks old; for in five weeks normal change in cephalic index is rarely more than two points.

¹ In all cases, except Rhoda N., delivery was spontaneous. In the case of Rhoda there was low forceps delivery and head rotation.

The cephalic index of one or both parents was obtained for three of the babies. They are as follows: Rhoda N. M., 75.1, F., 73, Charlotte C. M., 84.1, F., 77, Adrienne LaP. M., 82.3. From these fragments it will appear that the baby (Rhoda) with the two dolichocephalic parents showed the greatest deformation from the probable hereditary type toward brachycephaly of any. Probably it is usually true that dolichocephalic heads undergo more birth distortion than brachycephalics, since the latter type is better adapted to the circular cross sectional form of the birth canal.

Often following the adjustment of the head form during the first post-natal week in the direction of dolichocephaly the average head tends, as we have seen, to become more brachycephalic. The cause of this trend of growth of the head is uncertain. It is probable that at this stage there is a continuation of that markedly rapid increase in transverse diameter in the region of the parietal lobes of the brain that is marked before birth. Thus Scammon and Calkins ('29, pp. 97, 117) find, with an increase in crown heel length of about 48 mm. from 423 to 471 mm., the occipital-frontal diameter increased, on the average, 9.3 mm., while the biparietal diameter, on the average, increased 9.7 mm., an increase of 9.3 per cent in the first case and 12.5 per cent in the second.

The causes, other than genetic, of the variation in cephalic index have been much discussed, and there are those who think that environmental causative factors are of great importance.⁸ The fullest experimental study of this matter was made by Walcher ('05). He experimented with 555 infants born in a school for midwives during five years. Half of them were provided with hard pillows, and had attractive objects dangling from the sides of the crib, so they tended to lie on the sides of the head. Half had such very soft pillows that they could hardly turn in them so that they tended to lie on the back of the head. Of those kept lying on the back of the head 84 per cent gained, after some weeks a head index that was, on the average, 3.75 points larger. Of those trained to lie on the side of the head 62.7 gained a head index that became progressively smaller (narrower heads) on the average, by 2.56 points. Of the 16 per cent in the first case who failed to gain a larger head index the average reduction of the index was 1.39 points. In the second case the 37.3 per cent who failed to get narrower heads, the increase of head index was 1.66 points. All

⁸ Reference may be made to the casual observations and speculations of Bean and Speidel ('23).

those results support the conclusion that there is an inherent tendency for the average child's head, after the first week (when birth distortion has been smoothed out) to become more brachycephalic, but this internal tendency may be in part counteracted by the action of gravity. In other words, the heads were more easily made progressively more brachycephalic than dolichocephalic. It may be added that Basler ('27) 15 to 20 years later determined the cephalic index of 5 of the persons who as babies had been subjected to Walcher's experiments. Basler concludes that his measurements 15 to 20 years later do not answer the question whether the present head form depends solely upon the experimental influencing of the children or on a congenital *Anlage*. But why should we expect the result to depend solely upon one factor?

Kruse ('34) repeated Walcher's experiment on 14 infants, keeping them in the lateral position for 1 to 3 weeks. The mean index before the experiment was 85.8, after the experiment 73.3. Thus Walcher's results were confirmed. Kruse is inclined to minimize the importance of heredity in influencing head form but he admits we have no reason for thinking that the brachycephalic children of south Germany lie in bed differently from the dolichocephalics of north Germany. However, short headed children would find it easier to lie on occiput than long headed children, so perhaps environment may help to accentuate a genetic difference, once it has appeared.

Catell and Grube ('34) also repeated Walcher's experiment. Their conclusions are important. "Without doubt one can succeed through a definite *Lagerung* in transforming the originally present form of heads in neonates and sucklings, but the degree depends upon the skull form originally present. Previously dolichocephalics do not have the dolichocephaly increased by the lateral position, nor in high grade brachycephalics is the brachycephaly increased by soft back-lying." Thus infants with a cephalic index under 80 to 85 by the lateral position gain only a slight reduction; but by back-lying a significant increase of the index; conversely, if the index is high, 80 to 100, the lateral position results in a significant reduction but in the soft back position only a slight increase. Extremes can not be rendered more extreme, but can be modified toward the mean. Also, the induced change toward dolichocephaly or brachycephaly is not persistent. In the construction of the eventual skull form self-differentiation, that is the hereditary

tendency, clearly is much the stronger, indeed the form determining factor.

That the head is easily deformed in infancy is obvious not only by the changes in shape occurring at birth, but by the structure of the skull. The infant cranium is made up mostly of the paired frontal, parietal and temporal bones and the quadripartite occipital. Between these bones are membranous spaces, the fontanelles, which permit movement of the bones in various fashion even to overlapping. Some peoples have taken advantage of this plasticity of the skull to deform the head by means of bandages, as among various tribes of Indians. This may be regarded as a mark of distinction, like small bound feet among Chinese babies or plucked eyebrows in modern girls. In extreme deformation by bandages a permanent modification is caused.

Bayley ('36, pp 8-13) has considered the question, what causes the change in cephalic index by which it "increases from birth to seven months and decreases after ten months"? Based on observation of a Cæsarian birth in which the cephalic index increased profoundly "during the early months" she thinks the hypothesis that the change is not a result of external pressure at birth is given some support. Also, boys' heads, though larger and thus more apt to be deformed at birth, do not become in the first eight months more brachycephalic than girls'. Moreover, the head indices of first born babies are for the first four months only slightly smaller than for later born. These lines of evidence are not altogether satisfactory, since most of the effects of birth distortion are smoothed out within a few days after birth. Assuming that the less active infants lie inert on the back of the head for more hours than the active infants, and so might become more brachycephalic, Bayley found a coefficient of correlation between the cephalic index of an infant and its "activity at twelve months as rated on a seven point scale" to be — .39. The conclusion is drawn, that "pressure on the back of the back of the head does have some influence in changing a child's cephalic index." However, the Bayley technique must be regarded as less well adapted to securing an answer to the problem than that of Walcher. Bayley (p 11) does refer to the case of one of the infants who habitually lay on one side, and opposed being placed on the back. She was very dolichocephalic at all ages, but nevertheless the index rose from 71 at one month to 76 at 8 months, but after nine months decreased again.

TABLE 5

HEAD DIMENSIONS OF PATIENTS BED-RIDDEN FROM BIRTH,
ALL BUT ONE MEASURED ON TWO OCCASIONS

Number	Age Yrs	Girth	Length	Max Width	Height	Minimum Frontal	Height Length	Cephalic Index
<i>Boys</i>								
2024a (1)	25	567	190	152	131	114	69.0	81
3253b (2)	23	453	144	135	124	100	86.1	94
3415c	22	497	158	137	125	107	79.1	87
2814d	20±	523	177	140	123	110	69.5	79
<i>Girls</i>								
2065e	22	500	165	138	119	112	72.2	84
3136f	24	536	166	127	119	105	71.3	78
3284g	27	520	177	140	120	101	67.8	80
6688h	28	—	158	149	129	102	86.6	95

Notes Based on Social Investigator's Reports

- a Spastic quadriplegia noticed at birth, cannot hold body erect
 b Paralysis of right side from (or 2 days after) birth Never learned to sit up, could not hold up head
 c Never walked or talked Bed patient at Randall Island when received from there at 3 years of age
 d Never walked or talked As a baby could not hold up head
 e She has never made any attempt to walk or talk
 f Has never walked, spastic from birth
 g Injured at birth, has never walked or talked
 h Born paralyzed in legs and arms, has never walked or talked

To see whether head height is especially modified by children who are known or believed to have been bed-ridden from birth, and upon the vertical component of whose skull gravity has acted very little I measured the head height, length and width of 4 boys and 4 girls, all post adolescents, and who had been bed-ridden from birth. The mean head height ÷ length ratio was 75.9 ± 3.56 per cent for the boys and 74.5 ± 3.62 per cent for the girls, as compared with about 68.0 for walking children. Also, the mean ratio of head height to head width was for the boys 84.2 ± 3.27 per cent, for the girls 85.0 ± 2.9 per cent, while for the walking boys it was about 86. Hence in neither ratio was there clear evidence of a permanent deformation of the head as a result of special relation to gravity, though the mean head height/length ratio is in both sexes of the bed-ridden patients slightly above the mean (Table 5).

The best conclusion from all of these observations and experiments would seem to be that the form of the head, and of the brain, which must in early infancy largely determine that of the cranium, depends primarily upon genetic growth factors whose operations have been analyzed by Frets ('17, p 22), Schreiner ('23), Hildén ('25) and Abel ('34), but that the course of growth may be influenced by birth distortions (usually for only a few days) and by gravity (if it is allowed to work chiefly in one direction) The influence of these two factors is usually temporary although it is possible that the experimental modification of head proportions during the first 6 to 9 months may result in their slight permanent modification Finally, artificial deformation of the head, as practiced by various peoples during many months of infancy and childhood, may lead to permanent gross deformation

Summary —The cephalic index, percentage ratio of head width by length, after decreasing during the latter part of gestation to a low point of narrowness rebounds after birth, often through 10 points However, after 6 to 12 months the head begins to become more dolichocephalic again The first post-natal month is one of great diversity of index, probably owing to the variation in distortion that the head experiences in the process of birth The causes of the great increase in brachycephaly during the first post-natal year are uncertain There is some reason for thinking that the habit of sleeping on the back of the head may cause it in some cases. The most probable cause in the latter half of the first post-natal year is an increase in width dependent on the flattening of the brain and brain case that accompanies the acquisition of vertical posture.

In general, the head of the female is more dolichocephalic by about half a point than that of the male The idiot group is about 2 points more dolichocephalic than the standard The mongoloid is 2 to 5 points more brachycephalic than standard The Italian boys have the highest index and the American Negro the lowest

Individual variation in change of index with age is striking Generally the index diminishes, but in some individuals it increases during 10 years. Families usually show a characteristic trend of the curve, though at very different levels The same is true of identical twins, though the difference in level is usually slight—from 0 to 3 points. Microcephalics have usually a low index, cretins a high one; ateleotic dwarfs vary in their index

2. Head Height to Length

General —The height/length index of the head varies in different adult races of men from 60 to 77.5. Heads with smaller ratios (from 58 to 62.9) are called orthocephalic, with the larger ratios, hypsicephalic (Martin, '28, p. 798). In the anthropoid apes the mean percentage indices run from 62.9, Gibbon ♂ to 82.5 Orang ♂.

The mean change of the head height/length ratio with age is shown in Fig. 57. The pre-natal part is based upon a small number of heads of fetuses measured by me at the Department of

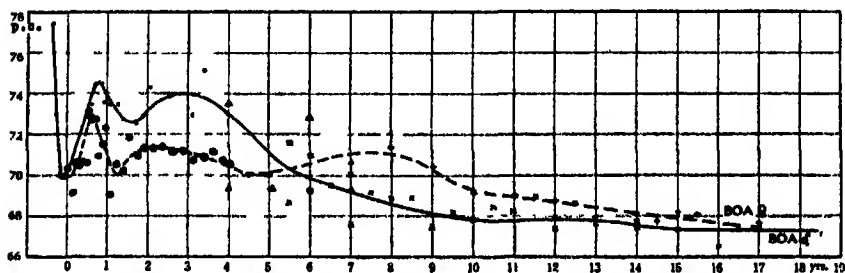


FIG. 57. Mean curves of change with age of the percentage ratio of Head Height to Head Length. Standard series. For meanings of symbols see Fig. 1.

Embryology, 1 or 3 at each month of development. At the fourth lunar month the head is still very high so that the ratio of height to length is over 80. The index falls as shown in Fig. 57 until at 5 months it is 78 (75.4), 6 months, 76 (75.2), 7 months, 77 (75.0), 8 months, 69.5 (75.5), 10 months, 70.4 (74.9), 1st month post-natal, about 70. The numbers in parenthesis are ratios of means computed from Scammon and Calkins ('29, p. 136). After birth the mean ratio rises rapidly to about 74.5 at 9 months, diminishes from 9 months to 20 months, rises to a maximum at about 3 years and then slowly falls, about 1.8 points p.a. at 4 to 5 years, then to 2.5 points p.a. at 7 to 9 years. Thereafter the change in the ratio is very slight.

What is the meaning of these post-natal changes in size of the ratio? Some suggestions may be hazarded. The pre-natal fall in the ratio is, of course, due to the rapid growth of the head length which increases about 40 per cent faster than the height. This is largely due to the especially rapid development of posterior lobe of the brain. The low vault of the head before birth may be a partial adaptation to the form of the birth canal. After birth the head height grows relatively even faster than head length for a few

months as though to make up for the earlier retardation and thus the mean index reaches the infantile maximum at 9 months. Even before this the growth of the absolute head height has begun to slacken. From a rate of 80 mm p a at birth to 3 months there is a fall to 10 mm p a at about 15 months. It seems probable that this slump may be due to the action of gravity, flattening the brain, as the child begins to go with its head up, as we have seen at page 31. The action is progressive and is not completed until about 21 months of age.

After 21 months and until 35 years head height grows at a higher rate than head length as if again to make up for lost time and the mean index rises to 74 p.c. After about 3 years the head height becomes absolutely diminished and accordingly the height/length ratio diminishes very rapidly until about 10 years of age at which time the downward slope of the ratio ends. However, beginning at 13 years, the time of the beginning of the adolescent spurt in growth and as the frontal sinuses are enlarging, the slope of growth in head length seems again to exceed that in head height and the ratio declines for a year or two. The union of the bones of the skull, with obliteration of sutures begins from 5 to 15 years later, at between 20 and 30 years of age in the male. Schwerz ('10, p. 31) noted this decrease in proportional head height in the children of Schaffhausen. Saller ('30, p. 80) found a decrease in the height + length ratio from 11 years in Fehmaren Islanders and Keiter ('33, p. 347) in Schleswig Holsteiners.

Sexual.—Since the weight of the adult female brain is about 10 per cent less than that of the male (Vierodt, '06, p. 76) it is to be expected that the changes in growth of diameters and perhaps ratios will be less than in the male. We have seen (Fig. 15) that after birth the growth of head height is markedly less in the female than the male, while there is a less striking difference between the sexes in growth of head length. Accordingly the position of the head height/length ratio in the female lies always below that of the male from birth until 5 years (Fig. 57). After 5 years, however, the female age-line of ratios decussates with that of the male in an extraordinary fashion. The difference between the sexes in this ratio after 6 years is apparently due to the fact that the frontal sinus develops rapidly at about 6 to 7 years (Davis, '14, p. 133). Of the frontal sinuses, Terry ('33, p. 141) states "They are larger in the male than in the female." Perhaps the greater enlargement

of these sinuses in the male is one reason for the greater growth of head length in the male and for the further fact that the height/length ratio at 6 to 9 years falls in the male so far below that of the female at this time when the mean head height of the male is actually getting less (Fig 15) while that of the mean female is increasing. Is it possible that the formation of the frontal sinuses interferes temporarily with growth of head height?

Eventually, or at about 17 years, the head height/length ratio comes to be the same in the two sexes. This result is very different from that found by Scherz ('10, p 30) for Schaffhausen children, where at 17 years the ratio is much greater in girls than boys.

Social—A comparison of the standard Nordic males (BOA) with Nordics of lower degree of intelligence shows that in the head height/length index at 12 years the mean BOA group lies at the bottom, with an index of 67.8, the Idiot and LVD groups lie higher up and the Mongoloids occupy much the highest position, at 71.5. Obviously this is the consequence of the short heads of the Mongoloid dwarfs.

In all cases the index slopes downward before puberty, although at varied gradients, pretty steadily with age.

Racial—The Nordics show the highest index, from 70.3 to 68.5, and the Negroes the lowest, 67.9 to 67.5. Thus the Negroes have the longest and lowest heads. The Mediterraneans are intermediate. In all cases there is a loss of the size of the index with age of about 2 points per a. Saller ('30, '31) finds a similar decrease in the case of north Germans, among whom at 16-19 years the index is 68 or 69 in males and 1 or 2 points higher in females.

Individual—Figure 58 gives individual changes in the head height/length index for 4 babies. The range in position at a given age, say 4 months, is very great, from 67.0 to 79.8. The changes in the first two or three months are bewildering, but since many repeated measurements are available the details of the curves probably are significant. In three cases the index falls from birth. In No. 1 the fall is during one month only, in Nos. 3 and 4 during three or four months. In No. 2 the index begins to rise directly after birth and continues to 2 months. The first year maximum is shown in No. 2 at three months; in No. 4 at $4\frac{1}{2}$ months, in No. 1 at four months; in No. 3 at $8\frac{1}{2}$ months.

The rapidly occurring and large changes in the head height/length ratio during the first 1 or 2 post-natal months are a recovery from

an adjustment of the head form to the birth canal, a recovery in which genetic factors have a chance to show themselves. Thus Nos 2 and 4 have, at birth, indices between 68.5 and 71.5, or 3

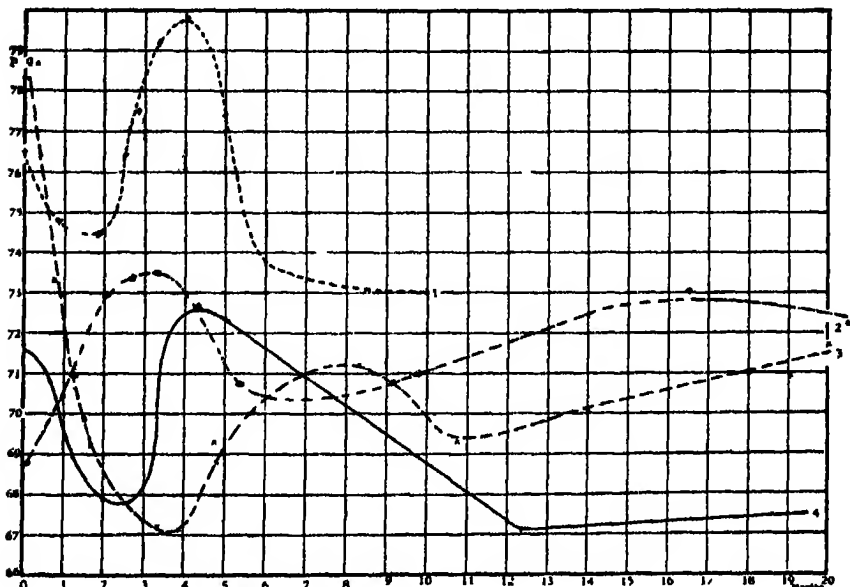


FIG 58 Individual curves of change with age of percentage ratio of Head Height to Head Length for four babies—from birth to 20 months 1, L L m, French, 2, A L f, French, 3, C C f, U S, 4, R N f, U S

points, but at 12 months their indices are five points apart. The action of gravity in flattening the head appears in Nos 3 and 4 and perhaps 1 and 2 also (at 6 or 7 months).

Figure 59 gives the change of the index of height to length in the course of years for eight boys. The range is at 13 years great—from 60.5 to 73.2, or 12.7 points. Different types of curves can be distinguished. Upslope on the one hand and clear downslope on the other at around 16 years are marked.

Familial.—Figure 60 gives the changes in the head height/length index for 10 members of the Mea. fraternity. The curves are mostly U-shaped, the minimum usually located at about 16 to 17 years. No. 6 offers a curious irregularity at 16 years. The meaning of this irregularity is uncertain. Since it depends on three successive measurements taken within a year it can hardly be due to observational errors. In this family there is a clustering, at 16

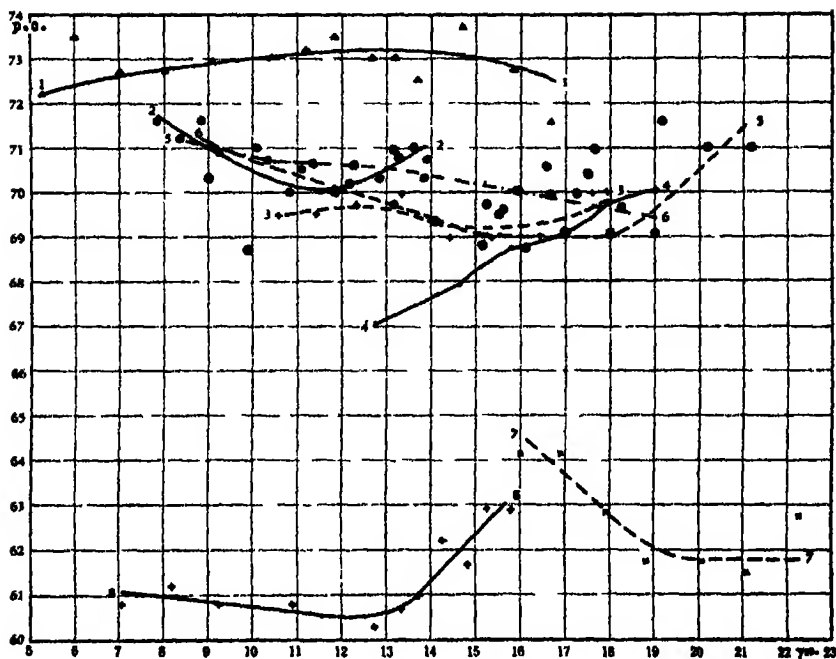


FIG 59 Individual curves of change with age of percentage ratio of Head Height to Head Length for eight I and LVD boys 1, CH No 83, 2, J M No 129, 3, WE No 15, 4, P M No 10, 5, C D No 30, 6, M H No 2, 7, W W No 13, 8, G H No 82

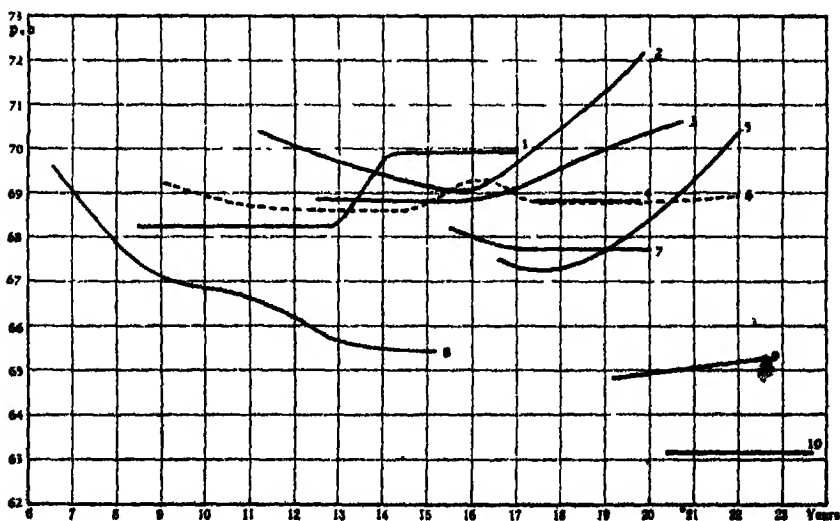


FIG 60 Individual curves of change with age of percentage ratio of Head Height to Head Length in Mea fraternity 1, R M f, 2, H M m; 3, N M f; 4, E M f, 5, H M m, 6, G M m, 7, Ma M f; 8, B M f; 9, L M m, 10, Mi M f

years, at around percentage index 69, but three individuals lie at about 65, or lower

Figure 61 gives changes in head height/length index in two other families. The upper set of curves is clustered around two

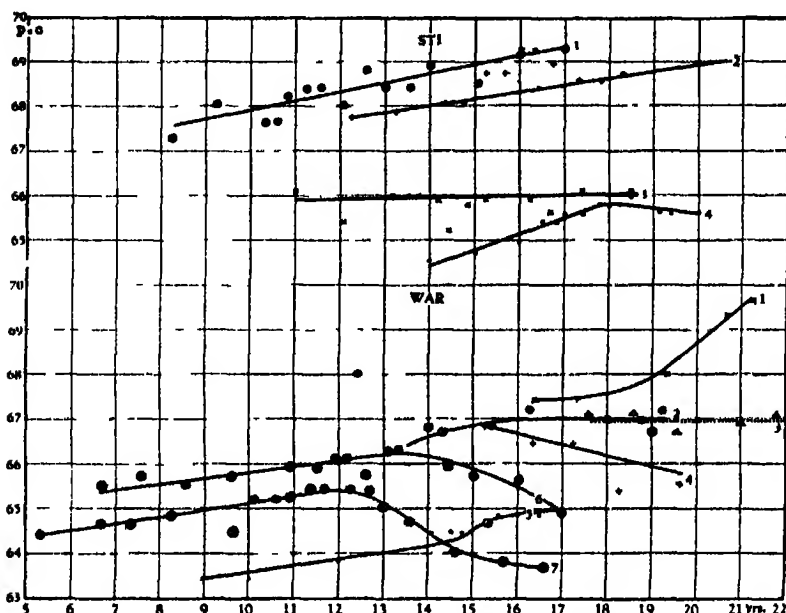


FIG 61 Individual curves of change with age of percentage ratio of Head Height to Head Length in Stt (above) and War (below) fraternities. Stt 1, M S f, 2, I S m, 3, E S f, 4, C S m. War 1, Fd W m, 2, Fk W m, 3, B W f, 4, W W m, 5, M W f, 6, S W m, 7, G W m. Two sets of ordinates

indices that, at 16 years, are near 68.5 and 65.5 respectively. In this family all indices are increasing through adolescence and puberty.

Below are shown curves of the War family. In this family the indices increase to 12, sometimes to 15 years. Of the 7 children the indices fall, in three cases, during puberty, in the other four they are rising or remaining constant. There is perhaps a segregation into two types.

Twins—Figure 62 gives growth changes in four pairs of twins of which the Gar., Ols and Sha are probably monozygotic. Comparing the changes in the indices shown by members of the pair it appears that they usually run parallel but one or two points apart. Especially the lengths of the head of monozygotic twins may differ, even while the heights are closely similar. The Dot

twins are colored girls, certainly dizygomatic. Their indices differ by about 6 points.

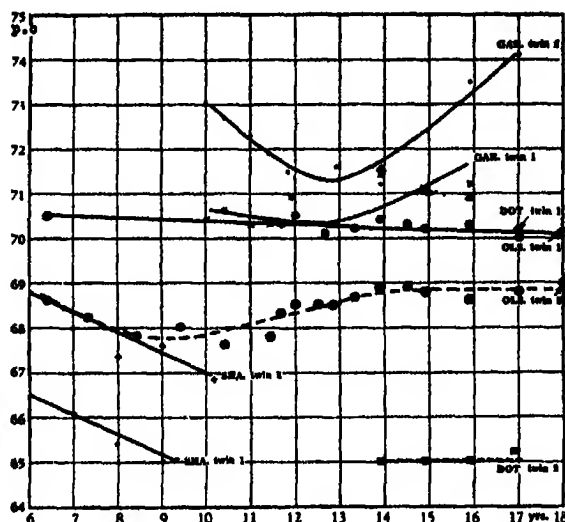


FIG 62 Individual curves of change with age of percentage ratio of Head Height to Head Length in twins Dot twins, dizygotic; others monozygotic

Special Cases—Figure 63 shows growth changes of the head height/length index in a variety of more or less pathological cases. The extremes of the cases given lie at 62.5 and at 78, both dwarf girls of somewhat different types. No. 4 is also a dwarf who in middle puberty was growing rapidly—a case of retarded development. V.R. is the case of a microcephalic boy, whose head shape seems to have undergone quite marked changes during adolescence and puberty. No. 5 is the case of a cretinous girl with a fairly constant ratio.

Summary—The changes in the head height/length index have hitherto been insufficiently attended to, partly on account of difficulties of technique. The study of the changes of the ratio in masses and individuals reveals, first, great changes during the first post-natal year; perhaps associated with adjustment to the processes of birth or, in part, to deformation during parturition. Gravity plays an even more important part in this index than in the cephalic index, since gravity depresses the vertex in the standing infant.

Of the social groups, the short headed Mongoloid dwarfs have the highest index, the standard boys the lowest. Negroes have a

consistently lower index than whites. Individual babies show rapid changes in the height/length index, however, the chance for error in making measurements is large. The range of the index in older children is very great, even in children of one race and kept

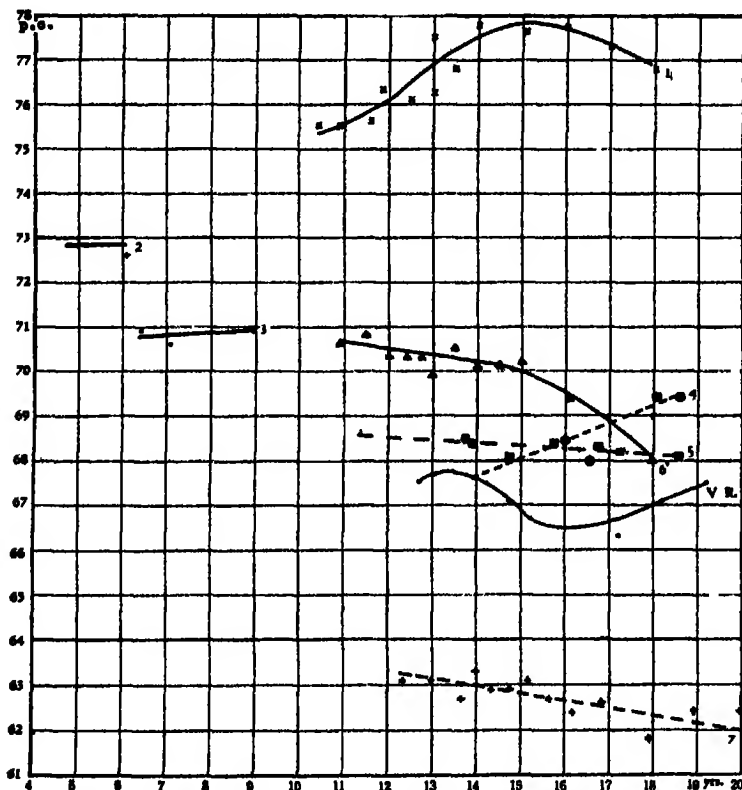


FIG 63 Individual curves of change with age of percentage ratio of Head Height to Head Length in eight special cases 1, L S f, achondroplastic, 2, H L m, cretin, 3, A A f, cretin, 4, M W f, ateliotic dwarf, 5, C B f, cretin, 6, M C f, dwarf, 7, A W f, ateliotic dwarf One set of ordinates

at one institution. The variation seems not to be closely correlated with intelligence. In families the index may vary greatly, but usually there is a clustering about two points at late adolescence, suggesting segregation. In twins the changes of the index run parallel, but do not coincide. Ateliotic and other dwarfs show a great range in the index.

3 Head Height to Width

We have already noted the irregularities in head height in infancy. We have now to consider the ratio of head height to width.

Sexual.—Figure 64 gives the curves of the mean changes from mid-gestation to near maturity. Starting in mid-uterine life with

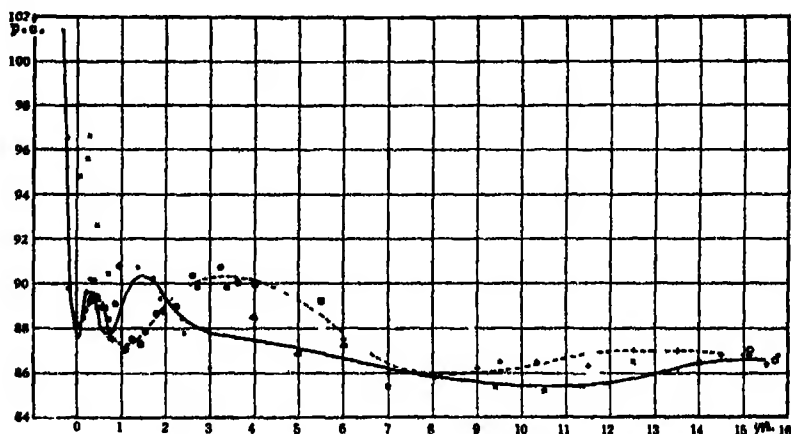


FIG 64 Mass curves of change with age of percentage ratio of Head Height to Head Width in standard series For significance of symbols see Fig 1

a mean percentage ratio of 106, the ratio falls rapidly as the head width (Fig 9) increases faster than height (Fig 15). At birth the percentage ratio is about 88. The relatively great head height at mid-uterine life is well figured in Stratz ('22, Fig 32) where the height at the fifth month of gestation is over $\frac{1}{4}$ total body (crown-sole) length. Immediately after birth the mean male ratio begins to increase again due particularly to rapid increase in head height. The result is that by the first quarter of the first post-natal year the mean percentage ratio has risen to 89, or perhaps 90. After that the mean percentage ratio begins to decline acceleratively to about 8 months. Then it rises rapidly to about 18 months and falls more or less steadily into the adolescent period.

The curve of changes in the female mean percentage ratio runs in part parallel to that of the male, but shows some significant differences. The female ratio is about 2 points above the male at birth, and doubtless there is a difference for some weeks before. The ratio rises during the first post-natal years as in the male, but the maximum is slightly retarded. There is a similar depression

at about 12 months—2 or 3 months later than in the male. Thus there is a I decussation with the male curve at 6 months and a II decussation at 9 months. From 12 months the female curve rises to a maximum of 91 at 36 months and then slopes downward. There is thus a III decussation with the male. It occurs at 24 months. After exceeding the male curve by about $1\frac{1}{2}$ points for 3 or 4 months the curve of the female descends to the male level at $7\frac{1}{2}$ years, after which the curves part again.

It will be noted that the preadolescent minimum occurs in girls at about 8 years at 86 per cent, in boys at $10\frac{1}{2}$ years at 85.4 per cent. This age difference agrees with the difference in onset of the adolescent spurt in the sexes. It is of interest that axial dimensions, more than transverse, are so generally affected by those hormones that are so largely responsible for trunk and leg growth.

From Saller ('30, p. 82) it appears that girls more frequently than not have a larger height-width index than boys, but this is not so true of the Probstes ('31, p. 242).

Social—The standard (BOA) series occupies a fairly low position, especially at ages 8 to 11 years. The LVD_I group lies somewhat lower still after age 12. The Mongoloid and Idiot groups lie fairly high, though the LVD_{II} series is still higher. At age 12 the sequence from below upward is LVD_I, BOA, M, LVD_{II}, and I. A pre-adolescent spurt appears in 4 of the 5 series at 10, 12, 13 and $14\frac{1}{2}$ years respectively. The M series shows no inflection. The minimum in the curve is in general retarded with increasing mental deficiency and this fact is paralleled by the retardation in the adolescent spurt.

Racial—In general the U. S. and North European group lies above the Mediterranean. Perhaps on account of the small sample the Negro curve cuts across the other two, rising to 86 per cent at 14 years, as compared with the Nordics' 85 per cent at this age.

Individual.—Figure 65 shows the growth changes in the head height/width percentage ratio in 5 infants. In some cases, like Barbara B. and Jas. C., there is a rapid change of slope within a few weeks after birth, which is perhaps due to readjustment after birth distortion. In both cases the percentage head height/width ratio rises. Evidently the birth pressure tended to flatten the head, even though, in anticipation of birth, the index was already low.

The rapid decrease in the ratio from 4 to 12, or even 16, months

(adjustment to vertical posture) is striking in every case. So also the rise in the ratio after the middle of the second year.

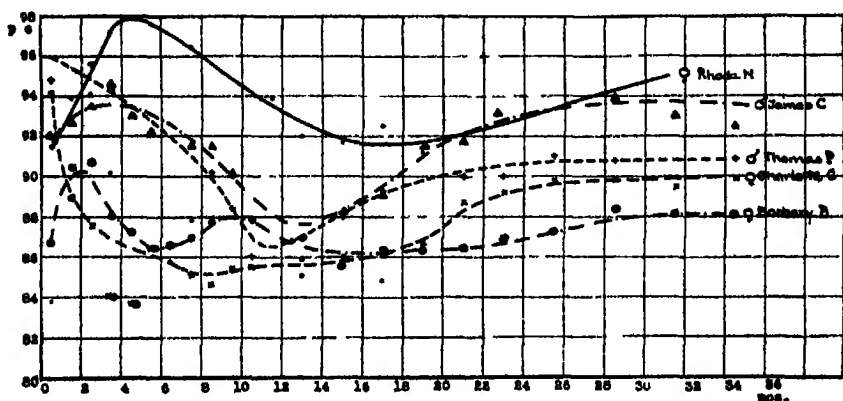


FIG 65 Individual curves of change with age of percentage ratio of Head Height to Head Width in successive measurements of five babies as listed Abscissae in months

Figure 66 shows age changes of 8 feeble-minded individuals, some (2, 3, 7, 8) of low, others (1, 4, 5, 6) of high grade of intelligence and general development. Two of the low grades have the lowest crania and one of the high grades the highest All curves, except Nos 1, 7 and 8, rise as age increases from adolescence onward. The gradient of this increase is very varied In some cases the change is proceeding even after 16 years

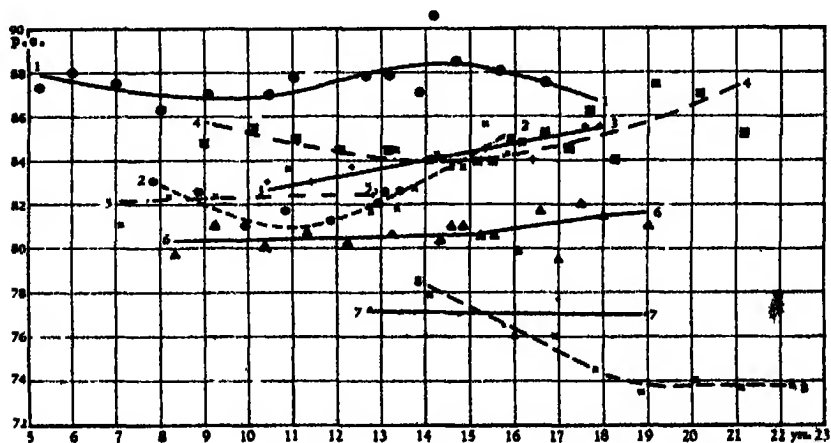


FIG 66 Individual curves of change with age of percentage ratio of Head Height to Head Width in eight boys of the LVD; and I series 1, CH No. 83, 2, J M No. 129; 3, WE No 15, 4, C D No 30, 5, GH No 82; 6, M H No 2, 7, P M. No. 10, 8, W W No 13

Familial.—Figure 67 gives curves of ratio changes in the head height/width ratio for 10 members of the Mea fraternity. The range is striking, from 81.5 to 90.3. An S-shaped curve (more or

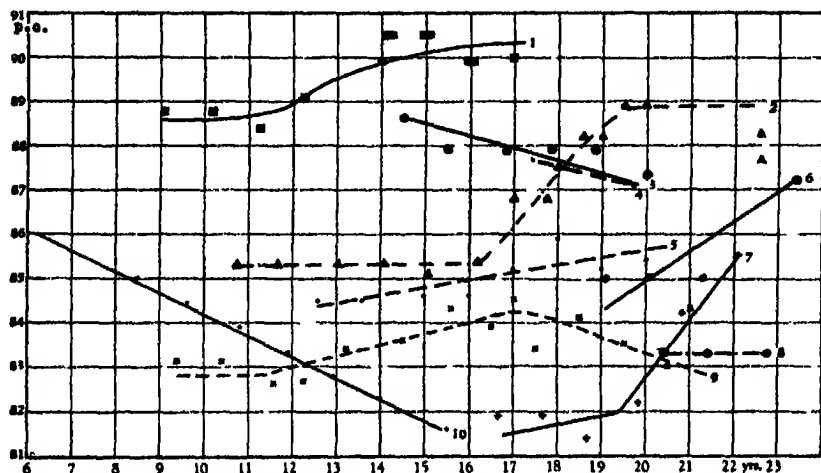


FIG 67 Individual curves of change with age of percentage ratio of Head Height to Head Width in 10 members of the Mea fraternity 1, R M f, 2, W M m, 3, Ma M f, 4, F M f, 5, N M f, 6, L M m, 7, H M m, 8, M₁ M f, 9, G M m, 10, B M f

less complete) is shown in Nos 1, 2 and perhaps 7. Nos 3 and 4 practically coincide. In some cases change of proportions ceases after 20 years, in others (Nos 5, 6 and 7) the slope still is upward. In one case (No 9) it is clearly downward.

In Fig 68 are shown the curves of the Sti (Nos 1–4) and War (5–11) fraternities. The former are mostly grouped at the upper part of the chart, the latter at the lower. There is not much difference in mean intelligence in the two families. Nos. 2, 3 and 4 show similar S-shaped curves. Nos 7 and 8 almost coincide. Nos 9 and 11 are close together until 17 years. No 9 runs a parallel course to 7 and 8 at a lower level.

Twins.—In Fig. 69 are shown the curves of 4 pairs of twins, all monozygotic except the Dot. twins. In the monozygotic twins the pairs of curves are close together at some one point, although there may be a difference at one age of 2 or more points. The form of the head of monozygotic twins is apparently less exactly determined by genetic factors than the form of the nose.

Special Cases—In Fig. 70 are shown the age changes in certain special cases. The range of the ratios is about 15 points. Nos 1, 4 and 7 are dwarf girls as indicated in the description of the figures.

Nos. 6 and 7 are certainly remarkable in the low position of the ratio, 79 per cent Nos 3, 5 and 6 are cretins These curves lie at strikingly different levels. No 2 is the curve of an extreme microcephalic, the head width is exceptionally small



FIG 68 Individual curves of change with age of percentage ratio of Head Height to Head Width in the Sti and War fraternities 1, MS f, 2, LS m, 3, GS m, 4, ES f, 5, Fd W m, 6, CS m, 7, MW f, 8, SW m, 9, Fk W m, 10, BW f, 11, WW m

The mean height/width percentage ratio of 9 mature microcephalics is 86.9, of 7 mature microcephalic females is 82.1. The first mean does not differ from that of standard mature males, the latter is some 5 points below standard for females

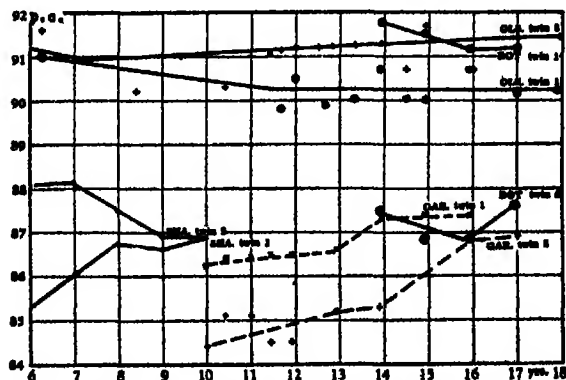


FIG 69 Individual curves of change with age of percentage ratio of Head Height to Head Width in 4 pairs of twins Dot dizygotic, others monozygotic

Discussion —The decrease of the height-width ratio to birth may be a temporary adjustment to the birth process by keeping the cranium low, thus facilitating the standard (λ -parietal)

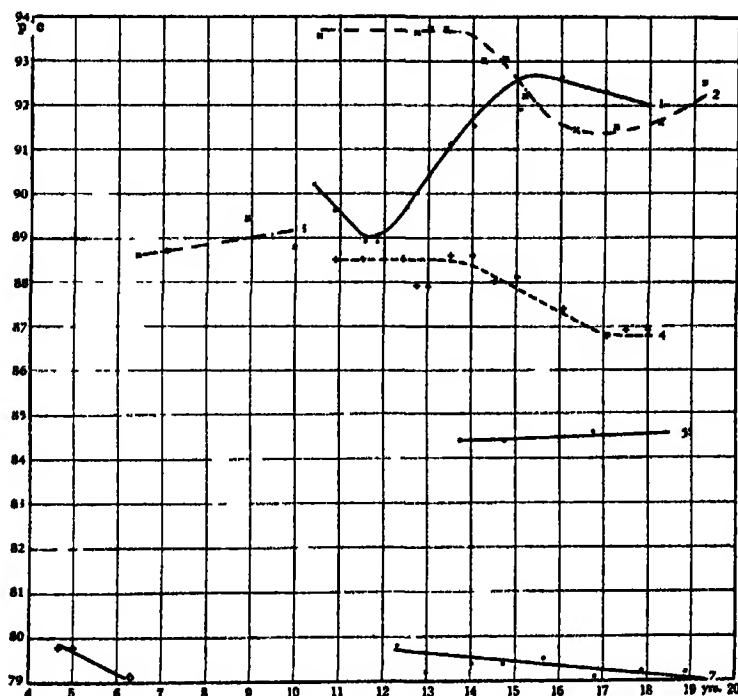


FIG 70 Individual curves of change with age of percentage ratio of Head Height to Head Width in some special cases 1, L S f, achondroplastic, 2, V R m, microcephalic, 3, A A f, cretin, 4, M C f, dwarf, 5, C B f, cretin, 6, H L m, cretin, 7, A W f, ateliotic dwarf

presentation Immediately after birth this ratio of the head rebounds, as it were, to a form which is determined by the interaction of heredity and environment Of the latter the most obvious factors are pressures by gravity acting on the head as the child lies in bed These, whether applied at occiput or laterally, would tend to cause an extension of the axis of the head perpendicular to the line of pressure, i.e., to an increase in height After 7 post-natal months the baby begins to sit up more or less and at 10 months to walk. Now, gravity acts along the vertex-basion axis of the cranium and this tends to depress the plastic brain and its largely membranous case and causes the case to expand laterally This, the period around 10 months, is one with a reduced head

ratio But again, as the cranial bones come to replace the membranes and to harden, the effects of gravity are diminished and during the following months the genetic factors become more efficient and more largely control the form of the head—especially making it more compact, narrower and higher After 3 years the head tends to become relatively wider, perhaps influenced by the pull of the jaw and neck muscles And this change is more marked in boys than girls, perhaps because they use the muscles for harder work Finally, as adolescence sets in with the spurt of growth, the vertical dimensions, even of the head, become like the vertical dimensions of other organs, increased more than transverse ones More detailed references to the effect of muscular pull on head changes may be found in Toldt ('14, p. 306), Hauschild ('26, p 163), Saller, Guthrie, Kohl and Schiereck ('33, p 80)

Summary —The total curve of change of head height to width has a number of irregularities due to adaptation of form of head to that of birth canal, to its reaction to gravity, to effect of muscular pull on head width, to differential hormone effects This ratio is greater in north Europeans than in Mediterraneans Individual babies show marked change in the ratio during the first 2 months, probably due to relief from birth pressure. The variability even in childhood and adolescence is very great even inside of one fraternity, but different families differ greatly in mean ratio at a given age. Age changes of twins tend to run close together. Some types of dwarfs have a low ratio, cretins a variety of ratios, a microcephalic has a high ratio

4 *Relative Head Length*

General —This ratio is the percentage of maximum head length to stature. It relates head length to general body size

This ratio is clearly important in quadruped mammals, such as a whale, or a mole, or even some dogs (like pointers), in which head and trunk lie in the same axis. In many other mammals, including most primates, the axis of the head forms an angle with that of the vertebral column. In man the relation of head length to total stature is less significant than in lower mammals and hardly comparable For human stature includes the leg length, while relative head length in lower mammals is in relation to trunk and head only, more nearly comparable to sitting height. But even with human sitting height we can not well measure length of trunk to end of snout, which is ordinarily taken in the lower mammals as the total body length.

Relative head lengths have been published since about 1894 (Koganei), or earlier. In very different parts of the world the mean length of the head of the mean adult man is between 1/10th and 1/8th of stature. And this fraction diminishes roughly as stature increases, but the short Japanese have a low ratio. Saller ('28, p. 97) affords some data on age-change in relative head length, ages 13 to 17

Sexual —The relative head length was not obtained for fetuses since total length is rather difficult to measure. At birth the index is about 23.5 per cent (Fig. 71). It descends rapidly, at the rate of

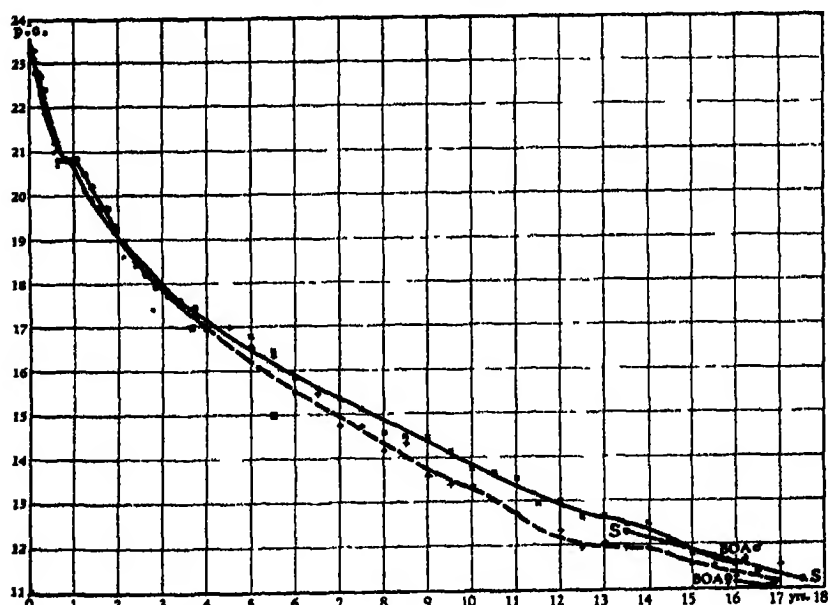


FIG 71 Mass curves of change with age of percentage ratio of Relative Head Length Standard series Symbols as in Fig 1 S, Saller ('30), Fehmaraner, males

3.6 points p a to about 10 months, at which time stature is taken standing (and accordingly reduced). After 2 or 3 months stature has overcome the gravitational effect and the reduction of the index continues, as the legs increase in length. The slope during the 2nd year is 1.8 p a, during the 3rd year 1.3 p a. The changes in slope naturally follow closely and inversely those in speed of growth of leg. Accordingly there is a slowing up of the reduction at 7-8 years (juvenile spurt of stature) and at 12 to 13½ (adolescent spurt). The decrease continues to 16 years, but the index is

stabilized at about 19 years, for our children at about 10 to 11 per cent.

In the female the index begins to depart from that of the male at about 1 year post partum. At 1 to 2 years the curve of change lies above that of the male, but from 2 years onward the female curve is below that of the male, about $\frac{1}{2}$ point. This is true despite the lower stature of the female, evidently because of the still smaller maximum head length.

In Saller's ('31, pp 6, 7) series from 6 to 16 years, the mean female index is always less than that of the male, but it gets to be larger after 16 years, possibly due to greater mean retardation in the growth of female stature than the male, possibly to sampling

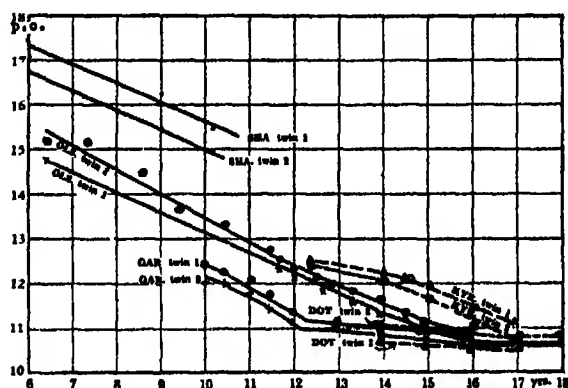


FIG 72 Individual curves of change with age of Relative Head Length in twins, all except Dot pair monozygotic. Dot and Gar female, others male

Twins.—In Fig 72 are given the curves of age changes in relative head length for the Sha, Ols, Gar, Kyr, and Dot twins, the last named being dizygotic. In all cases the curves of a pair lie parallel, but do not coincide. The difference varies from 0.5 to 0.2 point. Close lying parallel curves rather than those that are superimposed are characteristic of many dimensional ratios of monozygotic twins.

Special Cases.—In Fig 73 are curves of certain special cases. These all descend in typical fashion. High values are found for some dwarfs; for Nos. 3 and 4, achondroplastic sisters, at 22 years, nearly 14 per cent. Lowest is the curve of V R. a microcephalic with very short head. At 19 years he reaches a ratio of 10.5 per cent.

Summary.—The relative head length is not an important ratio, though it figures quite largely in the literature. It decreases during early life from 23.5 per cent to 11 per cent, as the stature increases. From 5 years the ratio is less for girls than boys, after

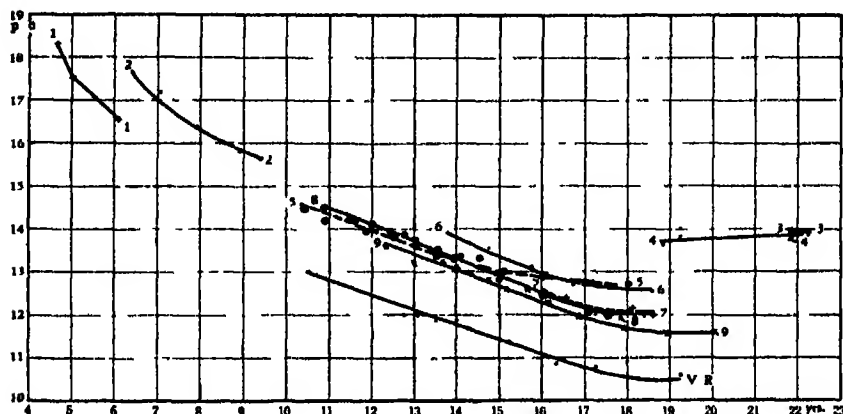


FIG 73 Individual curves of change with age of Relative Head Length in some special cases 1, H L m, cretin, 2, A A f, cretin, 3, K S f, achondroplastic, 4, L S f, sister of 3, achondroplastic, 5, L S f, achondroplastic, 6, C B f, cretin, 7, M W f, ateliotic, 8, M C f, dwarf, 9, A W f, sister to 7, ateliotic

adolescence less for standard children than for the feebleminded, less for Negroes than Nordic whites. In families, parallel paths are usually followed. In twins these are only 0.5 to 0.2 point apart. Dwarfs tend to have high ratios (short legs) and microcephalics low ratios (short heads).

5 Post Auricular to Maximum Head Length

General—This index of the proportional growth of the horizontal head distances behind the ear opening to total head length was found by placing the subject so that the occiput touches the table or wall, as the case may be, while the eye-ear and sagittal planes are held perpendicular thereto. Then the distances from the plane surface to trignon and glabella are measured in quick succession with the depth measurer. The points of error of these dimensions are, lack of precision in placing and keeping the head planes perpendicular to plane of reference, variation in pressure of occiput against plane of reference, variation in thickness of hair on occiput, variation in precision of locating trignon, deviation of staff of depth measurer from perpendicularity. The difficulty of holding the sagittal plane of head perpendicular was met by

measuring the tragon distance on both sides before measuring glabellar distance. If these two measures differ by more than 5 mm the head is to be readjusted. Otherwise the two measurements are recorded and their mean taken as tragon depth. Measurements taken at successive yearly or semi-yearly intervals in a way warn against other errors.

This ratio is one of great ontogenetic as well as phylogenetic importance, as is well illustrated in Martin's *Lehrbuch* in Figs. 313, 314. The proportion that the distance occiput to porion is of the distance occiput to glabella is more than twice in man what it is in the baboon. Even in relation to anthropoid apes the human proportion is strikingly great. Of course all this has relation to the development of the brain.

Neumayer ('08, p. 13) speaks of a change in the relation of the pre- and post-auricular parts of the skull. The new born has a long post-auricular part, but in Swiss, by 20 years, this relation is reversed. Neumayer's statement is hardly universally true. The mean percentage ratio of the post-auricular part to the total head length is 48.3 for 5 fetuses at term, although a few days (2-7) after birth 3 white Nordic girl babies that I measured averaged 50.1 per cent for the post-auricular section and 8 boys 52.7 per cent.⁹ The ratio rises quickly during the first 3 months to 54.5 per cent. Individual babies within a few days of birth showed an index of 49.2 per cent. On the average, after 4 months the post-auricular segment of the head tends to become relatively shorter for a while. It reaches a minimum of 50 to 51 during the third year. After that there is an increase in the post-auricular segment to over 53 per cent at about 4 years. Thereafter the ratio decreases again to a mean of 51.6 per cent at 12 years (Fig. 74). Only about 10 per cent of the post-adolescent children of the LVD₁₁ series showed a post-auricular segment of less than 50 per cent as opposed to Neumayer's 17 per cent of children, but our children are defectives, though of high grade.

With Neumayer's conclusion that the post-auricular fraction remains constant after the 9th to 10th year, I cannot agree, since, as Fig. 74 shows, on the average, this fraction is still increasing from 13 to 17 years, and in some individuals to 20 years or beyond. Keiter ('33, p. 347) finds a mean decrease in this ratio in his Schleswig Holstein male children from 46.7 ± 2.7 at 6-7 years to

⁹ Schultz ('15, p. 374) finds the percentage ratio of occipital-auricular segment to maximum skull length in neonates to be 51.6.

45.4 ± 2.7 at 14-15 years and to 43.6 ± 2.6 at 25 to 60 years. Females at corresponding ages showed indices of 48.0, 46.7, 45.2. It seems possible that inadequate sampling may be responsible for this downward slope

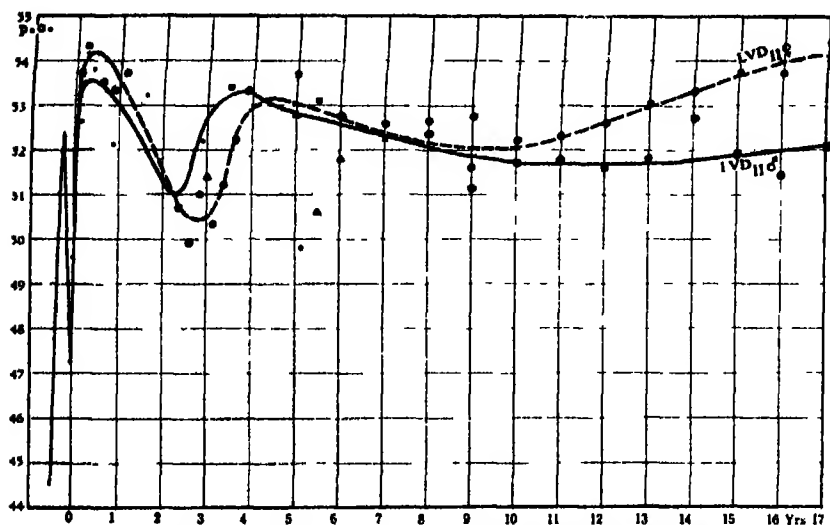


FIG 74 Mass curves of change with age of percentage ratio of Post Auricular to Maximum Head Length Standard curve, for fetuses, babies and Brooklyn Home series as far as 8 years, LVD₁₁ beyond Triangles, boys of Brooklyn Home, squares, girls of the same

Sexual—As Fig 74 shows, from a few days after birth a difference appears between the sexes in this ratio. The mean for boys increases after birth to 53.7 at 5 months, thereafter decreases to 51 at 2.5 years, then increases to 53.3 at 3.7 years, then slowly decreases to 51.7 at 12 years.

The mean percentage ratio of the girls rises above that of the boys at 5 months to 54.3 per cent, reaches a minimum of 50.5 at 2¾ years (about 6 months later than in the case of the boys), rises to 53.2 per cent at 4½ years or about a year later than the boys' and continues thereafter to be greater than the boys'. Schultz ('17, p. 53) finds the pre-auricular segment of head length in girls less than in the boys, which is in harmony with my findings. The difference between the sexes is very large after adolescence. I think a part of this difference may be due to the thicker hair over the occiput of the girls as contrasted with the boys.

Racial.—Since this ratio was not determined for the standard children we may as the next best series consider the differences

between the LVD_{II} races. The U. S. and Nordic groups have the relatively largest post-auricular head proportion (at 17 years, 52.4 p.c.) followed by Jewish and Italian. The Negroes have the relatively smallest post-auricular portion of the head length (about 51.7 p.c.) Since the mean head length of the Negro children exceeds that of the whites this means that the glabellar part of the cranium protrudes further anteriorly in the Negro than in the white children. Schultz ('15, p. 375) finds this ratio for adult Danes to be 47.9 p.c. for males, and 48.6 for females, and for adult Loango Negroes, 49.9 p.c. for males and 50.6 for females, a racial difference with which my results are not exactly in harmony.

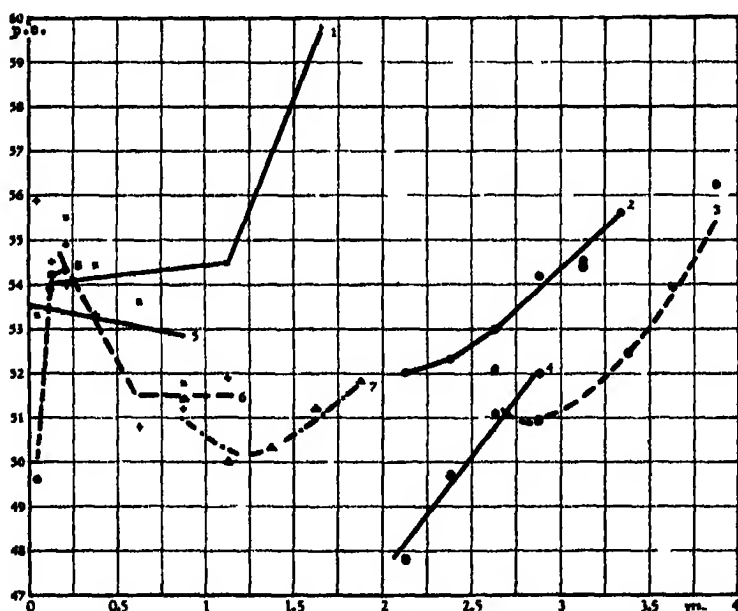


FIG 75 Individual curves of change with age of percentage ratio of Post Auricular to Maximum Head Length in eight babies 1, L L No 4, m, 2, A C No 19, m, 3, M K No 1, f, 4, J M No 25, m, 5, C D No 48, m, 6, P D No 47, m, 7, K H No 35, f, 8, B M No 52, m

Individual.—Figure 75 gives the curves of changes in the post-auricular segment for individual babies. They show that the slope upward, around the third year, is not merely a statistical phenomenon, but a biological one.

Figure 76 gives individual curves of changes in the post-auricular segment. Whereas the curves slope downward with age before 9 or 10 years, they generally slope upward thereafter,

demonstrating that the increase with age of the index after 10 years is not merely statistical but biological. In some cases the increase of the ratio amounts to 18 points p a The post-auricular segment

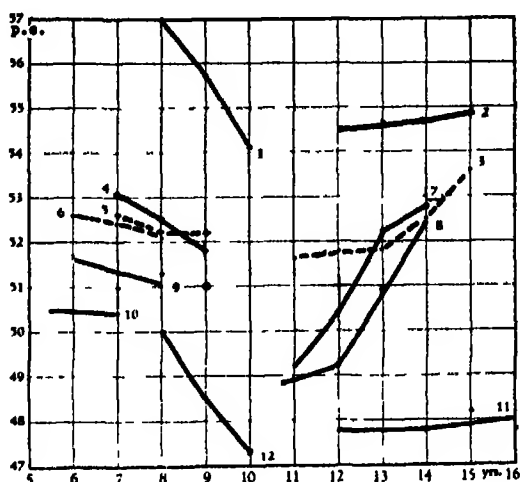


FIG 70 Individual curves of change with age of percentage ratio of Post Auricular to Maximum Head Length in 12 children of the LVD₁₁ series The cephalic index follows the reference number 1, S M f, No 318, C I 81.6, 2, L B col m, No 17, C I 79.3, A R f, No 89, C I 79.4, F G m No 309, C I 76.1, 5, H B f, No 301, C I 82.5, 6, A R m, No 334, C I 93.4, 7, N B f, No 11, C I 85.8, 8, P P m, No 81, C I 81.2, 9, J C m, No 331, C I 87.8, 10, F R m, No 339, C I 93.5, 11, J B m, No 9, C I 75.0, 12, L M f, No 317, C I 85.8

is increasing rapidly from 11 to 14 years, at the time of the spurt of growth

Twins.—In Fig 77 is shown the curves of age-growth changes for 5 sets of twins of whom all but the colored Dot twins are monozygotic. Noteworthy is the close relation of the curves of the Ols. and Gar. twins through most of their courses. The Sha twins run parallel courses, but a point apart. The courses of the curves of the Kyr twins cross each other in an unusual fashion. Of the dizygotic Dot twins the curves lie rather far apart.

Special Cases —Figure 78 shows the age-growth changes of some special cases. Above, 1 and 2 are cases of cretins. The relatively high post-auricular fraction of the cretins' heads (even up to 60 per cent) is striking. At the bottom is the curve of a microcephalic. At 12 years he has the remarkably low ratio of 47.8 per cent, but since 17 years this has been rising rapidly and is now 52.7 per cent. This case is characterized by late development of the post-adolescent rise. Nos. 3 and 4 are cases of ateliotic dwarfs.

Discussion and Summary—What is the meaning of the rapid rise in post-auricular segment after adolescence as shown in the individual curves of Fig 76? The rise comes in connection with the adolescent spurt May it be casually associated with that

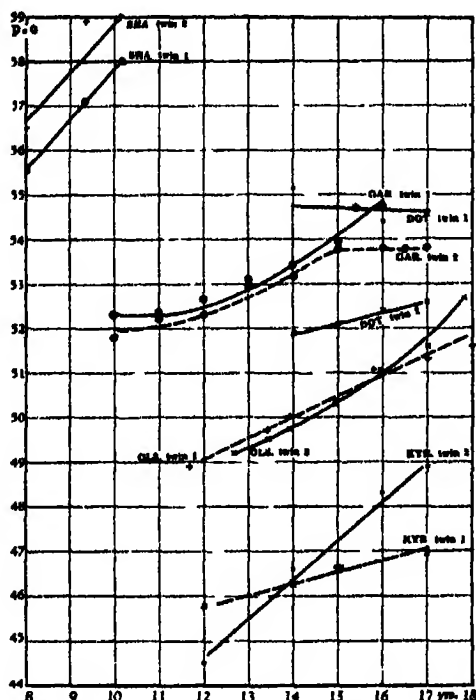


FIG 77 Individual curves of change with age of percentage ratio of Post Auricular to Maximum Head Length for 5 pairs of twins All except Dot are monozygotic Dot and Gar female, others male

spurt and with the necessity for a larger equilibration organ—the cerebellum—therein? Is this post-auricular increase due to enlargement of the cerebellum?

The objection to this hypothesis is that the cerebellum lies below the occiput But it is lodged in the occipital bone and its enlargement would tend to push the occiput backward from the foramen magnum Its growth might tend to elongate the temporal bone and account for the relative backward migration of the external auditory meatus with reference to the foramen magnum. The best series of weights of the cerebellum we have is that of Pfister ('03) This does not support the hypothesis, inasmuch as it indicates that the absolute size of the cerebellum increases only

slowly between 9 and 12 years, and its relative size does not increase at all.

Comparison of the hinder portion of the human pallum with that of even the highest anthropoid apes (c f Sonntag, '24) shows that the former has the temporal and parietal lobes much extended

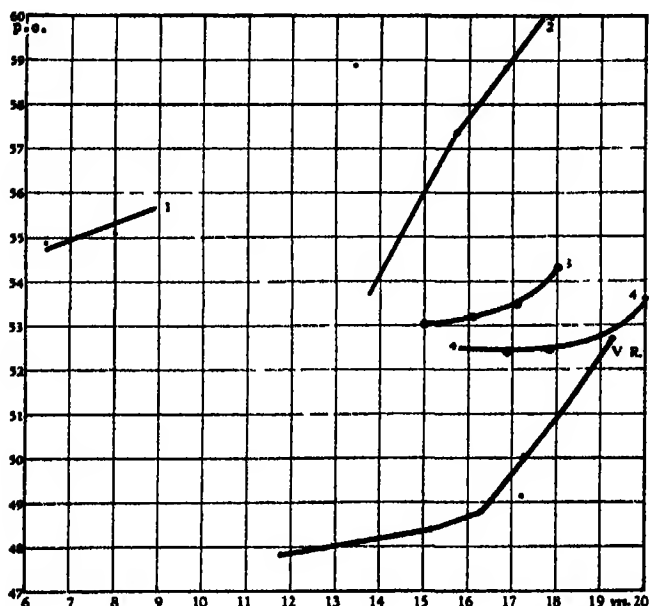


FIG 78 Individual curves of change with age of percentage ratio of Post Auricular to Maximum Head Length for five special cases 1, A A f, cretin, 2, C B f, cretin, 3, M C f, dwarf, 4, A W f, atelotic, V R m, microcephalic

toward the occipital region so that the portion posterior to the central and lateral sulci is greatly enlarged in man.

Whatever the explanation may be, the ear opening in the temporal bone does change its antero-posterior position on the skull during life, tends to move backward from 12 to 16 years, and downward as described at page 53. The post-auricular segment rises rapidly after adolescence despite the backward movement of the auricle. The post-auricular enlargement is, accordingly, the more striking.

6. Minimum Frontal to Maximum Head Width (Frontal Index)

General.—The minimum frontal width of the head is taken as described on page 31. The definition of head width is given on page 16. The problem is, in what relation does the minimum

frontal width stand during ontogeny to the maximum head width? Is it constant or does it vary relative to the width of the skull? This index has a considerable phylogenetic interest inasmuch as fossil man and the anthropoid apes have a relatively narrow minimum frontal width. The ratio is referred to by Martin ('22, p 199) as Transversal Fronto-parietal-Index (indice frontal). He states "The index expresses the relation of frontal development to parietal head width. It is, however, influenced by both dimensions and therefore by the cephalic index." Broca (teste Topinard, '94, p 249) compared the minimum frontal diameter and the maximum transverse diameter of the skull and found frontal indices varying from 68.0 in Parisians, 66.6 in Auvergnians and 64.8 in Javanese to 71.2 in Australian aborigines and 70.5 in Negroes.

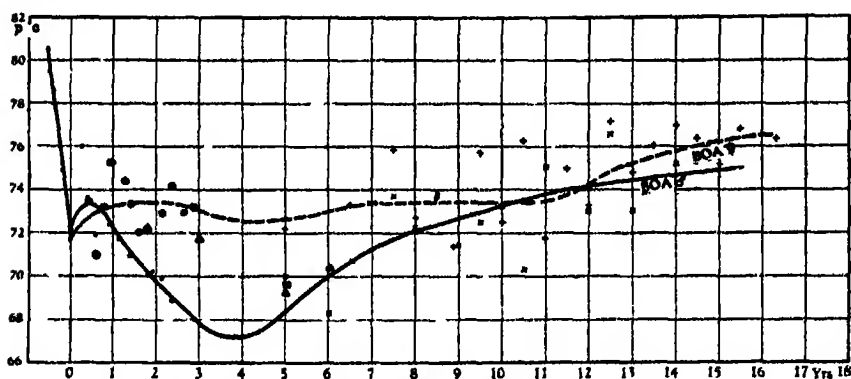


FIG 79 Mass curves of change with age of Transverse Frontal Index (Frontal/Maximum Head Width) Symbols as in Fig 1

The changes of this proportion with age are rather striking (Fig 79). At 20 weeks of gestation the ratio is a very high one—about 81 per cent. It thereafter falls steadily to birth where, according to my measurements of fetuses, it stands at 72 per cent. There is perhaps a marked rise during the first few months after birth to 73 per cent or more. I have actually found an individual with a mean of 76 per cent at three months.

The frontal index has been given for children by several authors. Saller ('30, p 88, 89) gives the most complete series for the Fehmaraners. His mean ratios are much less than mine. Thus, for boys at 6 years 66.8 per cent, instead of mine of 69.9 per cent. His values rise to 15 years (69.9) and thereafter slightly

decrease. He finds, as I do, the ratio usually, from 1 to 10.5 years, to be greater for the female

Sexual—During the first post-natal year there was not much difference in the mean ratio of the sexes, but after that a great difference appears in that the female ratio changes little to 11 years while that of the males undergoes a great depression at 4 years. During the period from 1 to 8 years the head width is decidedly greater in the male than the female (Fig 9), while there is no such marked difference between the sexes in minimum frontal width (Fig 23). The great head width of the male is associated with the loss of head height, after 3 years, which is more striking in the male than the female and may be associated with the greater activity of the male. After 10 years there is no obvious difference between the sexes in the index so far as my observations go.

Social—The standard group lies at the highest level. Of the remaining groups at age 16 the LVD_{II} series is above the I series, but the LVD_I series is below the others. This is apparently because the head width of the LVD_I series averaged greater than that of the I series and the minimum frontal remarkably less. In general the ratio seems to be positively correlated with intelligence of the group.

Racial—In our LVD_I series the U S and Nordic groups stood at the bottom and, after 12 years of age, the Negroes at the top. This is chiefly due to the relatively great frontal width of the latter race, especially after 12 years. The Italian and Jewish groups lie at an intermediate position.

Individual—Figure 80 shows the relation of the frontal index to age in 5 individuals of the LVD_I and LVD_{II} series. In general

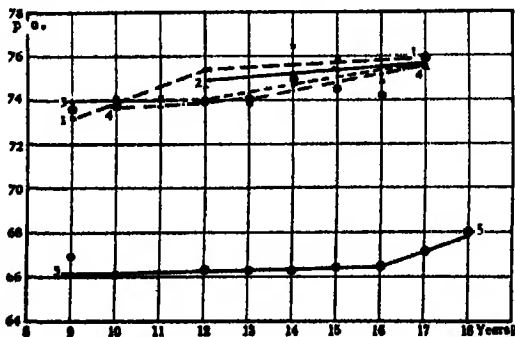


FIG 80 Individual curves of change with age of percentage ratio of Transverse Frontal Index for five boys of the LVD series 1, A C No 64, 2, M J No 55, 3, E C No 58; 4, V E No 59, 5, C H No 83

the slopes are upward, as in the mass curve. Whereas 4 of the curves are grouped close together, mostly within 2 points, the fifth individual, No. 5, is widely separated from the rest. As Fig 24, No. 7, below, shows, this boy was characterized as having small frontal development. His I Q is around 68. It is noteworthy that the slope of his curve is rapidly rising at the end, at a time when the slope of most curves is very small. There is here a strikingly retarded development, with a post-adolescent acceleration.

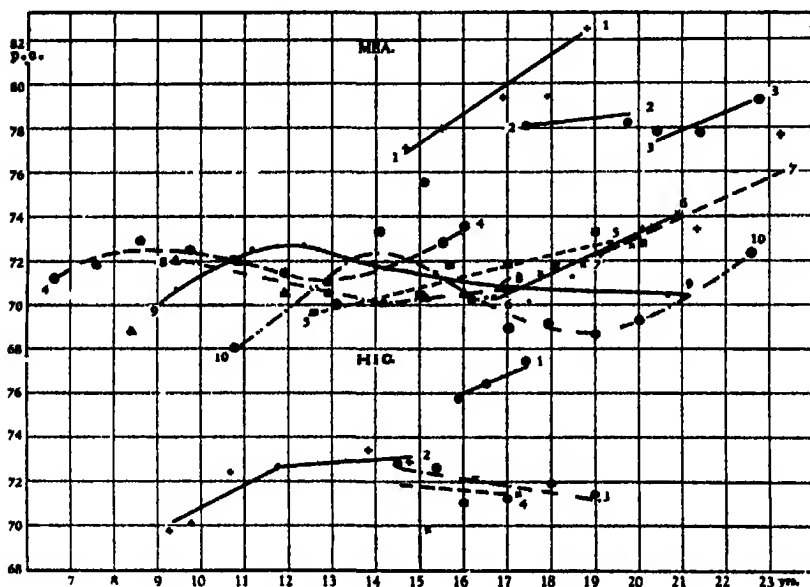


FIG 81 Individual curves of change with age of percentage ratio of Transverse Frontal Index for members of the Mea and Hic fraternities. Mea 1, Ma M f, 2, E M f, 3, M M f, 4, B M f, 5, N M f, 6, H M m, 7, L M m, 8, R M f, 9, G M m, 10, W M m. Hic 1, F H m, 2, Ch H m, 3, H H m, 4, Ce H m. Two sets of ordinates

Familial.—Figures 81 and 82 show the changes with age in the frontal index in four fraternities. The age curves of the frontal index of the Mea. fraternity of 10 White-Indian hybrid children are mostly grouped about 70 to 72 per cent. However, there are 3 aberrant children, girls, who show the higher index which is characteristic of the average girls much older than 14 years. Still another girl, No 4, is the uppermost at 16 years in the main group. Two of the boys, Nos 9 and 10, show interesting curves of a wavy type. In the case of No. 9 the growth of frontal width

was strikingly linear while that of head width was somewhat wavy. In the case of No 10 there is a marked change of slope downward at 16 years, which is associated with the unexplained slump in the minimum frontal curve beginning at that age (Fig 25). Nos 4 and 8, f, also show wavy lines of advance

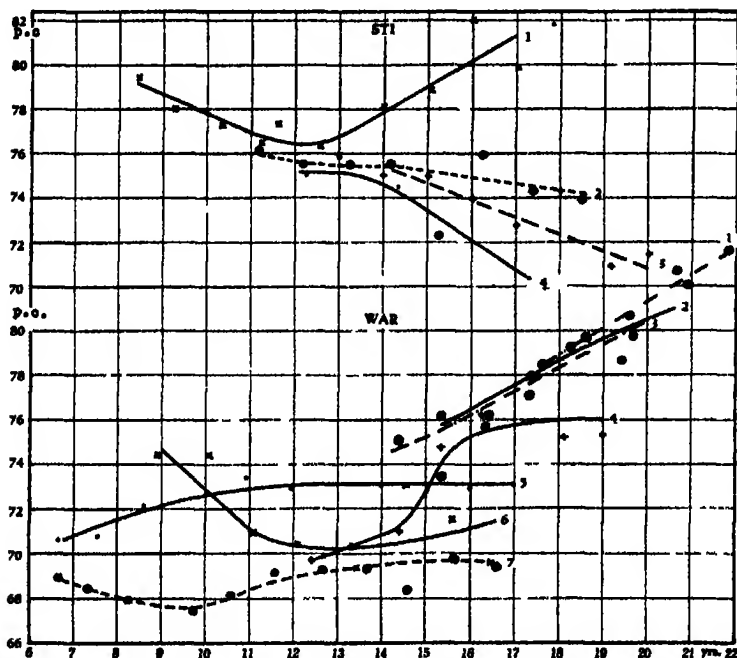


FIG 82 Individual curves of change with age of percentage ratio of Transverse Frontal Index for members of the Sti and War fraternities Sti 1, M S f, 2, E S f, 3, C S m, 4, L S m War 1, B W f, 2, F d W m, 3, W W m, 4, F k W m, 5, S W m, 6, M W f, 7, G W m Two sets of ordinates

Three members of the four of the Hic family (Fig 81, below) show a rather closely knit group so far as the frontal index goes.

The Sti fraternity (Fig. 82, above) show an unusual fanning out of the curves from 13 years upward While the course of the frontal width curve (Fig 25, No 4) of No 2 (E S) had little slope and has here a middle position the frontal width of No 1 (M.S.) advanced at puberty sharply and so does her frontal index Of L.S (No 4) frontal width was decreasing at puberty (Fig 25, No 3) and so is the index (No 4)

The War. fraternity falls into two groups (Fig. 82, below) In one group (2 boys and 1 girl) the frontal indices develop close together on parallel courses, and are still increasing at 20 years.

The brain case is undergoing a striking change in form at an age when cranial sutures have usually begun to enter upon the stage of fusion. These children, No 1, f, No 2, m, and No 3, m, showed a steady advance in frontal development even after 19 years (Fig 25). A brother, No 4, showed similar development. He, too, advanced rapidly in frontal ratio but a little more irregularly than the other three, partly owing to a non-linear line of advance in head width. The other 3 sibs, No 5, m, No 6, f, and No 7, m, had always comparatively narrow foreheads with median maximum head width.

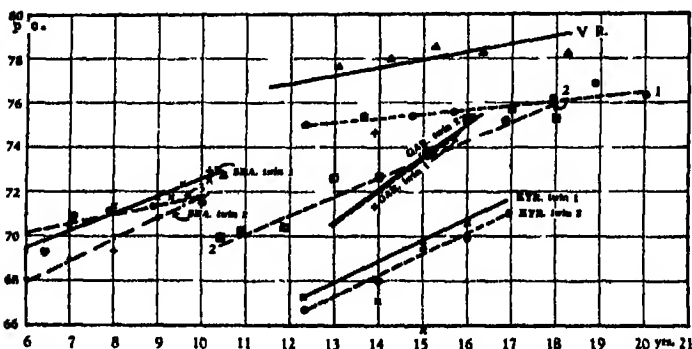


FIG 83 Individual curves of change with age of percentage ratio of Transverse Frontal Index in twins and special cases Gar f, Kyr and Sha m, monozygotic twins 1, M W f, atelotic, 2, L S f, achondroplastic, 3, A A f, cretin

Twins and Special Cases —Figure 83 shows the curves of change with age of 3 pairs of twins. The curves of the Gar. twins have practically the same locus. Those of the other two pairs run parallel courses, close together. The curve of the cretinous girl, No. 3, at the left, occupies a median position, as do also the curves of the other two dwarfs. The microcephalic, V R, lies at the top of the graph, despite his narrow forehead, because of his extraordinarily small parietal head width (Figs 26 and 14). Apparently his frontal development is less abnormally small than is the parietal width.

Summary --The frontal index, which has increased in man's phylogenetic history, tends to increase as the skull as a whole increases, ontogenetically from 4 years onward. The minimum frontal width increases from two-thirds of the parietal width to three quarters or more. It tends to be greater in girls than boys. Its size is roughly positively correlated with intelligence, yet in

children of about the same rather low grade of intelligence it is higher in Negroes than Nordics, because of the broad Negro face. Individuals show, in general, increase of the index with age. Families show segregation of curves, usually into two groups. In monozygotic twins the curves lie close together. In a microcephalic the curve runs high because of depressed parietal width.

7 *Minimum Frontal to Bizygomatic Width (Jugo-Frontal Index)*

General —This ratio gives the relation between the forehead width and the maximum width of the face at the zygomatic arch. It is the jugo-frontal index of Martin ('28, p. 677) who gives, as the probable error between two individual findings, 0.7 per cent. The index measures the relation between the maximum breadth of the face and the breadth of the upper face. The minimum frontal line marks the anterior limit of the attachment of the temporal muscle. This muscle runs to the coronoid process of the mandible inside, or medially, the zygomatic arch. The zygomatic arch finds its greatest importance in man as the line of origin of the masseter muscle which is attached to the ramus of the mandible. It is more powerful than the temporal. Thus the zygomatic arch is fundamentally important in chewing and its prominence (and hence the obliquity of the line joining the temporal line on the frontal and the middle of the zygomatic arch) is positively related to the degree of development of the muscles of mastication and the vigor of the process of mastication. Thus the Eskimo and Chinese have prominent zygomatic arches (broad face). It may be anticipated that the face broadens during development on account of use of the temporalis and masseter muscles, but more at the zygomatic arch than at the minimum frontal.

The pre-natal course of the jugo-frontal index is indicated in Fig. 84, based on measurements of about 3 or 4 white fetuses per month of each age from the 5th lunar month to birth. The ratio at 5 months is high, about 95 per cent. The jugal arch is little developed at this stage (Fig. 27), the face diameter at this level hardly exceeds that of the minimum frontal, but at birth it is prominent, 90 mm wide, only 40 mm short of its complete development. Also, the minimum frontal is small at birth and relatively quickly thereafter (at 4 years) acquires a relative size that is slightly greater than the adult's.

From about 9 years, with some change of slope, the ratio tends downward. Apparently as the child turns to food that requires

much mastication the muscles of mastication and their attachment in the zygomatic arch enlarge and the interspace between the parietal bone and the zygoma increases



FIG 84 Mass curves of change with age of percentage ratio of Minimum Frontal Width by Bizygomatic Width Standard series Also (S) male and female of Saller's '30, Fehmaraners

Sexual.—Figure 84 shows that the courses of change of this ratio with age in the sexes run approximately parallel. The female curve from 3 years of age lies always below that of the male as does also the minimum frontal. The bizygomatic width also is smaller in the female, but the sexual difference is perhaps not so marked as in the frontal width. At 15 years the mean difference between the sexes is not marked, and in the case of Letchworth Village children there is little sex difference in this ratio from 11 to 17 years.

Social.—The standard group has the highest jugo-frontal index and the curve slopes downward very little with time. The LVD_I , I and M series lie, at 17 years, at successively lower levels, but the LVD_{II} series lies below the others and forms a very smooth, albeit somewhat U-shaped curve. Since at 5 to 17 years the LVD_{II} group has a larger frontal index than the idiots and mongoloid dwarfs the bizygomatic width must be much greater than in the other groups (cf. page 38).

Racial.—Between the ages of $10\frac{1}{2}$ and $14\frac{1}{2}$ years, the Nordics lie at the highest level. This is largely because the minimum frontal width of the Nordic is the largest of all of our series while the bizygomatic width is mediocre. The Negro has the smallest index, because of his large bizygomatic width, despite the rather broad forehead.

Comparative.—The literature contains a number of references to this index in different races of mankind. Koya ('37, p. 88) finds an average for the adult full blooded Aino males of 75.61 ± 0.23 , females of 78.76 ± 0.18 . This group constitutes quite a

different type from our European stock with index of about 85. This low jugo-frontal index results from the prominent cheek bones of these north eastern Asiatics.

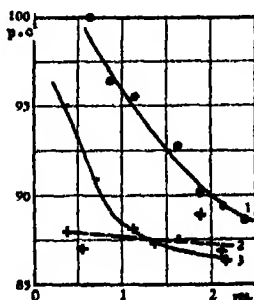


FIG 85 Individual curves of change with age of percentage ratio of Minimum Frontal Width by Bizygomatic Width in three boy babies 1, L D No 18, 2, A C No 19, 3, J P No 25

Individual—Figure 85 shows individual changes in this jugo-frontal index for 3 babies. The marked downward trend is shown in two of the three.

Figure 86 shows curves of change in ratio of a number of children between 6 and 18 years. The more extensive curves first rise to adolescence and afterward slope downward.

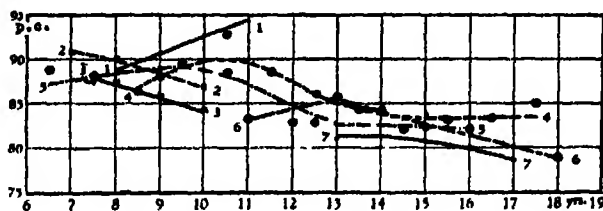


FIG 86 Individual curves of change with age of percentage ratio of Minimum Frontal Width by Bizygomatic Width in seven children of the I and LVD series 1, P S No 324 D₁₁ f; 2, F S No 320 D₁₁ f; 3, H B No 301 D₁₁ f; 4, C H No 83 D₁ m; 5, R O No 110, I, m; 6, C D No 30 D₁ m; 7, R B No 12 D₁₁ m

Familial—In Fig. 87 individual curves of changes in proportions are shown for 3 fraternities. While in each family the slope is downward there are one or more individuals in whom the ratio changes very slightly during a period of 4 to 6 years. In the upper and lowest family groups the trend with age is toward 80 per cent at maturity; in the middle family it is toward 77 per cent.

Twins and Special Cases.—Figure 88 gives the change in this index for two pairs of twins, for dwarfs and for a microcephalic

idiot The curves for the two pairs of twins run close and low since they have broad faces, that of the microcephalic occupies the lowest position owing to its small minimum frontal dimension.

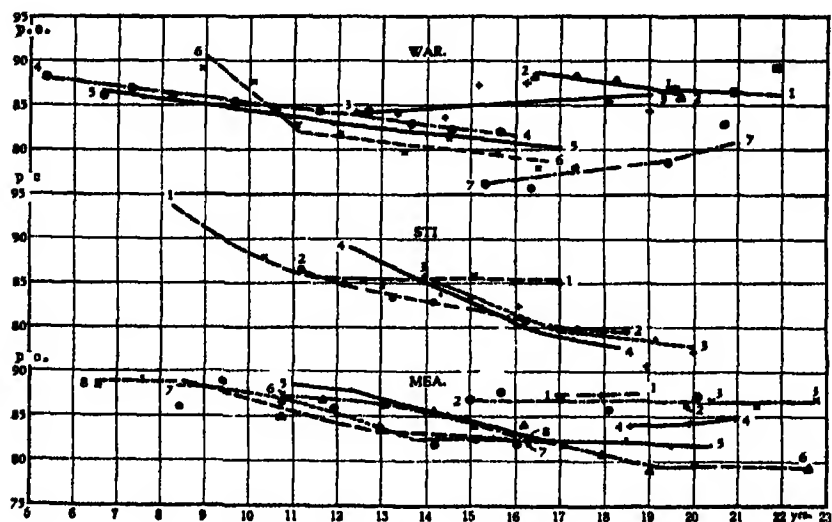


FIG 87 Individual curves of change with age of percentage ratio of Minimum Frontal Width by Bizygomatic Width in three fraternities War 1, BW f, 2, WW m, 3, Fk W m, 4, G W m, 5, S W m, 6, M W f, 7, Fd W m Stt 1, M S f, 2, E S f, 3, C S m, 4, L S m Mea 1, Ma M f, 2, N M f, 3, M M f, 4, H M m, 5, G M m, 6, W M m, 7, R M f, 8, B M f Three sets of ordinates

Mollison ('25, p 127) describes a similar small frontal (87 mm) in a microcephalic, Mezek.

Summary—Jugo-frontal index depends both on forehead width and zygomatic width The former is influenced by brain development, the latter by degree of mastication Doubtless internal, genetic, factors are most important of all Among the Nordics the more intelligent group has the highest index The index declines slowly but steadily with age, probably due to jaw use

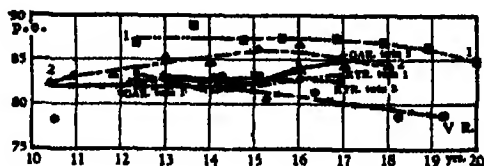


FIG 88 Individual curves of change with age of percentage ratio of Minimum Frontal Width by Bizygomatic Width in twins and special cases 1, A W f, ateliotic, 2, L S f, achondroplastic dwarf, V.R. m, microcephalic Gar. f, Kyr m, monozygotic twins

On the other hand one cannot maintain that the small index of the Eskimo is due solely to eating tough food (*e.g.*, blubber). The Siberians and Amerindians have a similar small jugo-frontal index, even though they eat cooked grains. Some of the Negroes of Africa, who are mainly agricultural, have a rather small index.

IV. CRANIAL CAPACITY

1. *The Growth of Absolute Cranial Capacity*

General.—The growth of cranial capacity is of interest both as a measure of the growth of the brain, which molds the form of the cranium, and on account of the cranium itself, which constitutes the major part of the head and determines its general form.

The determination of cranial capacity in the living is at best an approximation. Various formulæ for its determination have been proposed. That of Lee-Pearson is said by Martin ('22, p 197) and by Todd ('23, p 149) apparently to give the best results.

This formula has been criticized, however, and it is obviously not applicable to children of all ages. We have, however, computed it for ages 5 to 22 years to secure the best approximation available.

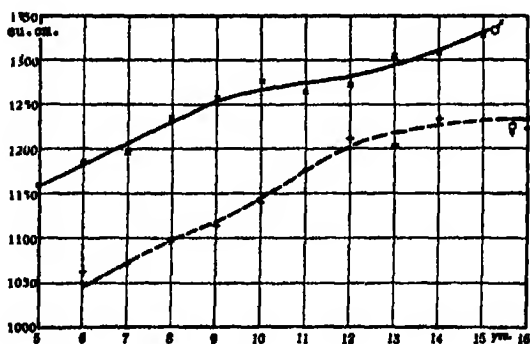


FIG 89 Mass curves of growth in Cranial Capacity Standard (BOA) series, m and f, 5-16 years

Sexual.—As Fig 89 shows, cranial capacity as measured by the Lee-Pearson formula increases, in the mean male, from about 1150 c.c. at 5 years to 1325 at 15 years, or 17.5 c.c. per year. The slope of the growth curve is not uniform, however. In childhood the positive gradient is about 25 c.c. per annum, in late juvenility cranial capacity increases only about 7 c.c. p.a. At the

beginning of puberty the rate of increase is about 22 c.c. p.a. Thus the curve of brain growth runs somewhat parallel to that of stature growth

The female curve, starting at about 1050 c.c. at 6 years, slopes upward to 1230 at 16 years, or about 18.0 c.c. p.a. During early juvenility the rate of increase is about 25 c.c. p.a. From 10 to 12 years the rate rises to 30 c.c. This corresponds to, or is a little earlier than, the girls' spurt of growth in stature.

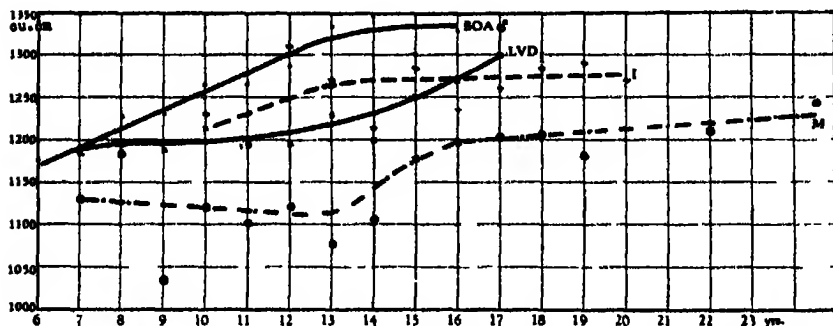


FIG 90 Mass curves of growth in Cranial Capacity in boys of four Social series, U S and Nordic Symbols as in Fig 3

Social—Figure 90 shows the mean growth in cranial capacity with age in 4 groups of varied intelligence. In all cases there is increase in cranial capacity with age, an advance that shows spurts of growth. The standard group has the greatest cranial capacity at all ages, and the mongoloid dwarfs, of extremely low intelligence, the least. At 16 years, and beyond, the LVD₁ series stand higher than the I series but earlier the relation is, strangely enough, reversed. On the average the intelligence of the LVD₁ series was greater than that of the I series.

Evidently a correlation between average cranial capacity and intelligence is present. In this respect our data harmonize with the detailed study made on this subject by Bachmeier ('28, pp. 53-56). Using a group of school children 6-8 years of age, and their school grades as measures of intelligence he finds indubitable evidence of a positive correlation between school grades and cranial capacity. A relation between size of head and intelligence has been noted frequently and findings to date of his publication are summarized in Martin ('28, pp. 751-755). There seems to be general accord among those who have measured head size or cranial capacity of groups of workers differentiated by the intel-

lectual quality of their work that the average head size of the more intellectual workers is the larger. Thus Venn ('88) found the mean head size of high honor men at Cambridge to range (in ages from 19 to 25) from 241 to 249 units; while means of candidates for the ordinary degree ranged from 229 to 244. The units are the product of length, width and height of head.

Racial—The mean cranial capacity as measured by the Lee-Pearson formula, which was originally worked out for the white race, is about the same for our Nordic and American Negro groups at all ages from 9 to 15 years. The mean of the Italian group lies about 50 to 100 c c below. The question arises whether the Lee-Pearson formula is equally applicable to the white and American Negro heads. This question has been studied by Todd ('23, p 178) and he finds, in opposition to Isserlis ('14), that the Lee-Pearson formula for white external dimensions does not fit well with the measured capacity of Negroes. The Lee-Pearson formula tends to give a result too high as compared with measured skull capacity. Using the most refined direct measurements Todd ('23, p 134) finds a mean capacity of 87 male Negroes to be 1350.25 ± 9.27 c c, and of 17 females, 1220.70 ± 20.28 c c. For whites the corresponding mean values are for 167 males 1391.08 ± 6.14 and for 31 females, 1231.93 ± 15.30 .

The small calculated cranial capacities for our Italians is, of course, associated with their small body size.

I have not found any other attempt to determine the racial changes in cranial capacity with age. Saller ('30, p 85) compares skull capacity (computed by the Lee-Pearson method) for Fehmarn islanders and other Nordic groups for two ages 20-24 and 25-60 years. His results give males 1501 c.c. at the earlier age and 1575 c c. at the later age. These are rather high values as compared with the average "liter and a half" (1500 c c.) of brains, of Todd ('27).

Individual—Figure 91 gives the curves of growth of the cephalic index of 4 rather high grade institutional boys and one idiot (No. 5). One notes the considerable range of the curves, at 16 years from 1130 to 1390. The highest of the values is above the mean of the BOA boys (1335). Most of these individual curves show the adolescent spurt at 14 to 16 years and a flattening out of the curves at around 18 to 20 years.

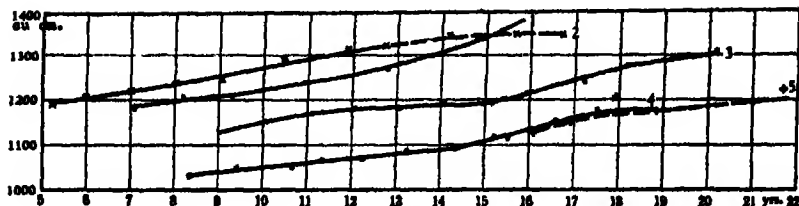


FIG 91 Individual curves of growth in Cranial Capacity of five boys of the LVD, series 1, GH No 82, 2, CH No 83, 3, C'D No 30, 4, MH No 2, 5, WW No 13

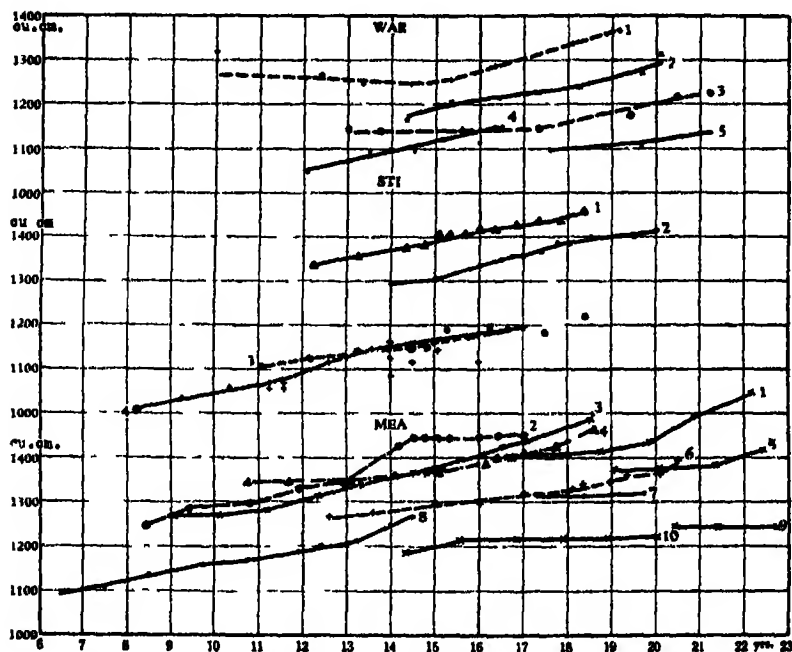


FIG 92 Individual curves of growth in Cranial Capacity in members of three fraternities War 1, Fk W m, 2, WW m, 3, Fd W m; 4, MW f, 5, BW f Sti 1, L S m, 2, CS m, 3, E S f, 4, M S f Mea 1, H M m, 2, R M f, 3, G M m, 4, W M m, 5, L M m, 6, N M f, 7, E M f, 8, B M f, 9, M i M f, 10, Ma M f Three sets of ordinates

Familial—Figure 92 shows (below) the curves of growth in cranial capacity for 10 members of the Mea. family All advance at different levels, though the curves of No. 3 and No. 4 are close together after 14 years The curves of the girls naturally lie at a lower level than those of the boys except that the curve of one of the girls, No. 2, is above all the rest. The curves at 16 years lie between 1215 and 1450 The maximum, reached at 22 years, is by No. 1 with more than the normal "liter and a half."

In the case of the *Sti.* family (above) the range at 16 years is between 1180 and 1412. In this family the cranial capacity is somewhat smaller than in the *Mea* family. Their stature is smaller.

At 16 years the mean cranial capacity of the *War.* family (on top) is 1192, of the *Sti.* family 1275, of the *Mea* family 1346 c. c.

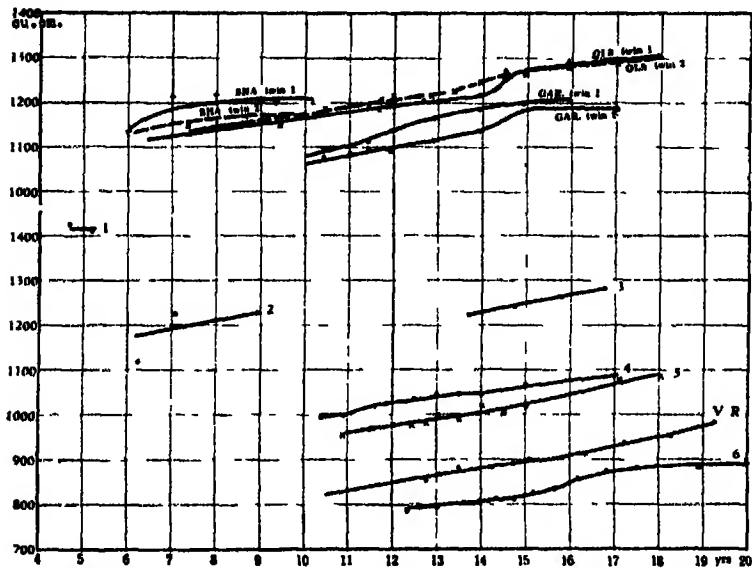


FIG 93 Individual curves of growth in Cranial Capacity in twins and special cases 1, H L m, cretin, 2, A A f, cretin, 3, C B f, cretin, 4, J S f, achondroplastic, 5, M C f, dwarf, 6, A W f, ateliotic V R m, microcephalic Gar twins, f, Ols and Sha m, twins, all monozygotic Two sets of ordinates

Twins and Special Cases —Among the special cases (Fig 93) the range is very great, at 16 years of age from 850 to 1270 c. c. The curve of the dwarf girl, A.W., No 6, lies at the very bottom. Next comes that of a median sized microcephalic boy, V R. His cranial capacity increased 150 c. c. in 8 years, thus at the rate of 19 c. c. p. a. as contrasted with the Ols. twins whose cranial capacity increased 120 c. c. during 7 years of the same period, or at the rate of 17 c. c. p. a. The cranial capacity of this microcephalic is growing fairly fast. The view that a child is microcephalic because the brain case can no longer expand is opposed by these data.

Next above are two dwarf girls at about 1050 c. c. The two cretins (Nos 1 and 2) have a high cranial capacity. One, at 16

years, of 1270 cc. A cretin, whose father was Korean, has a cranial capacity of 1415 cc at 5 years.

The curves of three pairs of twins are shown above. The members of a pair run close together, except in the case of the monozygotic Sha twins.

Summary—Cranial capacity gradually increases from childhood to maturity, and shows the adolescent spurt. Cranial capacity is closely correlated with total body size. Hence it is absolutely less in girls than boys. The means of children of lower grade mentality have generally, but not always, a lower position than of the higher grade. Racial differences are difficult to interpret because of the size differences between races. Families differ in mean level of cranial capacity. Twins develop brain capacity in much the same way. A vast difference in cranial capacity exists between microcephalics and cretins. Dwarfs reveal a small brain size that is proportional to their small stature.

2. Cranial Capacity in Relation to Stature

General—We have seen that cranial capacity increases with age during development. Since stature is a fair index of growth we have considered the percentage ratio of cranial capacity in cubic centimeters to stature in centimeters in order to learn to what degree cranial capacity keeps pace with growth of the body as a whole. This ratio is of the order 1000 to 800.

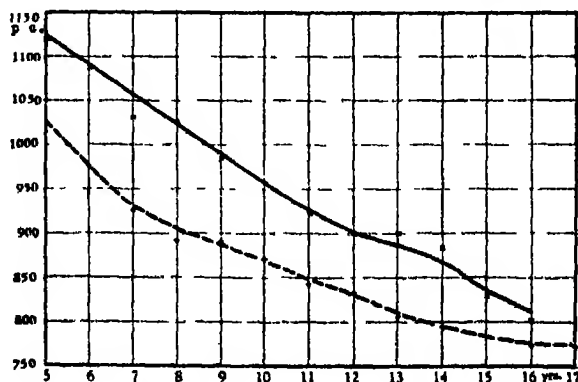


FIG. 94. Mass curves of change with age of percentage ratio of Cranial Capacity (cm^3) $\times 100$ to Stature in cm. Standard (BOA) series, m and f.

Sexual—The curves of relative cranial capacity slope downward with age sharply (Fig. 94)—from 5 to 16 years 310 points,

or 28 points p a This decline results from the slow growth of the brain on the one hand and the relatively rapid growth of stature on the other Thus between growth of body size and of brain size the rule holds that has been expressed for the relation between body and brain size of adults of various size belonging to the same ethnic group "As any bodily measurement, *e g*, stature, increases, the mean value of all other bodily measurements increases in absolute value, diminishes however (excepting total leg length) in relative value in comparison with the first" (Martin, '28, p 331). This is simply the law of regression applied to a specific case

The curve of growth of mean relative cranial capacity is not a straight line Its downward slope seems to diminish in the pre-adolescent period and then to increase at around 14 years in boys, at a time when their legs and stature as a whole are growing fastest

For girls the curve of mean relative growth of cranial capacity lies 10 per cent or more below that of boys We have seen above that absolute cranial capacity is less in growing girls than growing boys at all ages (5-16 years), and this was seen to be in part due to their smaller body size It is now obvious that even in relation to body size the brain of the girls is the smaller This fact has been noted before (Martin, '28, p 749) In view of the fact that the "size of the brain" is chiefly determined by the extent of the pallium and the pallium is regarded as the seat of intelligence (although it has a vast motor-control function) it is the more striking that intelligence tests reveal no clear difference in intelligence between boys and girls except for the higher proportion of boys in the gifted levels (I Q over 140) See Terman and Burks ('33, p. 777)

In its changing slopes the mean curve for girls runs roughly parallel to that of boys, but shows no such marked increase of downward slope at adolescence as the curve for boys, doubtless on account of the relative weakness of the adolescent spurt of growth in girls.

The ratio of the mean relative cranial capacities in the two sexes, the male being taken at 1, is for 6, 9, 12 and 15 years respectively. 0.90, 0.87, 0.88 and 0.91. The rule has been laid down by Ellis ('94, p. 90) of a "gradual sexual divergence in cranial capacity under the influence of evolution and civilization" So

far as our limited data go they suggest an ontogenetic convergence in relative cranial capacity between the sexes at least between 9 and 15 years.

Social—Considering changes with age of mean relative cranial capacity in U. S. males of different grades of intelligence we find, at 14 years, the order of levels is (from above downward) BOA (standard), I, LVD_I and M. This order agrees with that of absolute cranial capacity and also roughly with intelligence except that the I group is a little too high. In general the conclusion seems roughly to follow that the lower the grade of intelligence of any group, the smaller its relative cranial capacity. After 16 years the curves of the I and M series practically coincide.

Racial—In relation to stature the mean cranial capacity of the Nordic group (LVD_I series) stands highest at all ages, of the 3 groups considered, by 2 to 4 points. At 15 years the percentage ratio is 850. The curve for the Negro children lies below the other two curves, but rather close to the curve of the Mediterranean children. It reaches 812 at 15 years. In the LVD_{II} series the order is slightly changed, the Mediterranean group being about 20 p.c. above the Nordic.

To discover roughly how varied is the difference in relative cranial capacity in relation to stature between different races I have taken from Martin's tables the mean cranial capacity of various races, differing in degree of social development, and computed the percentage ratio of these means to mean statures in cm. The results are as follows: Papuan, 808, Wedda, 817, Marquesan, 819, Australian, 826, Tahitian, 857, Andaman, 862, Polish, 865, Bayern, 865, Singhalese, 883, Swiss, 889, Maori, 893, Dutch, 913, Hottentot, 915; French, 930, Aino, 933, Japanese, 935, Eskimo, 974.

Very roughly, the peoples of more primitive cultures have the smaller ratios of cranial capacity to stature—under 865; while the ratios from 865 to 974 include all five European groups and the Japanese and Maoris, but also the Hottentots, who have the shortest stature of the entire series.

Individual.—Of the four lowest curves in Fig. 95 three (Nos. 6, 7 and 8) are of low grade (I. Q. 26 to 35) boys, but the other one (No. 9) is of a boy who has an I. Q. of 64. The curve of No. 7 is remarkable as a horizontal line from 16 to 22 years. All the other curves slope downward at various angles. In 4 cases the

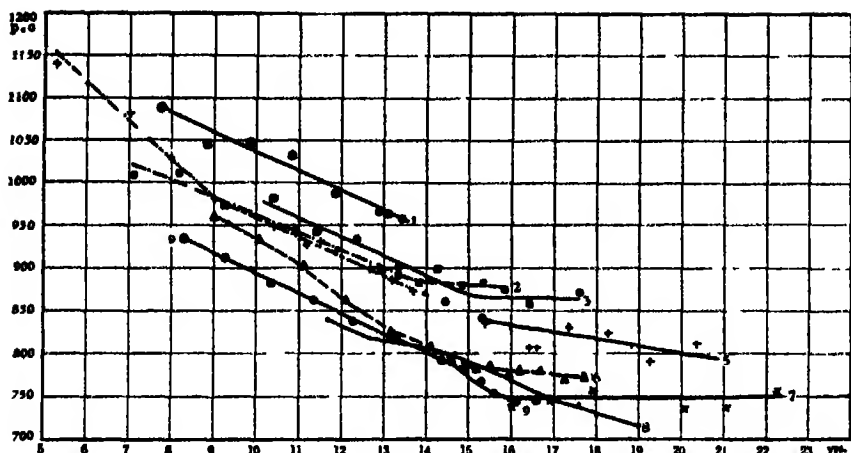


FIG 95 Individual curves of change with age of percentage ratio of Cranial Capacity $\times 100$ to Stature in cm in nine boys of the LVD series 1, J M No 129; 2, G H No 82, 3, E W No 15, 4, C H No 83, 5, W W No 13, 6, C D No 30, 7, W W No 13a, 8, P M No 2, 9, M H No 2a

angle of slope is much reduced after adolescence. Of the five curves that pass between years 12 to 16 all show a maximum downward slope somewhere during that period of maximum spurt of growth

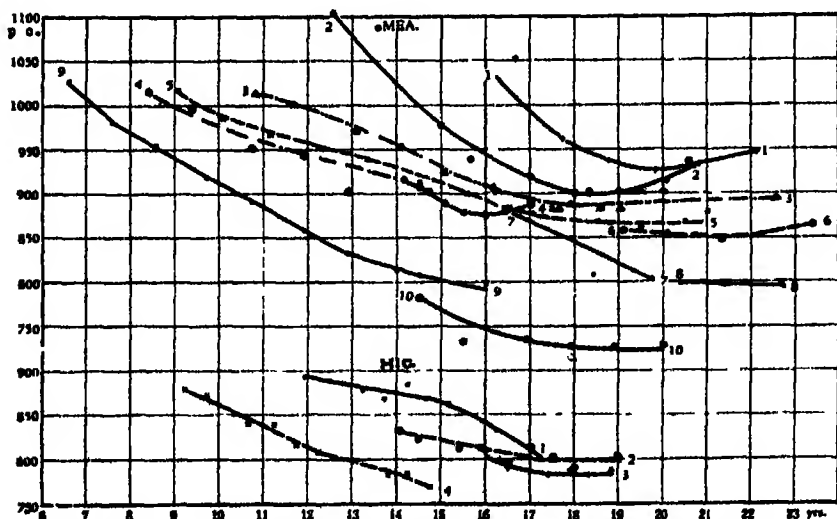


FIG 96 Individual curves of change with age of percentage ratio of Cranial Capacity $\times 100$ to Stature in cm in two fraternities Mea 1, H M m, 2, N M f, 3, W M m; 4, R M f; 5, G M m, 6, L M m; 7, E M f; 8, M i M f, 9, B M f, 10, M a M f Hic 1, C e H m, 2, H H m, 3, F H m, 4, C h H m Two sets of ordinates

Familial—In Figs 96 and 97 are shown curves of position with age of the relative cranial capacity index in four families. In all cases the curve slopes downward to 12 years, sometimes

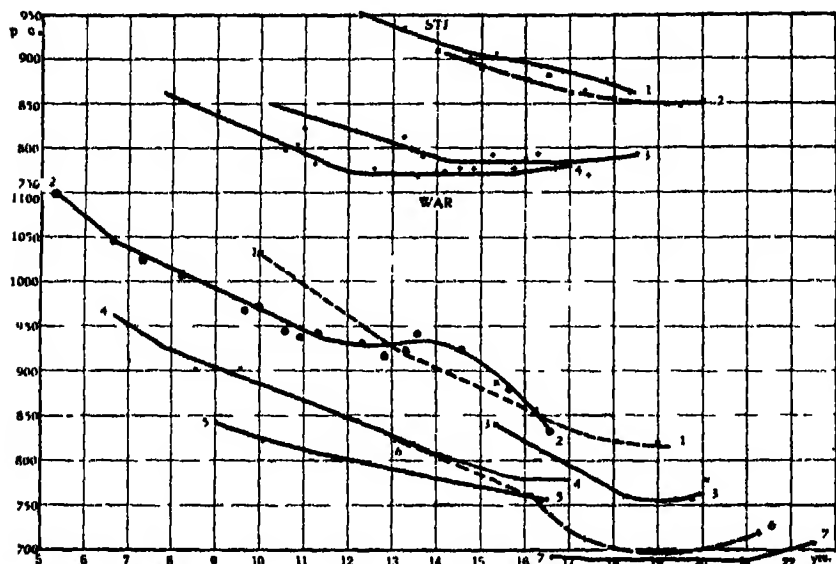


FIG 97 Individual curves of change with age of percentage ratio of Cranial Capacity $\times 100$ to Stature in cm in two fraternities Sti 1, L S m, 2, C S m, 3, E S f, 4, M S f War 1, Fk W m, 2, G W m, 3, W W m, 4, S W m, 5, M W f, 6, Fd W m, 7, B W f Two sets of ordinates

passes through a flattening at about 13 years. In the Mea family most curves are grouped at $16\frac{1}{2}$ years at a percentage ratio of 850 to 900. In two cases the curve slopes upward after 16 years, in others it becomes horizontal.

In the Hic family there is considerable dispersion around index 820 at 15 years. Two of the curves are descending at beginning of puberty, in two older boys the curves are becoming asymptotic to the X line.

The Sti. family (Fig 97) segregates by sex into two groups of two individuals each. In this family the asymptote is reached early.

Finally, in the War family the curves are quite widely dispersed, with those of the two girls at or near the bottom. This family has a low index, that of one girl being below 700 at 19 years.

Twins—Figure 98 gives the curve of relative cranial capacity for four pairs of twins. The curves of the twins are in all cases

very closely associated except in the case of the Sha twins where the curves are four points apart

Special Cases—At the bottom of Fig 98 are shown curves of 5 cases of special interest At the top of these, with an index of

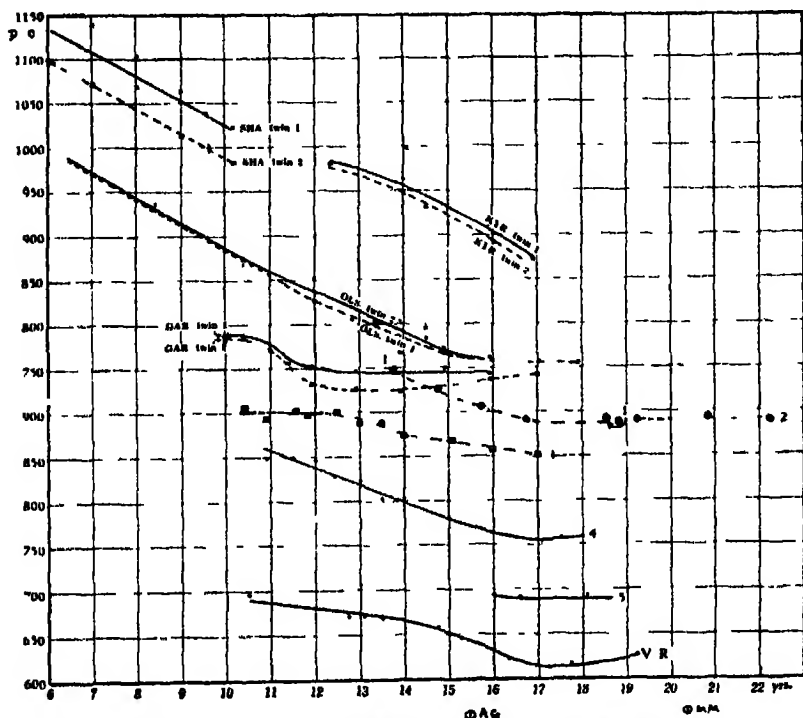


FIG 98 Individual curves of change with age of percentage ratio of Cranial Capacity $\times 10$ to Stature in cm in twins and special cases 1, C B f, cretin, 2, E S f, achondroplastic, 3, I S f, achondroplastic, 4, M C f, dwarf, 5, M W f, atelotic V R m, microcephalic Also 2 microcephalics Alma G, f, at 16 5 yrs, 562 1, Martin M, m, at 20 5 yrs, 570 Two sets of ordinates

950 to 890, is a cretin (No 1) At 890 from 19 to 22 years is a dwarf of the achondroplastic type (No. 2) Below, is another dwarf (No. 3) asymptotic at 850 Below two other atelotic dwarfs is a microcephalic boy of slightly substandard stature His curve descends to 615 and then rises slightly. His cranium has not stopped growing but his stature is growing faster The position of other microcephalics, observed once, is shown at the bottom of the diagram, two of them have indices of about 550

Mollison ('26, p 116) points out that the frontal lobes of the brain in microcephalics do not project as far in front of the zygo-

matic frontal suture as in normal Europeans. The external dimensions of the skull and the Lee-Pearson formula give too high a value for cranial capacity for this group.

Summary—The curves of changing relative cranial capacity with age in general descend with age since stature grows faster than cranial capacity. It is always less in girls than boys and at a given age is usually less as the intelligence is less. The Nordic group of our series has a higher relative cranial capacity than the Mediterranean or Negro. Individuals show a vast range in relative cranial capacity, of 150 points or more at 14 years. The curves show individual differences. The family curves tend to group around different percentages. At 14 years, at 84%, 84, 95, 83, respectively. Twin pairs keep close to the same ratio at all ages. Cretins have a high ratio (short stature) and microcephalics very low ratios because of small cranial capacity.

3 Cranial Capacity in Relation to Sitting Suprasternal Height

General—From Fig 94 it appears that, on the average, in males of the BOA series the downward slope of the cranial capacity/stature curve between 5 and 10 years is 33 p a. Thus the stature grows much faster than cranial capacity. Between 10 and 12 years the slope of relative cranial capacity is downward at the rate of 30 p a. This is the period of slow growth in stature. Since stature is so largely composed of leg length and increase in leg length is surely out of all proportion to increase in those brain cells that have to do with the functioning of the legs, it has been suggested that increase of cranial capacity might better be compared with increase of trunk length.

For trunk length we take the suprasternal height, sitting. The suprasternal point is taken at the superior border of the manubrium, in the jugular notch. An attempt is made to get close to the bone rather than on top of the interclavicular ligament. Especially important is the correct posture of the subject. Most children tend to assume the anthropoid slouch when sitting. The subject should sit erect, revealing the normal sigmoid curve of the vertebral column only slightly expressed. The top of the sternum is exceedingly labile, rising and falling with deep respiration or emotion. It is hoisted into an abnormal position with the order of "sit up straight!" Vertex height as well as suprasternal is measured while sitting and the distance between these measure-

ments is compared on the spot with the distance between the same points found standing. If the difference in these distances is 10 mm, or over, the measurements sitting and, if necessary, standing are repeated to secure agreement.

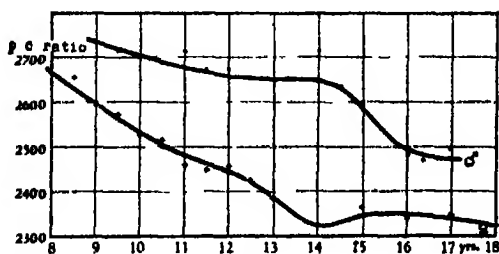


FIG 99 Mass curves of change with age of percentage ratio of Cranial Capacity in cm. to Sitting Suprasternal Height in cm. LVD_{II} series, Nordic m and f

Sexual.—Figure 99 reveals the percentage ratio of cranial capacity in cubic cm. to sitting suprasternal height in cm. during adolescence and puberty for children of the LVD_{II} series—the only one for which curves are available for several racial groups and for the Nordics of both sexes.

Taking the male Nordics, ages 10 to 12, the slope is about 20 points p a. on a percentage basis, as compared with the slope of 30 points p a. for cranial capacity in relation to standing height. The curve of the Nordic males flattens out at 12 to 13.5 years, then slopes downward rapidly (100 p a.) as the adolescent spurt of growth affects the trunk as well as the legs. Compare Fig 94.

The slight difference in slope of cephalic index in relation to sitting trunk height and to total stature is rather surprising and indicates how little dependence there is between the cubical growth of the brain and that of the body as a whole as measured by a single linear dimension. The downward slope indicates that the volume of the cranium grows much more slowly than linear bodily dimensions. It is indeed common knowledge that the size of the head at birth more nearly approaches adult size than other external feature of the body.

The female curve (Fig 99) lies at 11 years some 200 points (8 per cent) below the male, indicating that the small cranial capacity of the female (page 114) is not due merely to smaller body size. The female curve shows only a slightly increased slope at 12 years, the age of the slightly increased adolescent growth in body as a whole.

Racial—There is only a slight racial difference in cranial capacity in relation to sitting suprasternal height. At 14 years the order of the descending male series is: Jews, Italians, Nordics and Negroes. But the total range is less than 1 point. The Negro group has the steepest slope, 0.7 p a, in consequence of which it passes from uppermost to lowermost position in the set during the 13th to 14th year. The curves of the other series hold their relative position with a decrease of 0.3 to 0.5 point p a. The Nordic shows the greatest influence of the adolescent spurt.

Of the two female series the curve of Negro ratios is again steeper than that of the Nordic, about 0.7 points p a, while the Nordic slope is about 0.3 p.a. This steeper negative slope of the Negro curve of capacity in relation to trunk height is in accord with the progress of absolute cranial dimensions. The cranium of the Negro seems to develop more precociously in early childhood and to progress more slowly after adolescence than that of European stocks.

Summary—The course of change in cranial capacity relative to sitting trunk height is much like that in relation to total stature since in both cases the slow growth of the cubical content of the cranium is ever falling behind that of linear dimensions of the body.

4 Cranial Capacity in Relation to Body Modulus

General—Body modulus for the purpose of this study is the product of three dimensions that multiplied together seemed best to express the volume of the body. The factors in this modulus are height of suprasternal notch, kneeling, transverse chest, antero-posterior chest diameter. The modulus is expressed in cubic centimeters. The quotient of cranial capacity expressed in cubic centimeters, divided by this modulus, gives the percentage ratio. It is clear that, in this case, we are making comparison of volumes, which is a more significant procedure than comparing linear dimensions with volumes. In a concrete example S.M. (I, No 90) at 17.10 years has a kneeling suprasternal height of 91.6 cm, chest transverse, 27.0, chest a p, 20.0 cm, computed cranial capacity 1286 c.c. The formula is

$$1286 \div (91.6 \times 27.0 \times 20.0) = 1286 \div 49464 = 0.026 \text{ or } 2.6\%.$$

Figure 100 gives the curves of changes with age in cranial capacity in relation to body modulus for the standard series.

From 6 to 10 years the downward slope in the mean male is 25 points, or 0.625 point p a., as compared with about 30 points p a for cranial capacity in relation to stature and about 20 points p a

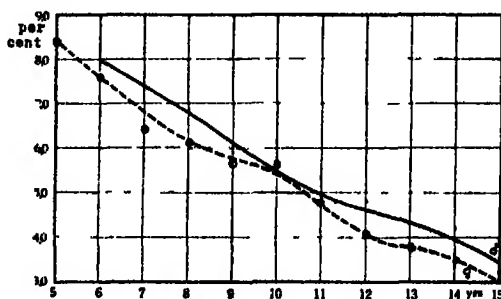


FIG 100 Mass curves of change with age of percentage ratio of Cranial Capacity in cc to Body Modulus in cc (see text) BOA, U S and mixed, m (unbroken line) and f (broken line)

in relation to sitting suprasternale. Thus, in comparing body volume with cranial capacity, changes in the denominator are so much greater per annum than in the case of the linear functions of stature or sitting suprasternale that the present index really gives a more adequate idea than the other ratios of the diminishing relation with age of brain size to body size.

Sexual — In girls the general trend is the same as for boys. The female's curve lies 4 or 5 points below that of the male except that, at 9 to 10 years, just before the time of the precocious spurt of body growth, the cranial capacity seems to make a relatively greater increase than the body as a whole. This leads to a near contact with the male's curve. However, a year later the body growth takes up again its relatively greater growth and the cranial capacity in relation to body size of the females again separates rapidly from that of the males.

Social — The social differences in this ratio are surprisingly small. The mongoloid dwarf curve runs in general above the standard curve, owing to the small size of the body.

Racial. — Of the Nordic, Mediterranean and Negro groups the latter's cranial capacity in relation to body modulus is the smallest of the three. The Nordic group shows the highest index despite its large body size.

Individual. — Figure 101 gives the age changes of the brain/body modulus for 7 Letchworth Village boys. No. 5 is of a boy whose stature is 15 cm. below standard and whose I.Q. is about 65.

His curve is placed high despite his medium sized body No 2 is a small boy, with an I.Q. of about 35 His high position is due largely to his small body At the bottom of the figure, No 7, is a boy about 10 cm shorter than standard, with an I.Q. of about 55. The relative position of the curves in the graph depends

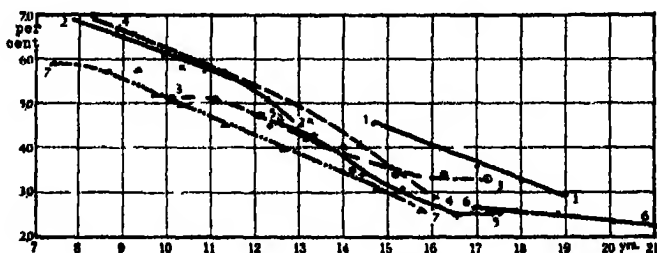


FIG 101 Individual curves of change with age of percentage ratio of Cranial Capacity in cc to Body Modulus in cc in males of LVD and I series 1, P M I, No 10, 2, J M I, No 129, 3, C D D_I, No 30, 4, M H D_I, No 2, 5, W E I, No 15, 6, W W I, No 13, 7, R C D_I No 84

evidently on both cranial capacity and body size and the former is loosely correlated with intelligence.

5. The Cephalic Modulus

This is a computed datum obtained by summing length, height and width of the head, or sometimes by dividing that sum by 3 (cf Martin, '28, p 655) The two methods would give like shaped curves The cephalic modulus has a certain advantage over the "cranial capacity", since it is independent of various statistical assumptions.

Figure 102 gives the graph of the mean cephalic modulus from mid-gestation to 18 years Noteworthy is the rapid increase during gestation and the first half year post partum Thereupon follows a period of rapidly decreasing rate of growth until after 3 years the addition to the sum of the 3 dimensions is less than 2.5 mm per year. The differential of increases on a yearly basis is shown in the lower half of the figure. This curve shows vividly in how short a time after birth the adult volume of the brain case is almost completely achieved

The module at 18 years is 460.3 compared with that obtained by Hrdlička ('25, p. 187) from Old American males (ages 20-29) of 490.5 (after multiplying Hrdlička's module by 3 to render the two modes comparable).

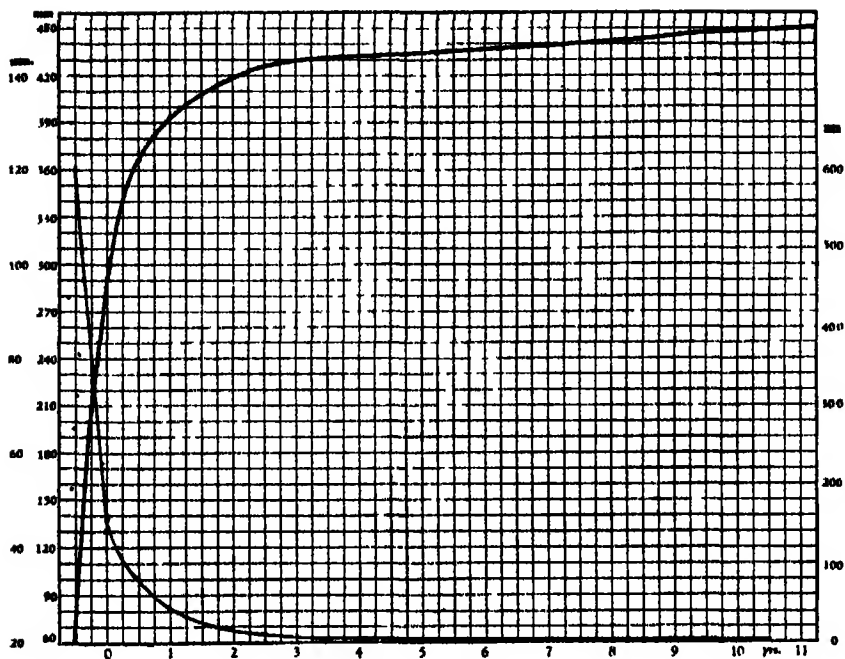


FIG 102 Above, mass curve of Head Modulus, Head $L + W + H$, fetuses, Babies Hospital and BOA males Also, with left hand scale of ordinates, Head Modulus, $\frac{\text{Head } L + W + H}{3}$ Below, mass differential curve of Annual Increments of Cephalic Modulus, $L + W + H$ in cm Ordinate scale on right margin

V GROWTH OF ABSOLUTE DIMENSIONS OF THE FACE

1 Introduction

General—The head is divided, as we have seen, into cranial and visceral portions. The former has been considered. The visceral part of the head is not at all sharply demarked from the cranial. Thus, the zygomatic arch is made up in part of the parietal process and in part of the temporal process, but it comprises also a maxillary process which latter is properly regarded as visceral. All the facial bones below the upper margin of the orbits down to and including the lower jaw we may regard as certainly visceral.

The changes in the visceral part of the face with age are much greater than those of the cranium, as is sufficiently obvious by comparing the face of an infant and an adult.

One of the principal factors in the change is the appearance of

the molars. With their eruption the horizontal depth of the maxilla is increased by about one-third. The mandible increases at the same time and rate. Metric studies on the growth of the face have been made by Hellman ('32, '35, '37), Keiter ('33), Abel ('34), Goldstein ('36), Koya ('38). Repeated roentgenograms of the face of the same child have been published by Broadbent ('37). Krogman ('30-'31) has made valuable studies on the growth of the face of anthropoids.

That there is an inheritance of facial dimensions must be obvious to any observer of families. Saller, Gutbier, Kohl and Schiereck ('33), have computed the correlation coefficient between mid-parents and children for a number of facial dimensions. The values of r often vary from 0.323 ± 0.059 for width between inner eye angles to 0.470 ± 0.052 for bizygomatic width.

Material and Methods—For the study of the development of the face we have data derived from the same children enumerated on page 5. The measurements that were taken for this study are 9, viz. Trichion to gnathion (physiognomic face height), nasion to gnathion (morphologic face height), nasion to stomion, nasion to subnasale (nose height, of which an account was published by me in 1939), trichion to nasion, stomion to gnathion (the height of the lower jaw), subnasion to gnathion, bizygomatic width,¹⁰ bigonial width.¹⁰ From these were computed 7 percentage ratios as follows: Morphologic to physiognomic face height, nasion to stomion in relation to morphologic face height, nose height to morphologic face height, chin height to morphologic face height, forehead height to physiognomic height, physiognomic face height to bizygomatic width, morphological face height to bizygomatic width.

The measuring was done under the best of conditions. The results were tabulated, slightly smoothed, and means and ratios computed and graphs drawn from the smoothed data.

2. *The Growth of the Physiognomic Face Height (P.F.H.)*

General—The physiognomic face height (P.F.H.) is measured from the hair line on the forehead (trichion) to the gnathion. The trichion is defined by Martin ('28, p. 143) as the point at which the anterior forehead hair line is cut by the median sagittal plane. It is warned not to take the lowest-standing hair, but the physiognomic scalp margin. With babies this point is often indeterminable for in them a down, which grades into the down-like scalp hair, often

¹⁰ Already considered in earlier sections

covers the forehead. Even in older children it is by no means always easy to determine the hair line. A whorl of scalp hairs may extend in a more or less triangular form over the upper forehead, as a "cow lick," beyond the average hair line. This enhances the difficulty of making an accurate judgment of the average hair line. Among the idiots occasionally one finds the terminal hairs scattered over the forehead as in the Chimpanzee, in such fashion that it is impossible to define the limits of the scalp. There is no proper trichion. Such cases have to be eliminated from consideration in this report.

The gnathion is defined by Martin ('28, p. 147) as "*derjenige Punkt des Unterrandes des Unterkiefers, der in der Mediansagittal-Ebene am meisten nach unten vorragt. Gemeint ist ein Punkt des knöchernen Unterkiefers, der von unten her palpiert werden kann und natürlich weiter zurückliegt als die vordere Hautbegrenzung des Kinnes*."

The form of the chin is so variable that it is not always easy to locate this landmark. Wilder ('20, p. 42) recognizes a second landmark on the chin just above the gnathion, namely the pogonium, the most projecting median point of the chin, and this may in some cases approach the gnathion, even blend with it, or be entirely absent, as in the case of the Neanderthal skulls. As in other cases, the point to which measurement was made was on the skin overlying the bony landmark.

Sexual -- Already at the end of the 6th lunar month one can ordinarily differentiate the scalp hair from lanugo. I was accordingly able to measure the physiognomic face height on about a dozen fetuses of various ages. Beginning then at 7 lunar months with a mean face height of 73 mm (Fig. 103) this dimension increases rapidly to 89 mm at birth, and, in only a slightly diminished upward slope, to 130 mm. at the 10th post-natal month, or at the rate of 49 mm p. a. Then follows a period of slower growth probably associated with the loss of head height on account of assuming the erect position. The slope gradually increases to $2\frac{1}{4}$ years and is followed by a small average slope from $2\frac{1}{2}$ to 5 years of 1.6 mm. p. a. There is a more rapid increase during the sixth year, perhaps associated with the cutting of the first molar, then a slowing up and an increase again at the time of the adolescent spurt. The interaction of various developmental episodes results in a varied slope of the growth curve. Already at 16 years the mean curve is approaching asymptosis, giving an upper limit at

about 170 mm I have two cases of 17 year old boys with face heights of 193 mm Martin ('22, p. 894) cites one of 230 mm

The mean female growth curve of physiognomic face height is first clearly differentiated from the male's at about 6 months. The

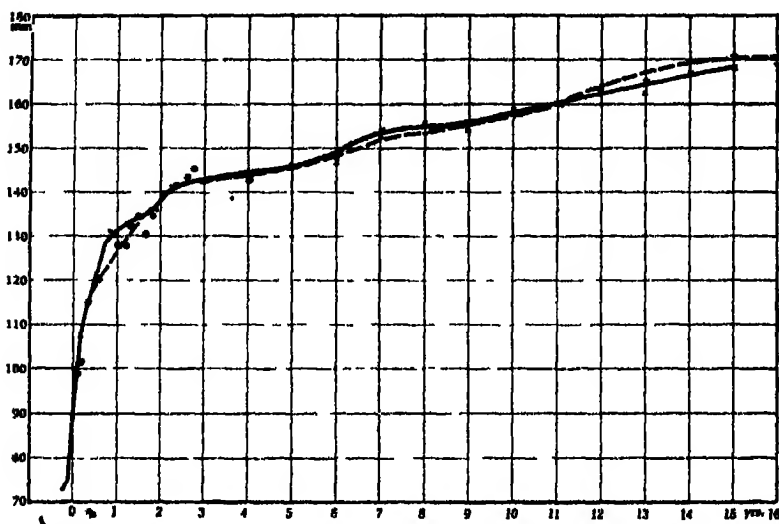


Fig 103 Mass curves of growth of Physiognomic Face Height Standard (BOA) series, males (unbroken line) and females (broken line)

female curve lies below that of the male a millimeter or two, on the average At 11 years it decussates with the male curve and thereafter lies above it until at least 16 years (Fig 103).

The findings of this chart are not of universal application In our LVD_{II} series the female curve lies always below the male to 17 years

Social—Five groups of varying intelligence were measured At 14 years the standard (BOA) group lies at a higher level (164 mm) than the others, except the LVD_{II} group, which is placed at 168.5 mm Below the BOA group at this age are, in order, LVD_I at 162 mm, the I group at 161.2 mm., and the M group at 158.5 mm, omitting the females of the LVD_{II} from discussion here. As in the case of other dimensions the superior position of the LVD_{II} group is not easily interpreted It is noteworthy that the LVD_I, I and M groups reach about the same level at 19 or 20 years; there is a difference in the time of the inflection. This occurs at 14.5 years in the LVD_{II} series, at 15.5 years in the LVD_I series; at 18.5 years in the I series and at about 15 years in the M series. This

relatively early inflection of the M curve is rather striking in view of the dwarfness and low mentality of the M group. As for the others, they suggest that different grades of development (as measured by intelligence) are in order characterized by successively later spurt of growth. There is successive retardation in development.

Racial.—The Negro group has the greatest physiognomic face height at all ages from 9 to 15 years. The Nordic group comes next and the Mediterranean at the bottom, associated with shorter stature. The mean head height of the Negro is only about the same as that of Nordics, while the mean nose height and lower jaw height are greater.

The ages of inflection are of interest as indicating the age of fastest growth. That of the Nordics is at 15 years (age of prepubertal spurt in growth), of Negroes at both $12\frac{1}{2}$ and $14\frac{1}{2}$ years, while there is no obvious inflection for the Mediterraneans up to the age of 15 years.

The data of Goldstein ('36, p. 49) from New York City Jews in an Infant Home and in public schools show measurements that are so large, even as compared with my standard series, that I have to conclude that he placed the trichion higher than I am accustomed to do.

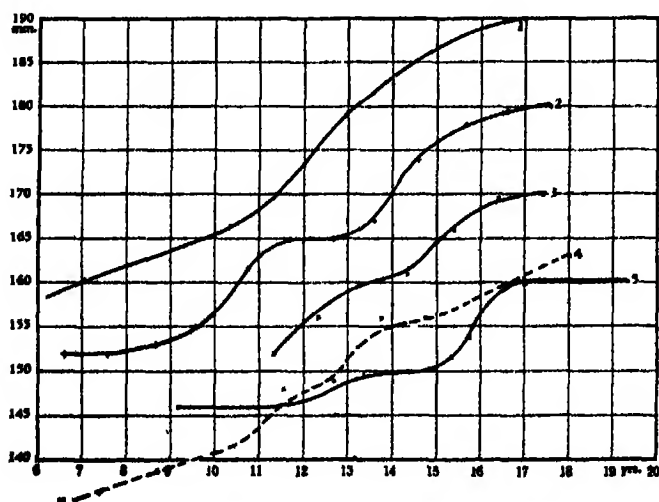


FIG 104 Individual curves of growth of Physiognomic Face Height for five boys, LVD₁ series, all U S white except 3. 1, V E No 59, 2, S B No 55, 3, F B colored, No 48, 4, J B No 54, 5, F C No 28.

Individual and Comparative —Figure 104 gives growth curves for 5 boys of the LVD₁ series. They run roughly parallel courses but at very different levels. At 14 years the lowest has a P.F.H. of 150 mm, the highest of 183 mm. A colored boy, No. 3, occupies a median position.

The age of adolescent inflection varies from 13 years in the top curve to 16 years in the bottom curve. In general the highest adult face undergoes its maximum speed of growth earliest. The curve of No. 4 is somewhat erratic. It will be observed that in all cases (except that of No. 4) the curves have become asymptotic or nearly so during the 18th year.

The total mean annual growth during the course of 10 years varies roughly in order of eventual height attained, nor is this associated with eventual stature (Table 6).

TABLE 6

RELATION OF INCREASE IN PHYSIOGNOMIC FACE HEIGHT TO STATURE
IN FOUR INDIVIDUALS DURING TEN YEARS

Individual	Increase mm. p. a. During 10 Years	Stature cm.
Curve 1 V E No. 59	3.3	160.7
Curve 2 S B No. 56	2.8	163.8
Curve 4 J B No. 54	2.6	155.1
Curve 5 F C No. 28	1.45	165.0

Familial —Figure 105 gives growth curves for two L V families. Above are two girls and two boys of the St₁ family. The curves (3 and 4) of the two girls lie close together. Between 12 and 15 years they increase about 0.5 mm. p. a. Those of the boys run parallel in part of their course. At 15 years physiognomic height lies between 160 and 165 mm. The slope of the boys' curves at this age is about 2.4 mm. p. a.

Below are the growth curves of five boys and two girls of the War. family. In this family there is a great range in face height—at 15 years from 156 to 179 mm. The growth curves of this family fall into two groups. The one with the higher face is centered, at 14 years, around 176 mm, that with the lower face at around 155–160. The upper growth curves have a slope of about 2.5 mm. p. a. The lower ones of 1.8 mm. p. a. Curves No. 4, 6 and 7 lie close together at 14–15 years, No. 1 shows a remarkable

increase of slope from 17 to 19 years to 5 mm p a The curve of No. 5, a girl, shows no increase at all

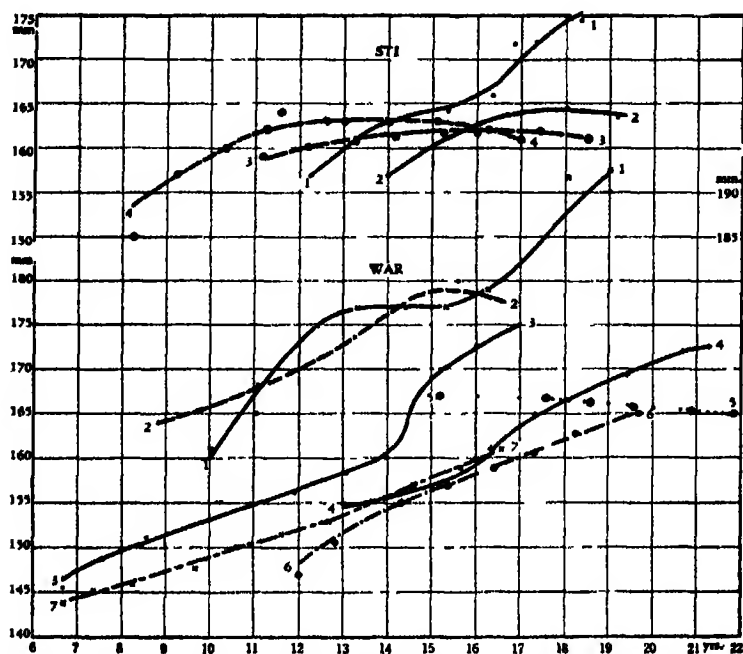


FIG 105 Individual curves of growth of Physiognomic Face Height for members of two fraternities Sts 1, L S m, 2, C S m, 3, E S f, 4, M S f War 1, Fk W m, 2, M W f, 3, S W m, 4, Fd W m, 5, B W f, 6, W W m, 7, G W m Two sets of ordinates

Figure 106 gives the physiognomic growth curve for the Mea. family of ten children The upper group includes the five girls three of them post-adolescents, the other two passing through juvenility to puberty. In the curves of the latter two the slope is about 3.5 mm p a All curves show a remarkable tendency to slope downward at their right hand ends, though the maxima lie at various ages from 15 to 21 years. The downward slope might be due either to the movement of the hair line downward on the forehead or to a loss of teeth. It cannot, however, be the latter since the morphologic face height shows no such downward slope

The curves of the four boys are placed below on a different scale of ordinates, to avoid confusion by overlapping. Before the post-adolescent flattening occurs they advance at an average rate of about 2.5 mm. p.a. No. 7 advances fairly uniformly from 10 to 21 years without showing any juvenile or adolescent spurt No 9

shows the adolescent spurt beginning at 14 and lasting to 17 years, rising 4 mm. p a. All curves approach an asymptote at 20 to 22 years

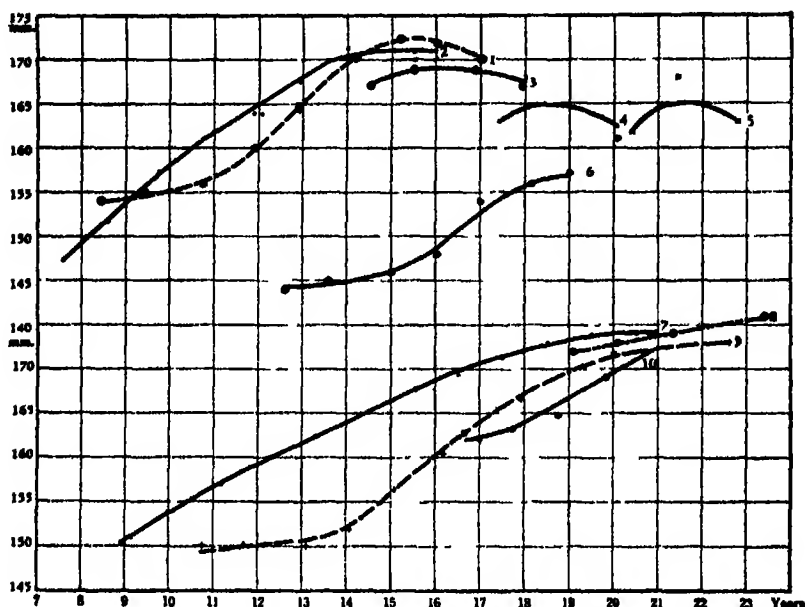


FIG 106 Individual curves of growth of Physalognomic Face Height for members of the Mea fraternity 1, R M f, 2, B M f, 3, Ma M f, 4, E M f, 5, M₁ M f, 6, N M f, 7, G M m, 8, L M m, 9, W M m, 10, H M m. Two sets of ordinates

Curves Nos. 1, 6 and 9 run in roughly parallel sigmoid fashion and the points of inflection are at 13, 16 and 16½ years respectively.

Twins and Special Cases —In Fig 107 are shown the growth curves of the Ols. and Gar. twins. The curves of the Ols. twins, boys, run close together about 2 mm. apart and with a post-adolescent slope of about 4 mm. p a, without slowing up at 18 years. The Gar. twins (certainly monozygotic) run roughly parallel courses 6 to 8 mm. apart. They give curves that are sigmoid, with inflections at 12 or 12½ years

The curves of all the special cases lie at a lower level than those of the twins. At the bottom are two curves (6, 7) of dwarf girls, these at 15 years stand at about 135 mm., as contrasted with the average of 165 mm. of the Gar. twins. No. 3 is of a girl of much retarded growth of no well defined endocrine type. Nos. 1, 4 and 5 are of cretins and No. 2 is the microcephalic, V.R., shown in many

other figures. He attains a face height of 163 mm at 19 years, at which time growth has nearly stopped

Summary—Despite difficulties in landmarks, the mass curve of physiognomic face height is found to develop, like head height,

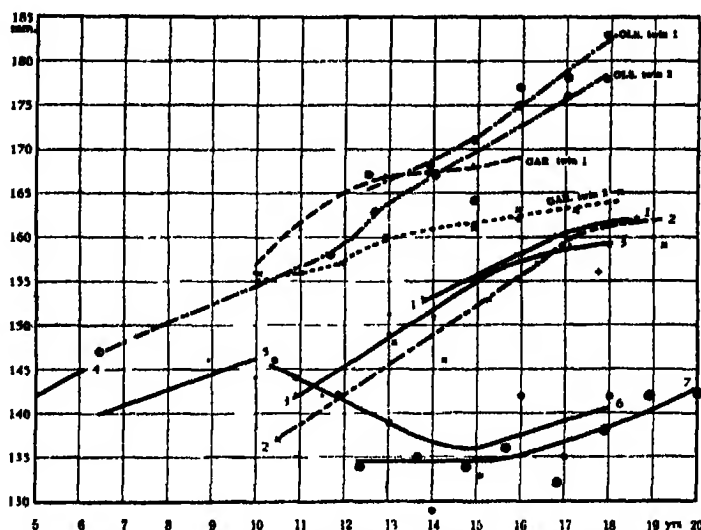


FIG 107 Individual curves of growth of Physiognomic Face Height for twins and special cases 1, C B f, cretin, 2, V R m, microcephalic, 3, M C f, dwarf, 4, H L m, cretin, 5, A A f, cretin, 6, L S f, achondroplasia, 7, A W f, ateliotic Monozygotic twins, Gar f, Ols m

rapidly to about 10 months post-natal, and then to slow up, probably partly on account of gravity's action on the skull In general, groups of white children of subnormal intelligence have lower faces in order of inferiority Negroes have the highest mean faces. Curves of individuals undergo, usually, a more or less parallel sigmoid course, but inflections are at different ages In families there is evidence of segregation in the position of curves and they have different centers of concentration. The curves of twins run parallel and approximated courses Dwarfs have low faces, a microcephalic boy a rather high one

3. Growth of the Morphologic Face Height (M.F H)

General.—This is the distance from nasion to chin angle (gnathion) often referred to in the literature as Gesichtshöhe, also Gesichtslänge. I have already discussed these landmarks and the limitation to precision which they present. This is a dimension

which has been often taken on skulls because its limits are well defined in them.

The special interest of this dimension is that its increase measures the growth of the visceral part of the skull which in man is so strikingly reduced from the anthropoid condition

Martin ('28, pp. 894, 897) gives the morphological face limits for adults of the various human races as 90 and 145 mm. Among the lowest faces, morphologically, of males are the Philipinos and Australian aborigines (103) and Congo Pigmies (109), among the highest are Eskimos (127). For adult Germans 123 mm is a mean found by Weissenberg (teste Martin, '28, p. 898)

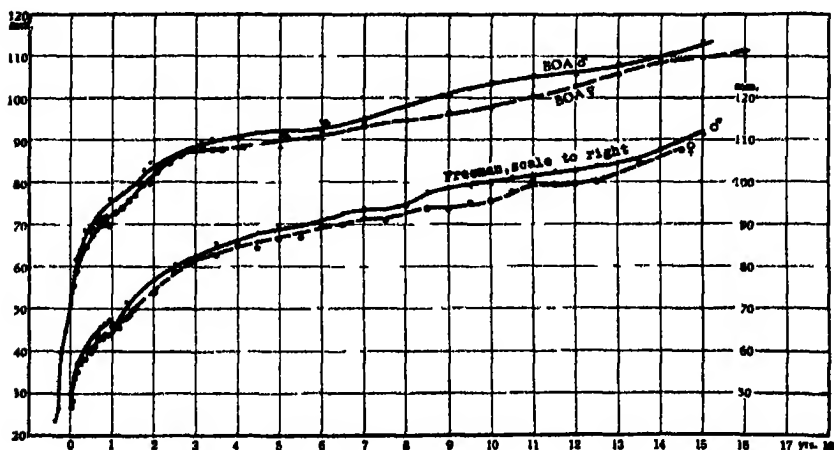


FIG 108 Mean curves of growth of Morphologic Face Height for BOA, m and f, also, for comparison, series of Freeman, '32, '33, using scale to right

As Fig 108 shows, beginning at about 5 months (pre-natal), with a height of 24 mm the mean face height rises rapidly to 51 mm. at birth. Kugler ('32, p. 502) finds for Zürich neonates a mean of 54 mm, Freeman and Platt ('32, p. 73) of 47 ± 0.25 . After birth the slope of the growth series gradually diminishes to 10 months and then diminishes still more rapidly for 3 or 4 months. The slowing up of this vertical dimension at about 1 year is associated with the slowing up of growth of head height, and is probably due to the action of gravity as the head is held upright in sitting and especially walking.¹¹ After about 18 months the increment in-

¹¹ This decrease in face height at about the end of the first year (13th month) appears in the table of averages with age of Freeman and Platt, but it is not referred to by them. See Fig 108

creases more rapidly as if to compensate for the earlier loss of increment After the age of 3 years the slope is small for 3 years, rises to a maximum at 9 years (juvenile spurt) Later there is a larger increment, beginning at 14 years (the adolescent spurt)

Sexual — In my data one first notices a difference in morphological face height between the sexes at about two months post-partum Kugler ('32, p 502) finds at birth a mean for females of 53 mm as contrasted with 54 for boys.

From one month the curve for females runs roughly parallel to that for males (but 1 to 3 mm below) to 7 years The male juvenile spurt carries the curves away from each other as much as 5 mm, then the means approach to within 2 mm at the adolescent spurt in girls at 13 years

The Niggli-Hurlimann findings on Swiss children are about 5 mm greater than mine for each sex for ages $4\frac{1}{2}$ to $6\frac{1}{2}$ years Hoesch-Ernst's ('06) results agree fairly closely with mine, especially as to sex difference Takabatake's ('38, p 86) results for Japanese children show a smaller sex difference than mine for North European stock, but in the same direction

On Fig 108 are drawn also the growth curves of face height based on Freeman's ('32 and '33) data for both sexes The notch at the age of 12 months is striking In general slopes the curves agree with mine, but Freeman took as nasion a point about 2 or 3 mm lower than I did.

Social — The highest morphologic faces are found in the LVD_{II} series, with BOA children next, after 14 years Before 14 the mean LVD_{II}, BOA and I curves differ very little The most distinct group is that of the Mongoloids, about 10 mm lower than the others

Racial — At 12 years, in morphologic face heights, the Negroes stand highest, Mediterraneans next and Nordics least Beyond 14 years, in my series, the Nordic morphologic face height exceeds that of Mediterraneans Schlaginhaufen ('39, p 28) finds a M.F.H for Swiss of 122 mm. Weninger's ('27, p 47) mean for West African male Negroes is 117.75 mm which is less than Weissenberg's findings of 123 for Germans Day ('32, p 70), on the other hand, finds a mean adult morphological face height of 123.65 mm. for "Negroids" (Negro-White hybrids), 124.50 mm. for full blood blacks and 121.60 mm. for whites So it is doubtless

correct to say that adult American Negroes have a greater M.F.H. than whites.

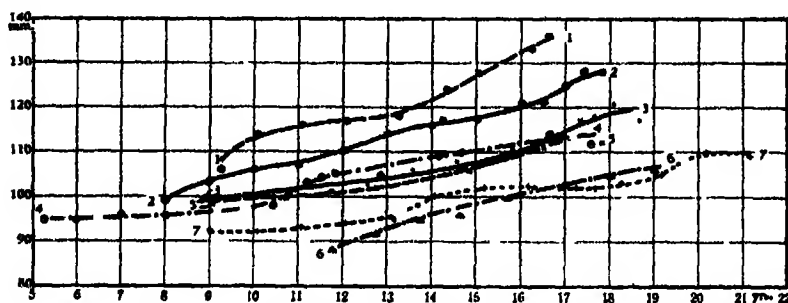


FIG 109 Individual curves of growth of Morphologic Face Height for seven boys of the LVD₁ and 1 (one case) series 1, H W No 66, 2, A M No 7, 3, J C 10, 4, C H No 83, 5, I J No 18, 6, P M 1, No 10, 7, C D No 30

Individual—Figure 109 gives curves of growth of 7 individual boys measured during about 10 years. These are L V. boys, all but No. 6 of fairly high grade. Most of them show a steady rise with age, about 2.2 mm per annum. Increase of slope occurs at the juvenile spurt (Nos 1, 2, 4) and again at adolescence (Nos. 1, 2, 3, 7). Curve No 6 is of an imbecile boy of small stature.

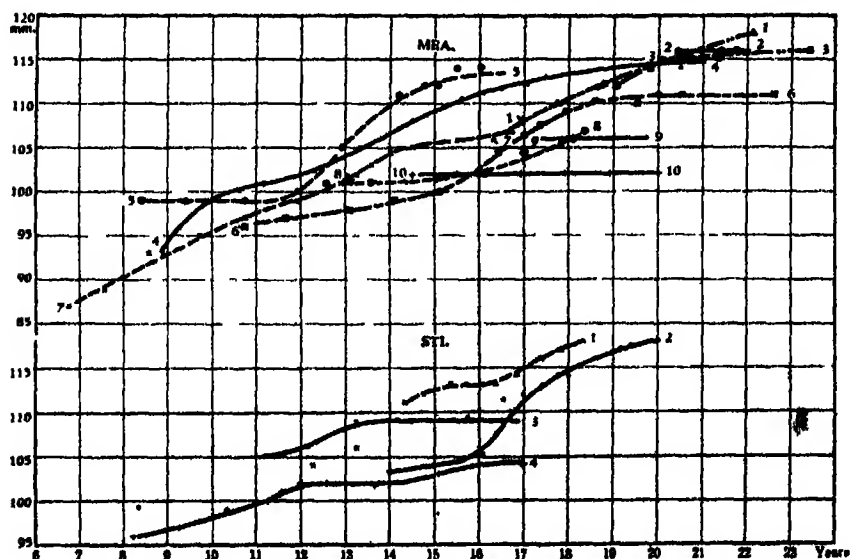


FIG 110 Individual curves of growth of Morphologic Face Height for members of the Mea and Sti fraternities Mea 1, H M m, 2, M i M f, 3, L M m, 4, G. M m; 5, R M f, 6, W M m, 7, B M f, 8, N M f, 9, E M f, 10, M a M f. Sti 1, L S m, 2, C S m, 3, E S f, 4, M S f. Two sets of ordinates

His curve of growth advances without spurt but at a fairly steep slope.

Familial.—Figure 110 gives individual growth curves of M F H for two families—Mea and Sti. At 14 years the first family is centered at about 104, the second at 106. Yet the children of the Mea family are larger than those of the Sti family. In the Mea family there are three of the older ones whose face height has nearly stopped increasing. The others advance about 2 mm per year with increasing slopes at periods of spurts of body growth. No doubt development of the permanent molars plays an important role here.

In the Sti family the periods of most rapid growth of the face height (inflexion) are apparently spaced some years apart, partly because the curves do not cover the same life span, so that all do not cover both spurts of general body growth.

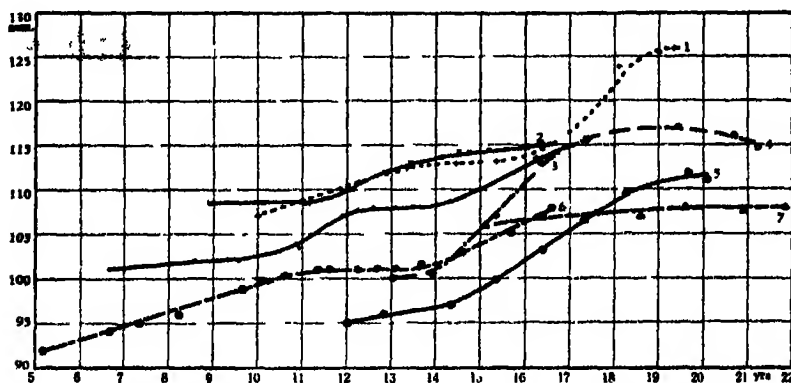


FIG 111 Individual curves of growth of Morphologic Face Height for members of the War. fraternity 1, Fk W m, 2, M W f, 3, S W m, 4, Fd W m, 5, W W m, 6, G W m, 7, B W f

Figure 111 shows curves of growth of M F.H. in seven members of the War. family. The curves exhibit typical spurts of growth.

Twins and Special Cases —In Fig. 112, below, the growth curves of morphologic face height in the Ols. twins run very close together during the course of 12 years, coinciding at 16 years. In the Kyr. twins there is pretty constantly a difference of 2 mm, and a strictly parallel course of growth.

Among the special cases (above) the curves of three dwarfs (Nos. 1, 3, 4) lie at various levels. The cretins (Nos. 2, 5, 6) occupy an intermediate position.

Summary.—The morphologic face height increases rapidly until the child stands, more slowly thereafter and less rapidly in girls than boys. This dimension is low in mongoloid dwarfs and high in Negroes. The individual curves show an adolescent spurt of



FIG 112 Individual curves of growth of Morphologic Face Height in twins and special cases 1, M C f, dwarf, 2, C B f, cretin, 3, L S f, dwarf, 4, A W f, ateliotic, 5, H L m, cretin, 6, A A f, cretin

growth, doubtless associated also with the development of the molar teeth. Between twins, growth of the dimension is similar.

4 Growth of the Dimension Nasion to Stomion

General—This dimension is approximately that of morphological face height from which the lower jaw height has been subtracted. It corresponds with Martin's ('28, p. 187) No 19, physiognomic upper face height, also called middle face height, also nearly with Young's ('37, p. 460) nasion to upper incisor (incisal margin). The upper landmark is the (somewhat indefinite) nasion, the lower landmark is the slit between the lips when the mouth is gently closed. The lower landmark is mobile and accordingly it is subject to an error of 2 or 3 mm. This dimension measures the visceral, facial, part of head above the mouth. It differs from the morphological upper face height, since in the latter the lower

landmark is the prosthion, or the margin of the maxillary gum (gingiva) between the two upper median incisors. The difference between the two dimensions is approximately the exposed part of the median incisors. The difference is variable as between young children and older ones and inside of either group, it is, in adults, of the order of 5 mm.

The physiognomic upper face height seems not often to have been measured, the morphological upper face seems preferred. Schultz ('26, p. 79) measured Martin's No. 19 dimension upon 37 Nicaraguan Indians and found a mean of 76 and 79.6 mm respectively in two tribes. Takabatake ('38, p. 98) gives the physiognomische Obergesichtshöhe in Japanese children from 7 to 12 years. Saller ('30, p. 94) obtained the mean data for Fehmaraner of 6 to 20 years.

For adults of Hida, Kato ('38, p. 134) finds a distance of 73.6 ± 0.45 for men and 70.7 ± 0.46 for women. Of Ainos, Kato ('37, p. 92) finds for adult men a mean distance of 73.6 ± 0.87 to 79.9 ± 0.64 mm, and for women 71.2 ± 0.70 to 76.9 ± 0.50 mm for various sub-races.

Sexual — In the male (Fig. 113) for two months after birth the growth continues at the same rate as before birth, but at the end

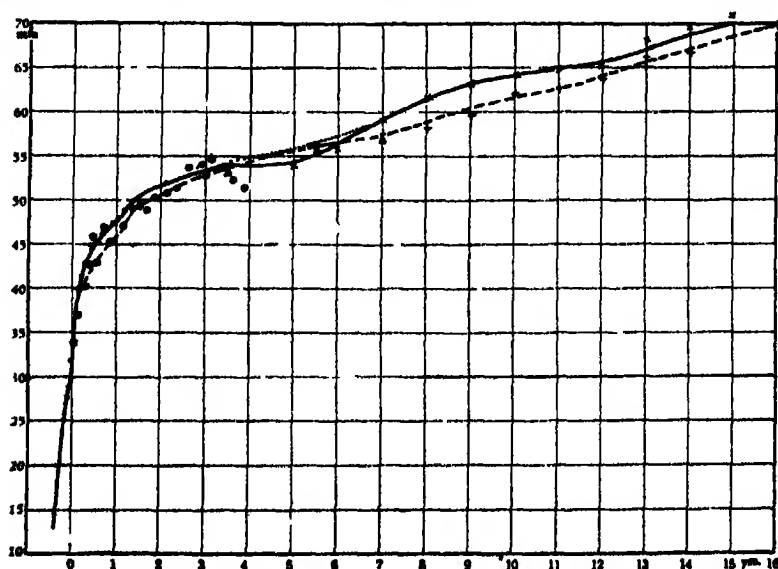


FIG. 113. Mass curves of growth of Nasion to Stomion. Standard (BOA) series. Symbols as in Fig. 1. Dotted line alternative to unbroken male line, years $3\frac{1}{2}$ to 7. Broken line, females.

of that time, height 40 mm., the rate gradually diminishes, from 4.5 p a to 5.5 mm p a. at around 1 year (or 47.5 mm height) down to 2.5 mm. p a at around 2 years. After 3 years occurs a fairly steady slope of about 2 mm p a to 8 years and then of 1.1 mm p a to 15 years when 70 mm. is reached. This is about 5 mm less than the Fehmaraners attained at that age. Our curve of the male shows a sag at 4 to 5 years. This is probably due to sampling, since a new group of boys comes in at this age. The more probable line of development is shown by the dotted line.

The curves of the sexes become differentiated shortly after birth. That of the girls lies from one to three millimeters below that of boys during 7 years from age 6 to age 13 when the curves approach to within a millimeter, and thereafter diverge slightly to stand about 3 to 5 mm apart at maturity.

Social —The mean curve of the standard (BOA) group of boys advances pretty steadily from 58 mm. at 6 years to 67.5 mm at 13 years, or about 1.4 mm p a, and thereafter at about 0.7 mm p a. The curve of the LVD₁₁ series of males lies at 1 to 2 mm. higher, and that of the I series about 3 mm higher. It is of interest that in developmentally defective boys the visceral (or animal) part of the skull should be absolutely larger than standard. The curve of the LVD₁ series is, for the most part, slightly below standard. The M series is 5 to 7 mm, or 10 per cent below, corresponding somewhat with their small stature. The M series gives a sigmoid curve with an inflexion at 13 years.

None of these curves is strictly linear. In general the slope diminishes after 15 or 16 years. Apparently the factors that promote growth of the body in general and molar tooth and maxillary sinus formation especially affect the growth of the face height in particular and this growth-promoting factor is less active in late adolescence than later.

Racial —The growth of the physiognomic upper face height was determined for 3 racial groups. The Nordic group shows the lowest mean size from 9 to 17 years of age, while the Negro group is highest after 12 years of age. There is reason for thinking that in the Nordic group the upper face height has become relatively degenerated. Takabatake's measurements show that the physiognomic upper face height is very low in the Japanese, is still lower than in Europeans, probably associated with their small body size.

Individual—Figure 114 gives curves of facial growth of 5 boys of the LVD₁ series, whose growth was followed for ten years, or more. These curves run more or less nearly parallel. None is

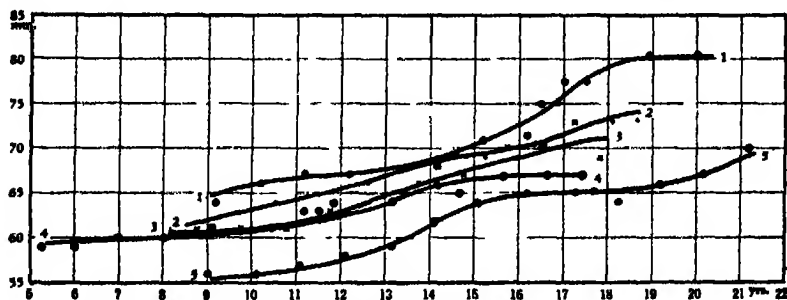


FIG 114 Individual curves of growth of Nasion to Stomion, males of LVD₁ series 1, H F No 20, 2, J C No 10, 3, I J No 18, 4, C H No 83, 5, C D No 30

strictly linear, mostly they are sigmoid. Inflections occur at 13, 16½ and 17 years. No. 5, whose curve lies at the bottom of the series, has not finished growth in facial height at 21 years, while No. 1, which lies at the top, had practically done so at 18½ years. No. 5's growth is retarded, but eventually reaches a stage equal to that of some of those who developed faster in childhood.

Familial—Figure 115 gives mid face growth for the children of two families. The curves are centered at 14 years in the War family at about 68, in the Mea family at about 64.5. The curves are mostly sigmoid. In the War family, inflections occur at 14, 16 and 17 years. In the Mea family at 13½, 15, 17 years.

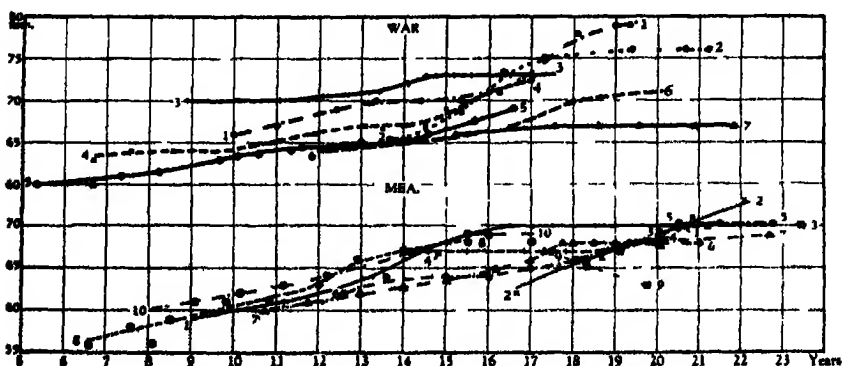


FIG 115 Individual curves of growth of Nasion to Stomion in War and Mea fraternities. War 1, Fk W m, 2, Fd W m, 3, M W f; 4, S W m, 5, G W m; 6, W W m; 7, B W f. Mea 1, G M m, 2, H M m, 3, L M m, 4, Ma M f, 5, Mi M f, 6, N M f; 7, W M m, 8, B M f, 9, E M f, 10, R M f.

There is in both families considerable dispersion. In the War family at 19 years it extends through 13 mm, in the Mea. family at 19 years through 7 mm. However, in both families the growth curves frequently lie close together. In the Mea family they lie so close that one is sometimes embarrassed to draw them as distinct. In both families there is evidence of segregation, inasmuch as there is a partially vacant intermediary space.

In the Mea family No. 9 shows, between 18 and 20 years, a reversed downward slope. During this interval 4 upper pre-molars and molars were extracted. This downward slope has been observed in other cases. Apparently the physiognomic face height may undergo a shrinking after puberty if the upper molars are extracted.

Figure 116 shows curves of face height growth in the Sti family. Two of these curves show a downward slope at 15 to 16½ years, again a lowering of the face in later or post puberty.

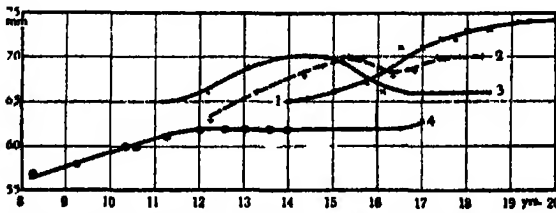


Fig 116 Individual curves of growth of Nasion to Stomion in Sti fraternity 1, CS m, 2, LS m, 3, ES f, 4, MS f

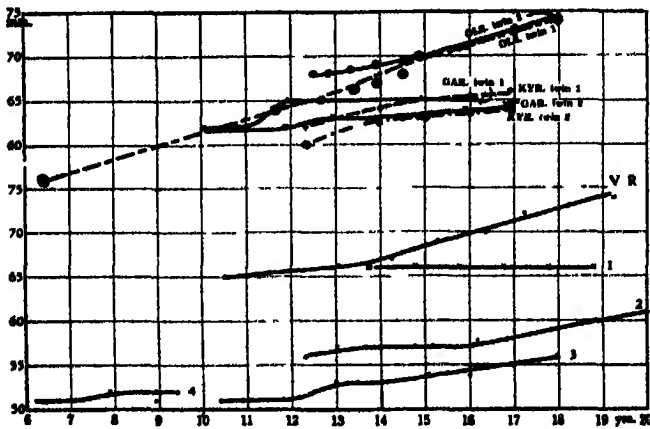


Fig 117 Individual curves of growth of Nasion to Stomion in certain twins and special cases 1, CB f, cretin, 2, AW f, ateliotic, 3, LS f, achondroplastic, 4, AA f, cretin, VR m, microcephalic. All twins monozygotic, Gar. f, others m. Two sets of ordinates.

Twins.—Figure 117 gives curves of face height growth in three pairs of twins. The curves of the Ols twins coincide from 15 to 18 years. The curves of the Gar and Kyr twins lie within 2 mm of each other, but, fortuitously, the curves of a boy and a girl from each pair lie closer together than the curves of the pairs.

Special Cases.—Below on Fig 117 are growth curves of physiognomic middle face height of cretins (1, 4), of dwarfs (2, 3) and of V R, the microcephalic, who has a high middle face.

Summary.—On the whole, the curves of this smaller dimension show less boldness of advance than do the curves of the total morphological face height. This difference results partly from the difference of lengths of the dimension and partly from the presence of more growing units involved in the larger dimension.

The upper face grows much as does morphological face height. The Nordics seem to have this dimension reduced, the low grade feeble-minded have it large, as in anthropoid apes. Its growth participates in that of the body as a whole.

5 Growth of the Dimension Trichion to Nasion (Forehead Height)

General.—This dimension is taken between two rather indefinite points, the difficulties in determining which accurately have been alluded to elsewhere. Consequently the measurement is lacking in great precision. Also the changes during juvenility and adolescence are slight. Much smoothing seems to be called for. Still, trends can be followed.

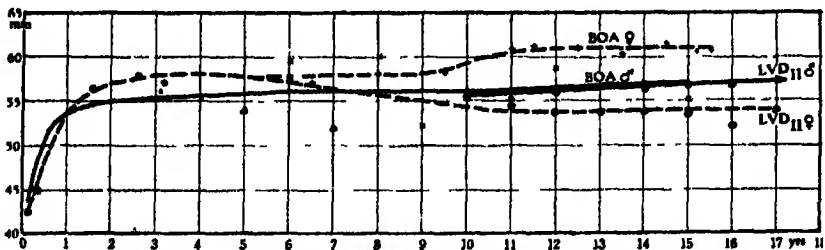


Fig 118 Mass curves of growth of Trichion to Nasion in standard (BOA) series, m and f, also LVD₁₁ m and f

Sexual.—Figure 118 gives the general mass growth curves from infancy to maturity. Beginning, at birth, at around 43 mm, the forehead height has increased to 54 mm. at 1 year. After that, change is extremely slow, only 2 mm. in 13 years, or 0.15 mm. p.a. The LVD₁₁ male series agrees closely with the BOA series from 10

years on. At late puberty the forehead is, in our series, about 58 mm high. Hrdlička ('25, p. 231) found an average of forehead height in adult Old American males to be 66 mm. Also he ('26, p. 3) found a mean forehead height of 64 mm. in male Indians of Panama. Nicolaëff ('26, p. 28) finds a mean "hauteur de la partie frontale" of Ukrainian peasants of 54.3, of "intellectuals" of 56.8. Of Chinese eunuchs Wagenseil ('33, p. 462) finds forehead heights varying from 31 to 56 mm—mode around 47 mm.

In the figure are given two mean curves of female growth in forehead height after childhood. Apparently the girl babies had at first a lower forehead than the boys, but after 1 year it was 2 or 3 mm higher than in the boys and remained 3 or 4 mm. higher in the BOA series as far as it went, to 15 years. However, possibly on account of sampling, the 60, or so, LVD_{II} girls had, after 10 years, a mean forehead height that was smaller than that of the LVD_{II} boys, by about 2 to 3 mm. Accordingly, 15 year old girls of the BOA series had a forehead height of 61 mm, of LVD_{II} series, of 54 mm. Hrdlička ('25, p. 231) found the mean forehead height of "Old American" women to be 64.8 mm, or about 1 mm less than that of men. For the Panama Indians the forehead height of the women was about 60 mm, or 5 mm less than of the men (Hrdlička, '26, p. 3). It is difficult to understand why the mean foreheads of the BOA girls are so high. It is true that the BOA girls were measured by my assistant and she may have taken the nasion lower than I. Abel ('34, p. 337) finds in young girls, 7-19 years, the forehead to be 1 to 4 mm lower than in boys.

Social—At age 15 the curve of the BOA boys lies above the LVD_I and I series, while that of the Mongoloids is highest. The high forehead of the latter is doubtless due to their scant scalp hair which has not developed far down on the forehead. The hair seems to be progressively lost with age. The boys of the I and LVD_{II} series occupy the lowest position. There is thus in our series only a slight correlation between intelligence and forehead height.

Racial.—At 14 years the Nordics occupy a middle position, with Negroes standing higher and Mediterraneans lower. In the Nordic series the growth curve is slightly depressed at around age 12. That of Negroes increases rapidly in sigmoid fashion from 10 to 14 years, with an inflection at 11½ years. The Mediterranean series shows little change in forehead height from 8½ to 14½ years. For

male residents of Berlin, Abel ('34, p 337) found a forehead height increasing from 65.6 at 7-10 years, to 67 mm at 19 26 years. He must have taken nasion lower and trichion higher than I

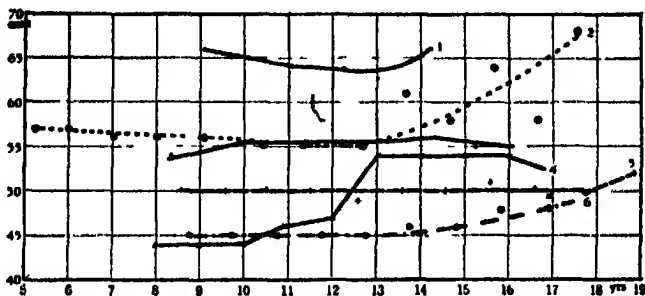


FIG 119 Individual curves of growth of Trichion to Nasion in six boys of the LVD₁ series 1, E T No 14, colored, 2, C H No 83, 3, M H No 2, 4, G P No 5, 5, J C No 10, 6, I J No 18

Individual—Figure 119 gives 6 individual curves of growth of forehead height, each extending mostly through 8 to 12 years. In some cases, as in curve No 6, the forehead height shows no change through many years. Nos 2 and 4 show a striking increase after adolescence sets in. No 1 is the growth curve of forehead height for a Negro. It lies at a decidedly higher level than the other curves. We are indeed led to expect that from the high mean Negro foreheads.

Familial—Figure 120 gives curves of forehead growth for two fraternities. The upper set is for the Mea family. Although at 14 years the two extreme curves lie far apart yet the eight remaining ones are somewhat concentrated at that age, at about 57 mm. Except for curves Nos. 3 and 5, which are somewhat U-shaped, the curves advance slowly with age and in roughly parallel lines, reaching at 22 years to about 59 mm.

The Sti. fraternity shows a somewhat different picture. Curves Nos. 2 and 3 lie close together. No 4 has an extraordinary downward course, indicating a downward growth over the forehead of the hair line. The curves at 14 years are centered around 55 mm.

The War fraternity (Fig. 121) have forehead heights that vary widely at 14 years from 45 to 62 mm, centered at 56.6 mm. For the most part the curves slope upward at about 1.0 mm. p.a. Nos. 1 and 2 run close together.

Twins and Special Cases.—Curves of twins are shown in Fig. 122. The curves of the Gar. twins are fairly close together, those

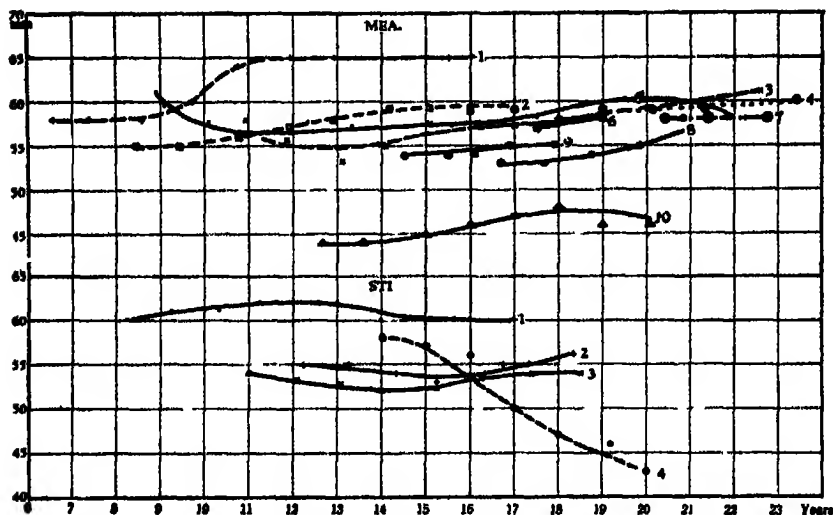


FIG 120 Individual curves of growth of Trichion to Nasion in Mea and Sti fraternities Mea 1, B M f, 2, R M f, 3, W M m, 4, L M m, 5, G M m, 6, E M f, 7, M i M f, 8, H M m, 9, M a M f, 10 N M f Sti 1, M S f, 2, L S m, 3, E S f, 4, C S m Two sets of ordinates

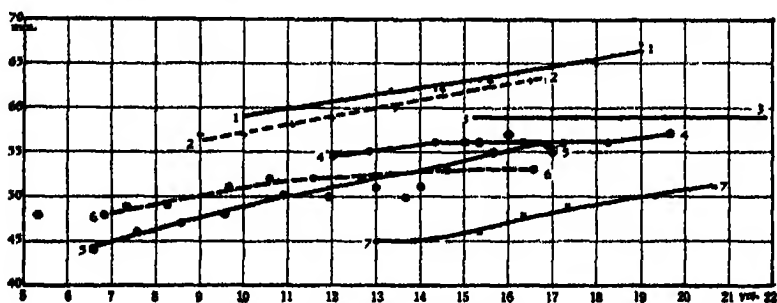


FIG 121 Individual curves of growth of Trichion to Nasion in War fraternity 1, Fk W m, 2, M W f, 3, B W f; 4, W W m, 5, G W m, 6, S W m, 7, Fd W m

of the Sha twins even more so, but the significance of this fact is somewhat diminished owing to the considerable smoothing that was entailed.

Two cretins, No. 2 and No. 1, have rather high foreheads. The dwarf, No. 3, has a forehead height of only 46 mm. The fairly tall microcephalic, V.R., has a retreating forehead with a height of 47 mm.

Summary—The forehead height shows, of all the child's dimensions, perhaps the least relative change. This is, of course, because the cranium is so highly developed at birth. Of the head

it is the visceral part that undergoes the most striking post-natal development.

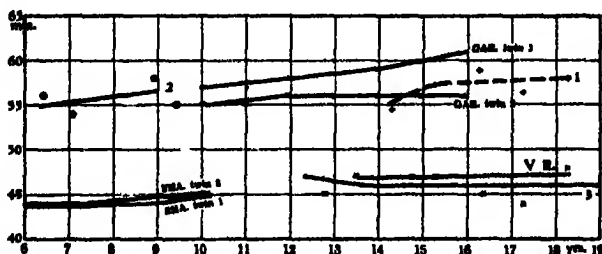


FIG 122 Individual curves of growth of Trichion to Nasion in twins and special cases 1, C B f, cretin, 2, A A f, cretin, 3, A W f, ateliotic V R m, microcephalic Twins monozygotic, Gar f, Sha m

6 Growth of the Dimension Stomion to Gnathion

General—This dimension is the height of lower face (Martin, '28, p. 189, No. 28). It was not measured directly, but both stomion (between closed lips) and gnathion were measured from nasion. The difference between the two measurements was taken as the height of lower face. It measures the physiognomic "chin and upper lip"

Rather few measurements of the stomion-gnathion distance on growing children have been taken. In both Hellman's ('27, '32, '33) and Goldstein's ('36) papers measurements were made between the gnathion and the gum papilla that lies between the mandibular central incisors (infradentale, of Martin). Abel ('34, p. 285) and Young ('37, p. 450) measured from the cutting edge of the lower incisors to submental point. Of course, all students of facial changes based on the skull must measure from bony points. My purpose has been less to enable a comparison to be made with skull measurements (and series of such measurements made on young skulls seem not to have been made), than to follow the changes in form of the face in the living, growing child. Accordingly, I have measured from the oral fissure (lips being in relaxed position) to the skin covering the gnathion.

Phylogenetically, the body of the mandible has undergone a reduction in height and thickness. For, as compared with recent man, it is much heavier not only in the anthropoids but also in most of the jaws of fossil man. *e.g.*, Mauer, La Quina, La Chapelle aux Saints, Krapina H. It is relatively receding below in the gorilla, chimpanzee and orang, less so in the gibbon (Symphalanger)

Sexual—Figure 123 gives the mass curve of growth of the height of the lower face. This proceeds from the minute dimension of 6 mm. at the end of 4 lunar months to 20 mm. at birth, or a little

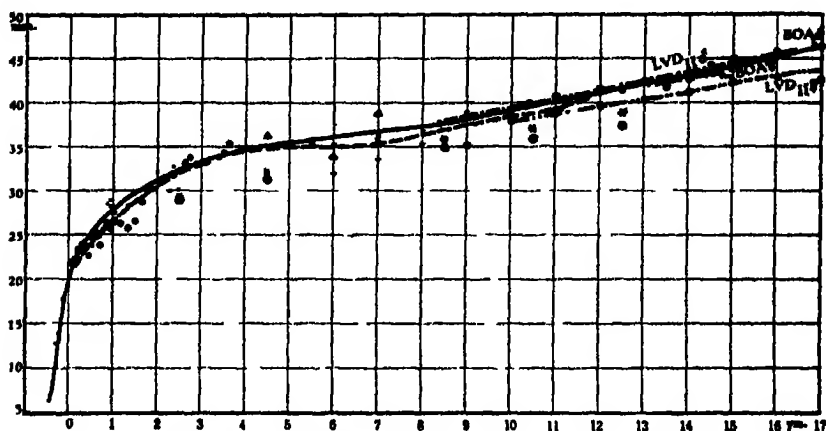


FIG 123 Mass curves of growth of Stomion to Gnathion (lower jaw height) in standard (BOA) series, also LVD₁₁ m and f. For symbols see Fig 1. Certain findings of Young are indicated, for males, by asterisk, for females by encircled point.

more than one-third of the mature size. It grows rapidly for six months after birth, about 10 mm. p. a., and then begins the slowing up process, there is an increment of 6 mm. p. a. in the second half of the first year, 3 mm. p. a. in the second year, 2.5 mm. p. a. in the third year, down to 0.5 mm. in the 6th year. At 7 or 8 years, when the permanent incisors are appearing, the height of the jaw at symphysis is gaining and the growth is accelerated so that at 9 to 11 years the rate is 2 mm. p. a. and continues at this rate until near the end of puberty when a height of 46 mm. is reached. Young ('37) by a somewhat different technique finds means whose values are represented on the graph by asterisks. These points are about 3 mm. below my smoothed values for the BOA males.

The curve of the lower face lengths of the females is probably constantly about 1 mm. below that of the males. The standard female curve touches the male curve during the thirteenth year and then falls below it again. Perhaps general growth processes as well as tooth formation play a part in this adolescent increase in rate of growth. The large circles indicate the position of the mean values for the females as found by Young ('37, p. 462), in London girls. These points lie from $\frac{1}{2}$ to $2\frac{1}{2}$ mm. below those of the boys. In Abel's ('34, p. 286) somewhat fragmentary series the girls have

mandibles that are 1 to 4 mm. lower at symphysis than those of the boys

Social.—At 14 years of age the order of social growth curves proceeding from above downward is—standard (BOA 43 mm), I, LVD_I and M, 40 mm. Apparently the standard boys have the best developed chin and lower jaw after 11 years, and the Mongoloids the least. As between the LVD_I and I series there is no significant difference.

Racial.—In my series there is no significant difference between Nordics and Mediterraneans. The Negroes, however, have consistently a higher lower-jaw than the Europeans. Martin ('28, p. 972) says in effect. The oft-repeated assertion that the mandible of the Negro and all colored races is larger than that of the European is not proven. My measurements seem to indicate that the mean height of the lower face is greater in American Negroes than in Europeans by about 2 mm.

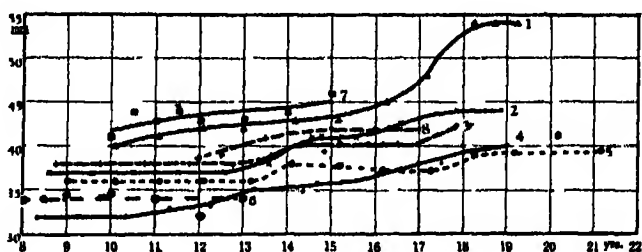


FIG 124 Individual curves of growth of Stomion to Gnathion in eight individuals of LVD groups, all male except C C 1, H M No 40, 2, J C No 10, 3, I J No 18, 4, M H No 2, 5, C D No 30, 6, C C No 133, 7, L B No 19, colored, 8, G W No 111, colored

Individual.—Figure 124 shows the course of growth of the lower face height for seven boys and one girl. In general, the growth is slow between 8 and 12 years of age. Usually there is a rapid increase of slope with an early inflexion at 12½, 13, and 14 years, and a later inflexion at 16½, 17, 17½ and 18 years. It is really remarkable that no maximum curvature occurs at between 14½ and 16, the period of the adolescent spurt. It looks as though the first group of inflexions was related to the juvenile, the second to the adolescent spurt, only the maximum growth of the mandible occurs about two years later than that of the extremities, and is chiefly due to the development of the second and third molars. There is much variation in the individual curves of growth. No. 4

shows slight change, while No 1 shows marked change in rate of growth at puberty and his profile shows a correspondingly heavy lower jaw. Compare the profile of No 1 with that of No 4 in Figs 7 and 9 respectively of Pl VII, Davenport, '39. See also Cushing ('12), Fig 212

In Fig 124 there is one girl (No. 6) whose curve lies near the bottom of the series. Curves 7 and 8 are of American Negroes. They lie above the average of the curves.

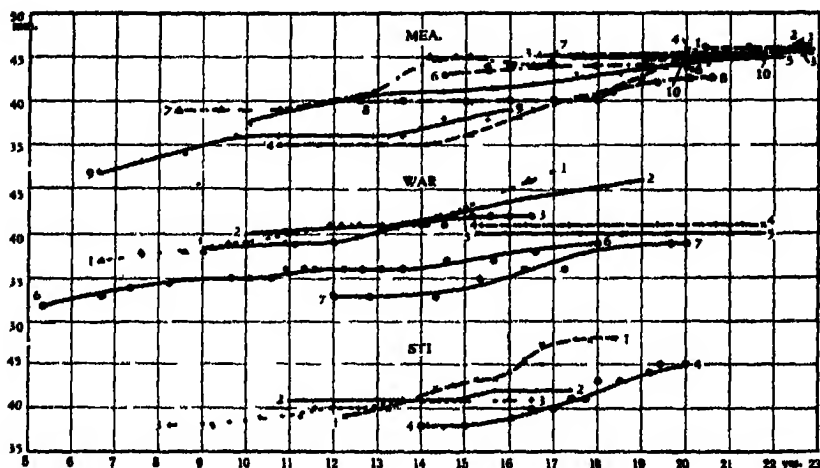


FIG 125 Individual curves of growth of Stomion to Gnathion in three fraternities
Mea 1, Mi M f, 2, H M m, 3, L M m, 4, W M m, 5, G M m, 6, Ma M f, 7, R M f, 8, N M f, 9, B M f, 10, E M f War 1, S W m, 2, Fk W m, 3, M W f, 4, Pd W m, 5, B W f, 6, G W m, 7, W.W m Sti. 1, L.S m, 2, ES f, 3, MS f, 4, CS m
Three sets of ordinates

Familial—Figure 125 shows the growth of lower face height in ten members of the Mea fraternity, seven of the War fraternity and four of the Sti fraternity. The mean of the Mea. fraternity at 16 years is 41.5 mm. Of the Sti. family it is 41.4, practically the same. Although the members of the Sti family are in general much shorter than those of the Mea. family at least one curve (No 1) of the former family reaches to the greater size, viz 48 mm. at 18 years, while none of the Mea. boys exceeds 45 mm. at 18 years. The Mea children start at a lower level than the Sti children. On the whole, the Mea. children have the smaller lower face height. Also, for the most part the lower jaw stops growing earlier in the Mea fraternity

In the War. fraternity the mean of the curves at 16 years is 40.8 The girls (Nos. 3 and 5) grow slowly Most of the boys, especially 1, 6 and 7, show a marked spurt in growth of lower face height after puberty begins

The Mea. fraternity is characterized by marked increase of slope at an early age, as compared with the Sti. family. The War. family curves show few steep slopes

In the Mea. family curves 1, 2, 3, 4, 5, 6 and 10 coincide to within 2 mm at around 45 mm, at 19 to 22 years of age Nos 2 and 3 run closely parallel courses In the Sti. family curves of the girls Nos 2 and 3 are close together and those of the boys, Nos 1 and 4, have a parallel course In the War family curves 1, 2 and 3 coincide at age 14 and are nowhere far apart Nos. 4 and 5, measured after adolescence, differ by only 1 mm Nos 6 and 7 run parallel courses, 3 to 1 mm. apart There is evidence of family resemblance, but, inside each family, a considerable variability

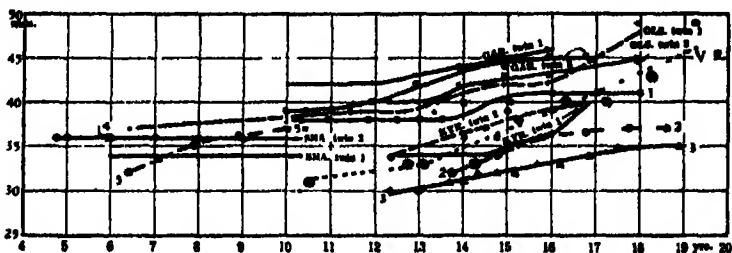


FIG 126 Individual curves of growth of Stomion to Gnathion in twins and special cases 1, L.S f, achondroplastic, 2, C.B f, cretin, 3, A.W f, ateliotic, 4, H.L. m, cretin, 5, A.A f, cretin All twins monozygotic and male, except Gar f

Twins and Special Cases —Figure 126 shows curves of growth of the Gar twins These lie fairly close together, 1 mm apart at 13 to 16 years The curves of lower face height of the Ols. twins are never more than 1 mm apart from 12 to 17 years The Sha. twins from 6 to 10½ years remain 2 mm. apart The curves of the Kyr twins lie parallel and about 2 mm apart

No. 3, ateliotic dwarf, lies at the bottom of the series with a lower face height of only 31 mm at 14 years No 1, apparently a pituitary dwarf, occupies a middle position. Nos 2, 5 and 4 are cretins. Their curves occupy median positions. The curve of the microcephalic (V.R.) advances rapidly from 31 to 46 mm in 9 years, or 1.67 mm. p.a. Despite his mediocre stature he acquires a high mandible.

Summary —The lower face height increases during the first two years post-natal, as the milk dentition is getting ready to cut, grows slowly during childhood, advances rapidly as adolescence sets in. It is slightly larger in the standard boys than the feeble-minded, corresponding with their greater stature. It is somewhat greater in Negroes than whites. It shows great individual variation in growth, producing the marked variation in lower jaw development of the adult. In different families the chin shows family similarities despite a good deal of variation in each family. In monozygotic twins growth of lower face proceeds in very similar fashion. In dwarfs the dimension is particularly small, in cretins mediocre. In one microcephalic, growth of the lower jaw is, in relation to stature, proportionally rapid. Throughout, the growth of this dimension seems associated with that of the body as a whole, although the spurts of growth of the jaw seem to lag behind, a year or two, as do also the periods of molar tooth development.

VI AGE CHANGES IN PERCENTAGE RATIOS OF THE SEGMENTS OF THE FACE

It has long been known that at birth the visceral part of the head is less well developed than the cerebral part. If, as has been suggested, the precocious development of the brain is necessary in order that the enormously complicated organ should be, at the time of birth, ready to begin to function and to perfect its functioning as early as possible, this demand is not so insistent in the case of the visceral part of the face. Here the functions are relatively simple: the support of the sense organs and the teeth and structures for the attachment of muscles used in the expression of the emotions and for sucking and chewing. Fig. 132 shows analytically the changes in facial proportions from before birth to maturity.

In the present study we shall trace in more detail the proportional growth changes in the visceral part of the face.

1 *Morphologic to Physiognomic Face Height*

Figure 127 shows the age changes in the percentage ratio of nasion-gnathion to trichion-gnathion from birth to post-adolescence as derived from mass studies.

Sexual—In the male the curve advances with some irregularities from about 59 per cent at birth to 67 per cent. During the first year there is an extraordinarily rapid drop of 2 points. By the end of the first year the ratio is back at the birth level and then

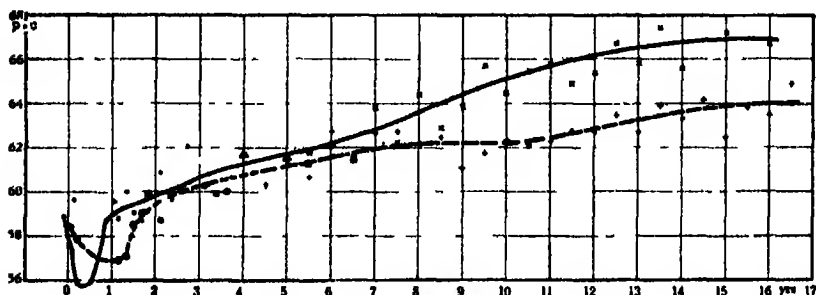


FIG 127 Mass curves of change with age of percentage ratio of Morphologic to Physiognomic Face Height Standard series Symbols as in Fig 1

advances very slowly to about 7 years, and then more rapidly for a time

The meaning of the drop during the first year is difficult to determine, the curve of height of the lower jaw shows a somewhat similar depression, and such depression in the curve of that organ would of course affect the curve of the morphologic face height relatively more than that of physiognomic face height With living young infants both of these dimensions are extraordinarily hard to measure since the infant tends to cry while being measured and this involves opening the mouth widely At such times, for measuring, the mouth has to be forcibly closed by the attending nurse and this may be responsible for the temporary reduction in vertical height of the face and especially the region around the mouth

The curve for males of proportional lower face height rises strikingly after 7 years in sigmoid fashion, the inflection standing at about 9 years By 16 years the curve has become asymptotic to 67 per cent, which is about the maximum mean ratio

The mean curve of the female also shows the dip during the first post-natal year—broader but not so deep as that of the male. From 2 to 7 years the curve of the female ratios lies about $\frac{1}{2}$ a point below that of the male. It ceases to rise from 8 to 11 years and then starts upward again about 4 to 3 points below the male curve, becoming asymptotic at 64 per cent. Apparently the female has a disproportionately small morphologic face height as

compared with the male In respect to face size the male seems more acromegalic than the female.

Social—The curve of BOA boys, despite their greater stature, at 13 years lies at the bottom of the series (excepting the mongoloid dwarfs) Next above in order are the LVD_{II} males, the LVD_I series and the I series This is the order of decreasing intelligence and forehead height

The boys of the I series show little change in proportions from juvenility to post-puberty, while in those of the LVD_{II} series there is a rapid increase with age in the index in both sexes, and this is due to increase in jaw height. The LVD_I curve shows a change in direction of slope at 12 years, as though the forehead then began to grow in height more rapidly for a few years than the rest of the face Finally, the mongoloid dwarfs have a relatively low visceral part of the face. But this is in part due to the fact that, chiefly on account of sparsity of the terminal hair-coat, the scalp does not extend far on to the forehead, hence the denominator of this ratio is large

On account of the varied slopes of the age-ratio curves the relative position of the curves varies with age At 9 years the LVD_{II} curves are below the standard and the mongoloid above the others (except the I series) The mongoloids apparently lose their scalp hair above the forehead early.

Racial.—In the mean growth curves the Mediterranean group lies on top, indicating that it has relatively the lowest forehead At 12 years the Negroes come next, and the curve of the Nordics lies at the bottom, but after 14 years the curves of the Negroes and the Nordics are practically superimposed

All three curves show a steep upward slope during juvenility, a slight downward slope during adolescence and then a steep upward slope during puberty.

Individual.—Figure 128 gives age-index curves of morphologic to physiognomic face height for 5 boys of the LVD_I series. All are of one type, namely, upward slope during juvenility, downward slope during adolescence and early puberty; upward again in later and post-puberty. That is, during late adolescence the difference between morphologic and physiognomic face height increases. the forehead is relatively a larger part of the face height and this increase may be of the order of 1 to 4 points of the index

Familial.—Figure 129 gives the age-ratio curves for members

of the Sti. and War. fraternities In the Sti. fraternity two curves agree fairly closely in position and form, but the other two are extremely unlike, and neither show the characteristic wave.

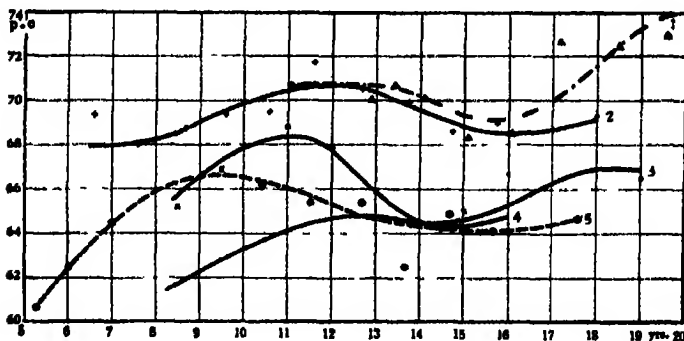


FIG 128 Individual curves of change with age of percentage ratio of Morphologic to Physiognomic Face Height in five boys of Nordic stock, LVD series 1, RB No 81, 2, JB No 54, 3, MH No 2, 4, GH No 82, 5, CH No 83

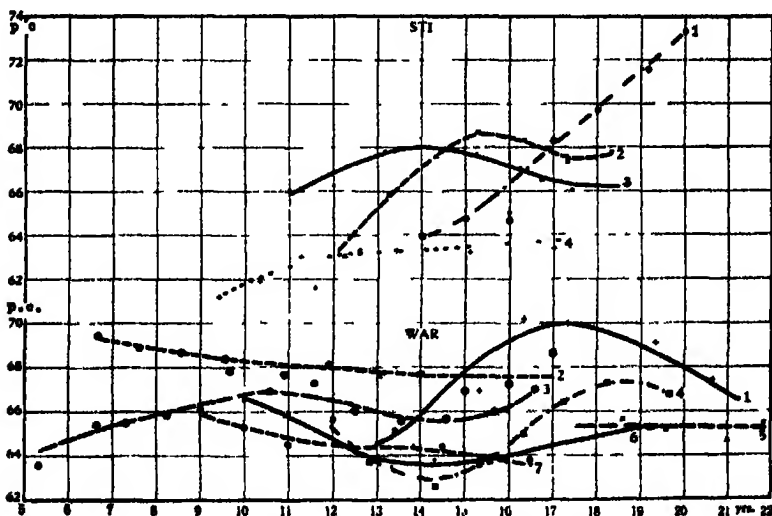


FIG 129 Individual curves of change with age of percentage ratio of Morphologic to Physiognomic Face Height in two fraternities Sti. 1, CS m, 2, LS m, 3, ES f, 4, MS f War. 1, Fd W m, 2, SW m, 3, GW m, 4, WW m, 5, BW f, 6, Fk W m, 7, MW f Two sets of ordinates

In the War. fraternity three curves intersect during the 16th year at 64 per cent, but for one boy (No. 1) the curve lies rather widely apart.

Figure 130 shows the curves for the Mea. family. These differ widely, ranging at 16 years from 62 to 69 per cent, or 7 points. The most complete curves all show the wave, but the bottom of the

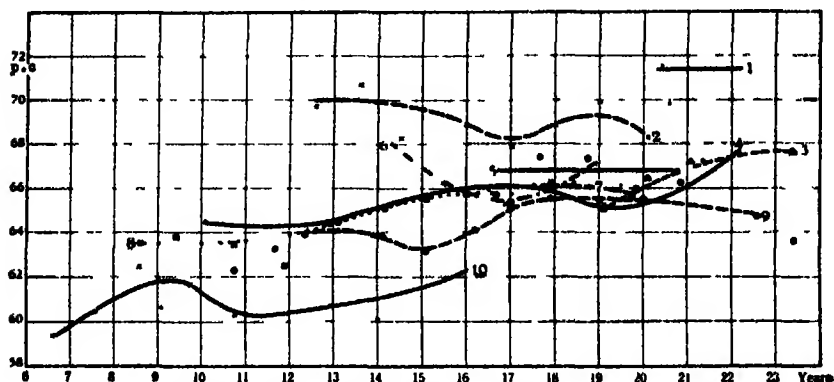


FIG 130 Individual curves of change with age of percentage ratio of Morphologic to Physiognomic Face Height in Mea fraternity 1, M₁ M f; 2, N M f; 3, L M m, 4, G M m, 5, H M m, 6, M_a M f, 7, E M f, 8, R M f, 9, W M m, 10, B M f

depression occurs at 11, 12, 15, 17 and 19½ years in respective curves. The curves of No 4 (male) and No. 8 (female) run close together at all ages. At age 13, 3 curves are close together, at age 20, 5 curves are within 2 points. Curves Nos. 1 and 2 are the most divergent from the main group.

Special Cases —The age-ratio curves of two pairs of monozygotic twins are shown at the bottom of Fig 131. The curves of the Gar.

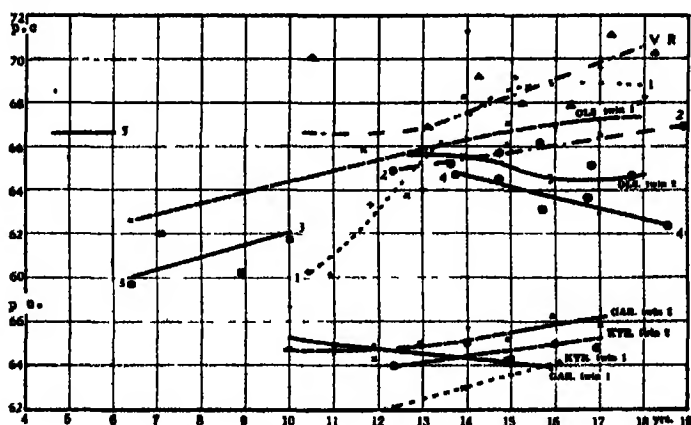


FIG 131 Individual curves of change with age of percentage ratio of Morphologic to Physiognomic Face Height in twins and special cases 1, L S f, achondroplastic, 2, A W f, atelotic, 3, A A f, cretin, 4, C B f, cretin; 5, H L m, cretin. V R m, microcephalic. Three pairs of monozygotic twins Gar f, Kyr and Qls. m. Two sets of ordinates

twins are fairly close together and intersect at 12 years, at the 65 per cent level. The curves of the Kyr. twins are from 2 to 1 points apart and run nearly parallel courses. The two curves of the Ols. twins diverge from a common point (65.5) at 12 years to become $2\frac{1}{2}$ points apart at 18 years. Curves 1 and 2 are of a pituitary and an ateliotic dwarf girl respectively. Curve V R. is of a microcephalic and has a high ratio (70 per cent), indicating a low forehead and high mandible. Nos. 3 and 4 are curves of two cretinous girls; they have a low position indicating relatively high foreheads or low mandibles. Curve 5 is of a young cretin whose ratio is $66\frac{1}{2}$ per cent.

Summary —The age-curve of morphologic to physiognomic face height is, on the whole, a rising one, and is higher in boys than girls. The level of the curve rises with decreasing intelligence, it is low in Negroes, high in Mediterraneans. Individuals usually show a wave-like age-curve, largely due to growth changes in sinuses and alveoli, but lying at very diverse levels. The ratio is concentrated at different points in different families. The age curve may be slightly unlike in monozygotic twins, it is especially high in a microcephalic.

2 Segments of the Morphological Face Height

The morphological height of the face may be divided into 3 segments which together equal 100. These are: nasion to subnasale (or nasal height), gnathion to stomion (or lower jaw, herein sometimes called "chin" for brevity), and subnasale to stomion (the upper lip, including the alveolar process of the maxilla).

Sexual —Figure 132 gives a general view of the mean changes with age of these three proportional parts. In the male, during the latter half of gestation the lower jaw increases from 32 to 38 per cent of the M.F.H., the nose decreases from about 50 to 39 per cent while the upper lip increases from 18 to 21 per cent. This increase of the jaws is, of course, a consequence of the development of the milk dentition. After this episode is finished and all the teeth in front of the molars are cut, by 2 years of age, the proportions of the face undergo few marked changes for a while. At 4 years, however, the jaws seem again to grow at the expense of the nasal height, as the development of the permanent incisors proceeds. After 6 years the development of nasal height is predominating except at 11 to 12 years when the permanent canine and the pre-

molars are getting ready to cut. Also, at the pubescent spurt of growth the lower jaw height increases and the "prominent chin" of maturity makes its appearance, while the nose height is relatively

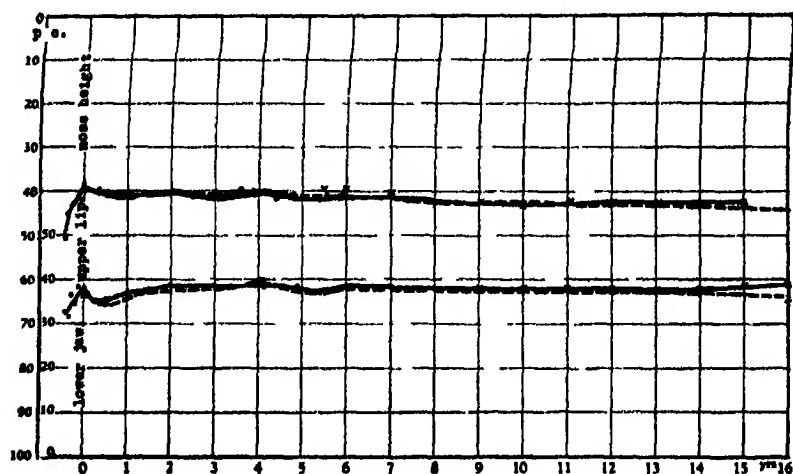


FIG 132 Mass curves of change with age of Proportional Heights of Segments of Morphologic Face Height Standard series Unbroken line, male, broken line, female Two sets of ordinates at left, measuring from Nasion and from Gnathion respectively

stabilized Hellman's ('32) Table XIV shows that, in his series also, relative lower face height ceases to diminish after about 13 years

The curve of proportional height of segments of the face of girls runs very close to that of boys The proportional chin height of girls seems slightly less than that of boys, and the nose is correspondingly larger, after adolescence, at least.

Social.—In the four groups that differ in mean intelligence the mean age curves of the facial proportions are not far apart. The standard (BOA) group occupies a middle position The mongoloid dwarfs have a relatively low nose ('39, p 194) and high chin, while in the I series the nose is high and the chin low ('39, p. 192).

Racial —Of the 4 racial groups of the LVD_{II} series, the Negroes and Italians in our series have a low nose, while that of the Jews is high. The relative chin height of the Jews is least, that of the Italians and Nordic series higher. The result is that the alveolar part of the maxilla of the Italians is relatively low The high maxilla of Negroes appears early in fetal life (Limson, '38, p. 220).

Individual.—Figure 133 gives age changes of the proportional face segments for 5 boys. Most show little change before 12 years

of age After that age the relative nasal height tends to increase, reaching a maximum at 16 to 17 years This is the age when absolute nasal height is increasing rapidly ('39, p 194) After 18 years the proportional nasal height becomes stabilized

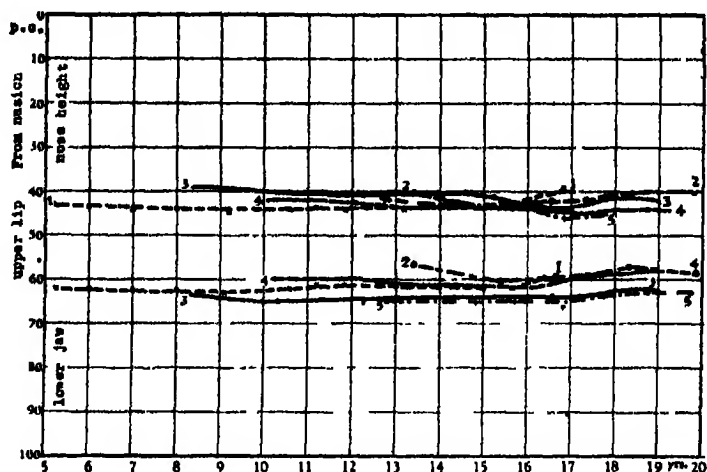


FIG 133 Individual curves of change with age of Proportional Heights of Segments of Morphologic Face Height in five boys of LVD series 1, CH No 83, 2, GH No 126, 3, MH No 2, 4, HM No 43; 5, AT No 1

The proportional lower jaw height begins to decrease at about 12 years, reaches a minimum at about 16 years, and increases to about 18½ years. It follows that there is, in many cases, a relative minimum of the alveolar process of the maxilla at about 17 years. For example this process has in No 3 a relative height of 23½ points at 14, and 20¼ points at 17 years This is an age when the morphological face height is growing rapidly, but the growth is chiefly in nose and lower jaw.

Familial.—Figure 134 shows the age-change curves for members of the Hic. and Mea fraternities The height of upper lip is slightly the greater in the Hic fraternity (above) Also lower jaw height is increasing rapidly in the Hic family at 13–14 years, at a time when it is a minimum in two members of the Mea fraternity There is a great similarity of chin height at 19½ years in the Mea. fraternity, so that the lines which converge here are almost confluent. On the other hand dispersion is great at 13 years. This is partly because the sexes are combined in the figure and they differ in the age of greatest change in facial proportions.

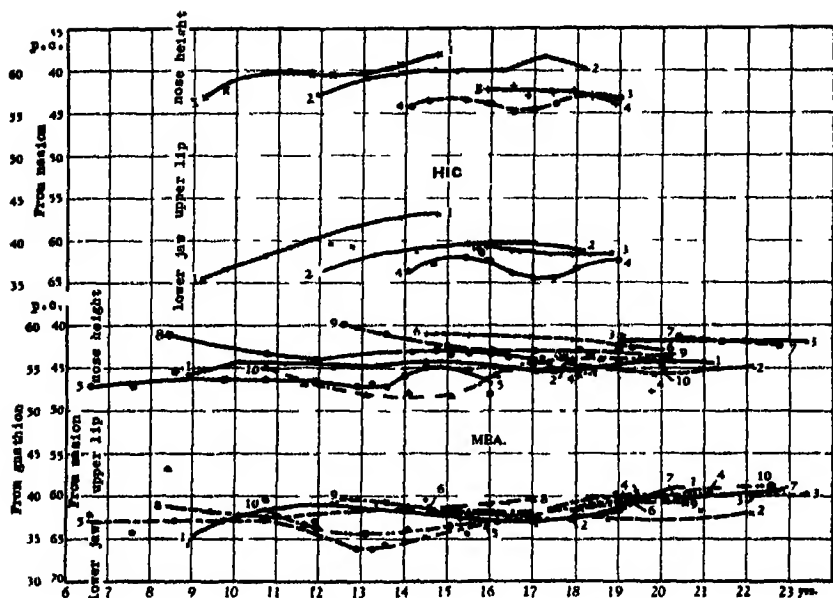


FIG 134 Individual curves of change with age of Proportional Heights of Segments of Morphologic Face Height for two fraternities Hic 1, Ch H m, 2, Ce H m, 3, H H m, 4, F H m Mea 1, G M m, 2, H M m, 3, L M m, 4, E M f, 5, B M f, 6, M a M f, 7, M i M f, 8, R M f, 9, N M f, 10, W M m Note that Nasion to Stomion proportional distances are plotted above the fraternity designated and Stomion to Gnathion below, in each fraternity Two sets of ordinates The inner ordinate column has its zero at nasion, the outer its zero at gnathion

Twins and Special Cases—Figure 135 shows, above, changes with age of the Gar. twins and below of the Ols. and Sha. twins In the case of the Gar. twins No. 1 shows almost no change, while No. 2 shows a marked change in proportions at the thirteenth year, in that the nose becomes relatively lower and the chin relatively higher—in both cases to the extent of 3 or 4 points

In the case of the Ols. twins, No. 2 shows less change than No. 1 In the latter the relative nose height increases to 14 years, then decreases, while the chin height changes in the reciprocal fashion.

Of the dwarfs, ateliotic No. 2 shows, from 12 to 20 years, a low relatively decreasing nasal height and correspondingly increasing chin height No. 1, achondroplastic, has a relatively low nose and high chin. The microcephalic, V R., has a rather high and unvariable nose and low, but increasing chin.

Summary.—Changes in proportion of the 3 segments of the morphologic face height are closely bound up with enlargements of the maxillary sinus, alveoli and teeth. Boys have proportionally

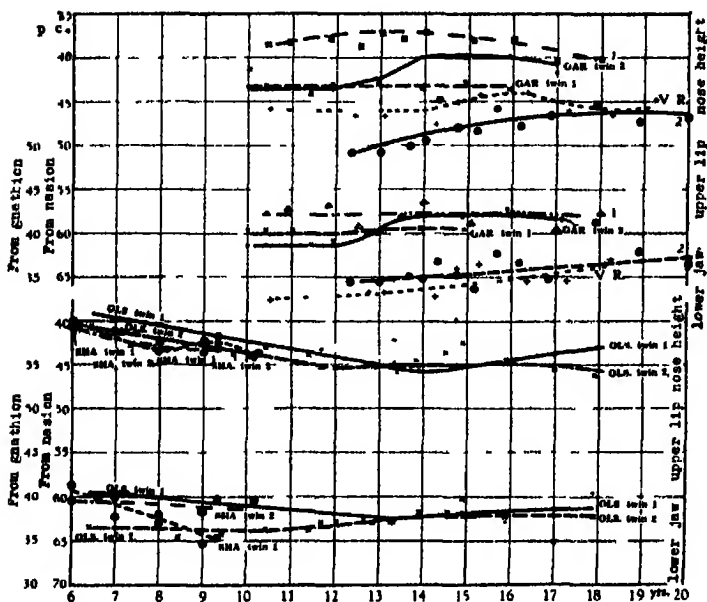


FIG 135 Individual curves of change with age of Proportional Heights of Segments of Morphologic Face Height for some twins and special cases 1, L S f, achondroplastic, 2, A W f, ateliotic, V R m, microcephalic

slightly higher chins than girls. Large nosed idiots have proportionately low chins and small nosed mongoloids the reverse. A slight racial difference appears. The individual differences in proportional chin height are sufficient to be striking on profile portraits. In two families shown the proportions of features tend to become most alike during late puberty. In twins the curves lack identity since tooth-cutting and sinus enlargements appear to be not always simultaneous. The microcephalic has a low chin associated with fairly high nose.

3 Lower Jaw Height in Relation to Morphologic Face Height

The changes in relative height of the segments of the face which have been considered in the last section are rather complex and, on account of carrying 2 sets of curves, are drawn on a small scale. It has accordingly seemed desirable to give growth curves of one of the sets, viz. stomion to gnathion, on a larger scale.

Sexual.—Figure 136 shows the mass age changes in percentage ratio of stomion-to-gnathion, lower jaw or "chin," height to M F H. from mid-gestation to maturity. The lower jaw height increases

rapidly before birth while the nasal height is proportionately reduced despite its rapid absolute increase ('39, p 187) The chin proportion at birth is about 37.5 per cent. After birth the upper

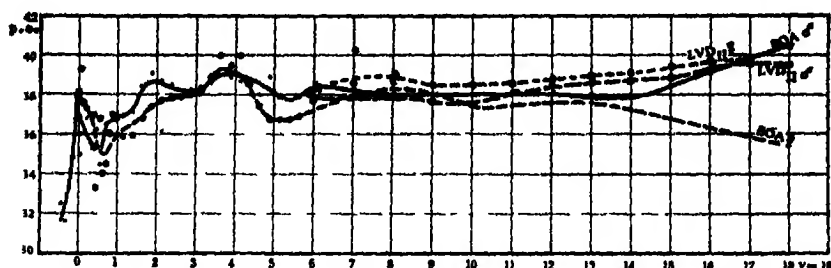


FIG 136 Mass curves of change with age of percentage ratio of Lower Jaw Height to Morphologic Face Height for Standard or BOA series Also, from 6 years onward, for LVD₁₁ series Males and females

part of the M.F.H. takes again the upper hand for about 6 months, possibly in part owing to the development of the teeth and alveolar process of the maxilla, possibly in part owing to outside pressure applied to the jaw to keep the mouth closed To be sure, the lower incisors usually precede by a few weeks the upper incisors in eruption, but they are smaller teeth than the upper incisors and make less call on the building up of the alveoli. After 8 or 9 months post-partum, on the average, the lower jaw begins to build up more rapidly than the upper until 2 or 3 years of age.

As Fig 136 shows, the further course of the proportional height of lower jaw is variable, until about 7 years by which time the permanent incisors are fully formed. Thereafter to maturity the lower jaw grows proportionally to the rest of the face or slightly more rapidly, except at around 8 and 9 years at which time the maxillary sinus is typically enlarging most rapidly (Davis, '14, p. 81). Goldstein ('36, p. 49) also finds a depression in the mean age-change curve of the lower jaw at about 9 to 10 years. Hellman ('32, p. 790) using a slightly different technique than mine finds a minimum for boys at 7.7 years and for girls at about 8.4 years; but still lower proportions are found at 18.6 years for boys and at 20 years for girls. The most striking difference between girls and boys in the proportional lower jaw height is the small value for the girls as compared with the boys at 15 to 18 years. Hellman ('32, p. 794) finds the proportion of lower jaw at 18 years to be about $\frac{1}{2}$ a point less in girls than boys I think it probable that my curve for BOA girls runs too low The measurements upon which the

girls' curve depend were made by my assistant. In the LVD_{II} series in which I made the face measurements on both sexes, the girls' curve is as shown in Fig. 136, located close to that of the boys at 17 years

Social.—The children of the Idiot series have the proportionally smallest lower jaw and those of the mongoloid series the next smallest. The nasal height of the Idiots is about the same as that of the standard ('39, p. 192) while the M.F.H. is mediocre, accordingly their proportional height of upper face is large and that of the chin small.

Racial.—The proportional height of the Nordic lower jaw is mediocre while that of the Negro is high. This is a consequence of the absolutely very high lower jaw of this race and the mediocre nasal height ('39, p. 193) The Mediterranean group has proportionally the lowest lower jaw

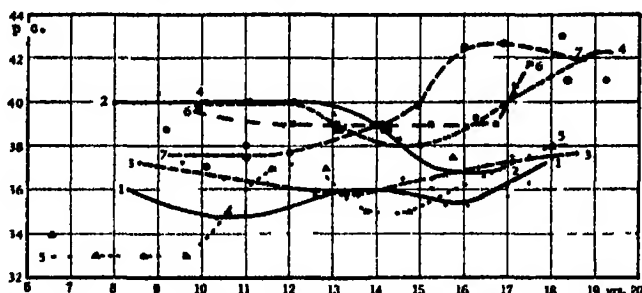


FIG 137 Individual curves of change with age of percentage ratio of Lower Jaw Height to Morphologic Face Height for seven boys of LVD series 1, M H No 2, 2, W M No 8, 3, J C No 10, 4, H M No 43, 5, J B No 54, 6, M G No 95, 7, T V No 65

Individual.—Figure 137 shows age-changes in individuals of the proportion of lower jaw to M.F.H. The curves are of different types. Some, like No. 7, ascend as a sigmoid, others, like No. 2 ('39, Pls. VII and VIII, No. 9), descend. Some are concave above (No. 4). Some show a wavy type (Nos 3 and 5) No 6 is striking because of the absence of change in proportion from 12 to 16½ years; followed by a sudden increase at 16½ years. The boy's profile is shown ('39, Pls. VII and VIII, No. 10) after the spurt of growth of the lower jaw has occurred. All of these curves are of boys whose photographs and changes in facial outlines are on Plates VII and VIII of the 1939 paper.

In Fig. 138 are shown age-changes of different types in the lower jaw/M.F.H. index. Above, are curves of 2 boys of the LVD_{II} series, in each of whom for 8 years this index remained

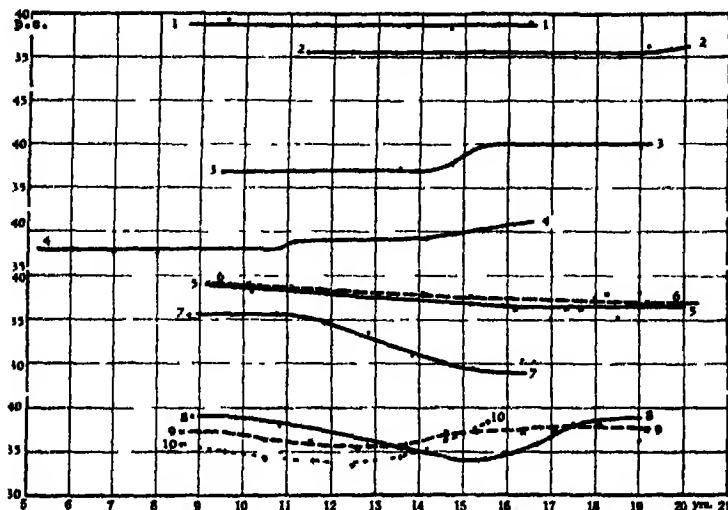


FIG 138 Individual curves of change with age of percentage ratio of Lower Jaw Height to Morphologic Face Height for 10 boys of both LVD series, showing different types of age change 1, T F D_I No 27, 2, F C D_I No 45, 3, F W D_{II} No 24, 4, C H D_{II} No 83, 5, H F D_{II} No 20, 6, C D D_{II} No 30, 7, R C D_{II} No 74, 8, H M D_{II} No 25, 9, J C D_{II} 10, 10, F C D_{II} No 75 Five sets of ordinates

constant. They are both of rather low grade. The I.Q. of No. 1 varied from 35 to 27, of No. 2 from 51 to 44. Their jaws did not increase disproportionately with age.

Next below are two curves showing a slight rising of the chin proportion. No. 3 has an I.Q. of 36 falling to 29, No. 4 of 63.

Next below are three curves of a descending type. The I.Q.'s of the boys are as follows: 5, 57-44, 6, 30-26, 7, 33 ±.

At the bottom are three curves of the wavy type, ascending after puberty. The I.Q.'s of these boys are as follows: 8, 72 falling to 64 at 11 years, 9, 102 falling steadily to 70 at 13 years; 10, 85 at 9 years falling to 70 at 13 years. The only generalization that seems to emerge from these data is that the boys of the wave like type of proportional chin growth have a higher mentality than boys of the other types.

Familial.—Figure 139 contains age-change curves of relative lower jaw height for 3 families. Above, in the Mea. family of 10, the curves fall into 2 groups; the smaller group containing 1 boy

and 1 girl (Nos 2 and 7) In general, the curves of this family are of the wavy type The age of the minimum varies between $13\frac{1}{2}$ and 16 years The curves tend to become horizontal after age 19, but Nos 2 and 5 continue to advance after that age There is a concentration of the curves at 16 years around $37\frac{1}{2}$ per cent

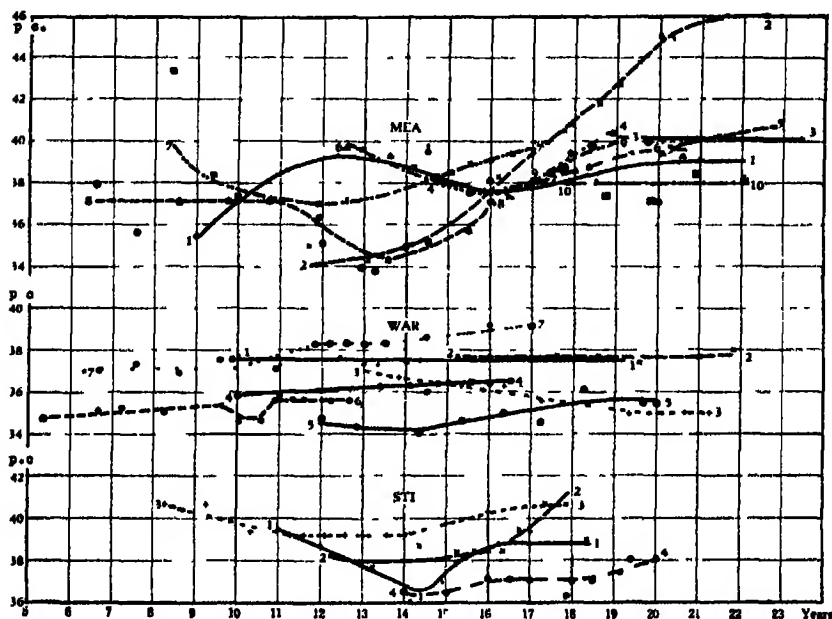


FIG 139 Individual curves of change with age of percentage ratio of Lower Jaw Height to Morphologic Face Height for three fraternities Mea 1, G M m, 2, W M m, 3, L M m, 4, M a M f, 5, M i M f, 6, N M f, 7, R M f, 8, B M f, 9, E M f, 10, H M m War 1, Fk W m, 2, B W f, 3, Fd W m, 4, M W f, 5, W W m, 6, G W m, 7, S W m Sti 1, E S f, 2, L S m, 3, M S f, 4, C S m Three sets of ordinates

In the War fraternity the proportions of the facial segments change slowly or not at all There is little concentration, the mean position at 16 years of the proportional chin height is 36.4 per cent, over 1 point lower than in the Mea. fraternity.

In the Sti. fraternity the curves belong to the concave-above type, with the minimum at or about the fifteenth year. At 16 years the mean position of the curves is 38.6 per cent, which is 2 points higher than in the War. fraternity.

Twins and Special Cases.—In Fig 140 are given the relative lower jaw curves of the Gar, Kyr., Ols., and Sha. twins. The curves of the Gar twins have a similar shape but do not everywhere coincide. The curves of the Kyr. twins are very like in position

at $12\frac{1}{2}$ and 17 years but depart from each other by some 4 points at the 15th year. It happens that just at this time No. 2, who was at other times 5 to 20 mm. the taller, fell 5 mm. behind his twin in stature, probably due to some physical disturbance. The

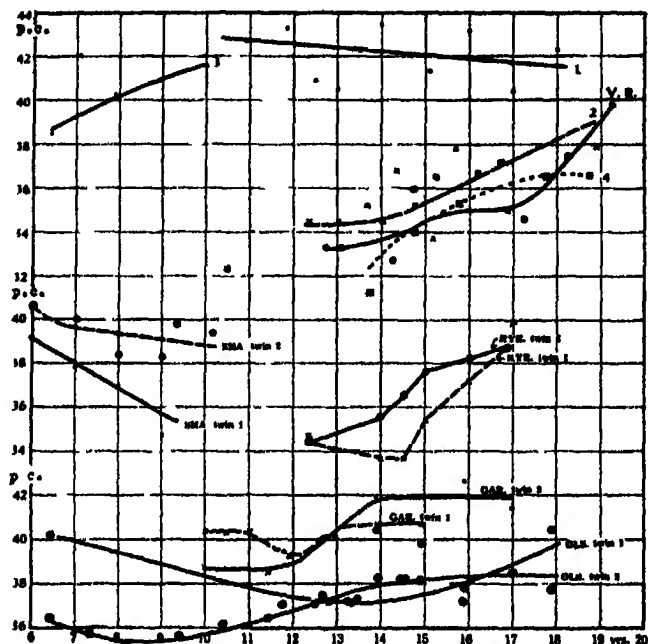


FIG. 140 Individual curves of change with age of percentage ratio of Lower Jaw Height to Morphologic Face Height for twins and special cases. 1, L S f, achondroplastic; 2, A W f, ateliotic, 3, A A f, cretin, 4, C B f, cretin. V R m, microcephalic. Twins all monozygotic and male, except Gar. Three sets of ordinates.

growth of the upper part of his face slowed up, as the observations show, and the jaw proportion became higher than in his twin who showed the pre-adolescent slowing up of the jaw proportion. The age-change curves of the Ol's twins are mostly close together after $12\frac{1}{2}$ years when closely comparable series of measurements were possible. The Sha. twins are on the down slope at 6 to 9 years, but 2 to 3 points apart.

In the upper part of the chart are given the age-changes in lower jaw proportion for a cretin (No. 3), whose ratio runs exceptionally high, and of another cretin (No. 4) whose ratio is low. The achondroplastic dwarf, No. 1, has a high ratio; ateliotic, No. 2, a medium one. The microcephalic, V.R., has a wave like series of ratios that gives a steep upward slope.

Summary.—Among proportional changes in height of facial segments, the greatest occur before birth as lower jaw height develops at expense of nasal height. After birth the nasal height increases faster than chin height for a few months, while the upper lip height remains at about 20 percent from 3 years to maturity. The proportional lower jaw height is less in girls than boys while the proportional nasal height is very slightly greater. On account of their large nose proportional chin height is especially small in Idiots. On account of the large nose of the Jews their jaw height is proportionately small. In various individuals the type of advance in proportional lower jaw height is varied. In 3 families considered the individuals of each show a specific type of change of facial proportions, with some dispersion at or around late adolescence. In monozygotic twins the age-changes of facial proportion are quite similar. The microcephalic has high nose and short chin. Viewed in more detail the vertical facial proportions are seen to change somewhat irregularly in infancy and childhood, depending largely on tooth development and eruption, as Hellman has pointed out. The changes in proportion of facial height are shown in clearer and greater detail by considering those of the chin by itself.

4. Physiognomic Face Height in Relation to Bizygomatic Width

This is the distance from the hair line to gnathion (measured by sliding calipers) divided by maximum width between the zygomatic arches (measured by compass calipers). The ratio varies with the general form (breadth vs narrowness) of the face. It is customary in anthropometry to compute ratios so that they will be less than 100. But in this case, so that comparison may be made between this ratio and that of morphologic face height to bizygomatic width, both ratios are computed in corresponding fashion. However, in defence to custom, in the general figure the reciprocal of the ratio is also given (in Fig. 141) above the other.

Sexual—Figure 141 shows the age change curve of this percentage ratio from mid-gestation to maturity. Taking height in relation to width, *B*, the face is seen to be becoming relatively lower and wider during 2 or 3 months preceding birth. Indeed, at birth the mean percentage ratio has fallen to 95. Immediately after birth the height of the face increases greatly and rapidly during the first year. Then, in boys, it seems to flatten out.

This is probably, as discussed under physiognomic face height (page 127), because of the action of gravity on the skull during the period of "first steps" During the third year the ratio advances

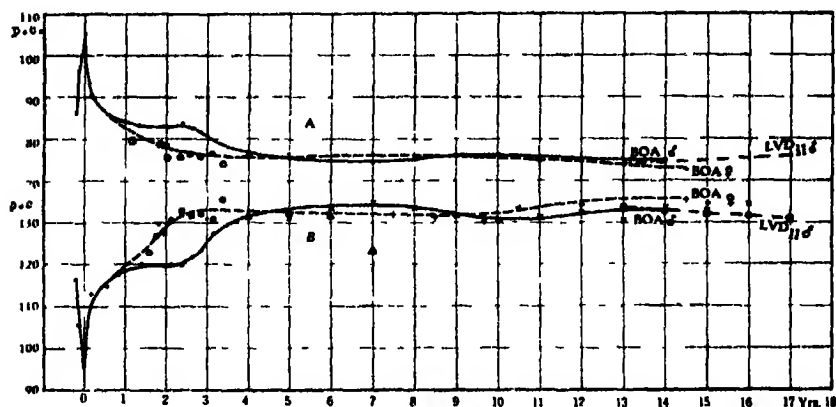


FIG 141 Mass curves of change with age of percentage ratio of Physiognomic Face Height to Bizygomatic Width (B) and reciprocal (A) Standard series, also LVDII from 13 years Both sexes 2 sets of ordinates

rapidly again in boys and becomes nearly stabilized at about 5 years At 15 years it reaches 132.5, or its reciprocal 76 Hrdlička ('25, p. 203) gets an index of 74 for "Old Americans" This index has not been often computed.

In girls the age changes during the first 3 post-partum years are quite different from those in boys This is associated with the sexual differences in physiognomic face height (page 128 and Fig. 103), and with the remarkable differences that we shall have to discuss later in the morphologic face height/bizygomatic width ratio Girls do not show the period from 1 to 2½ years of slight change that boys do Is there a sex difference in the movement of the trichion? After 5 years the course of change in girls is like that in boys. The age curve of girls lies a point or so below that of boys from 5 to 10 years It thereafter changes its relation to the age curve of boys in the BOA series, but not in the LVDII series.

Social—The growth curve of the mongoloid dwarf group is above the others from 11 years onward This group has the smallest bizygomatic width and a high, because sparsely haired, forehead The I group lies a little below standard because the zygoma is closer to the (large) standard than the physiognomic face height is in this series.

Racial.—Of the 3 groups into which our children fell, the Nordic (U S) series gives a curve that is concave above from 10 to 15 years, whereas the other 2 curves are convex above. The Negroes have the highest ratio from 10 years onward because of the high face and despite a bizygomatic width that slightly exceeds that of the Nordics. The curve of the Mediterraneans lies low because of their low face

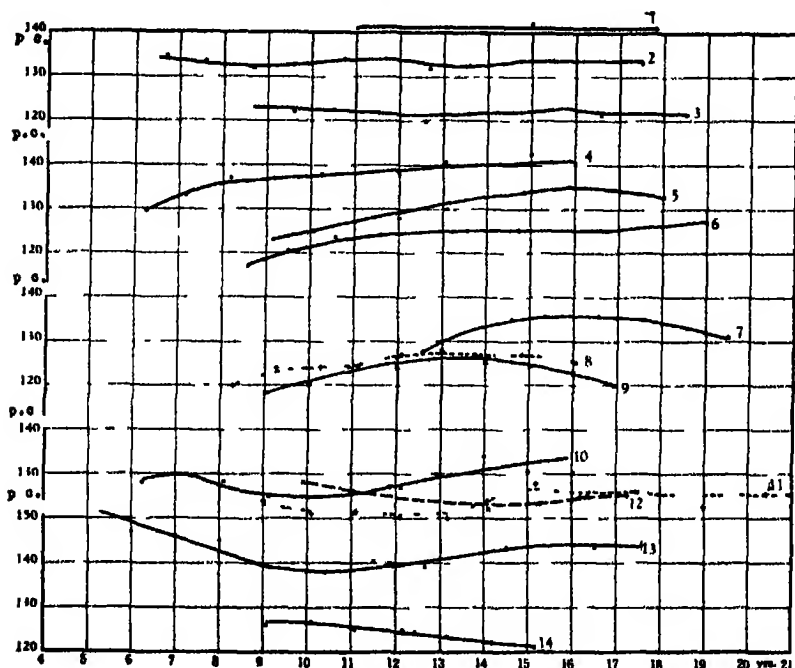


FIG 142 Individual curves of change with age of percentage ratio of Physiognomic Face Height to Bizygomatic Width for 14 boys of LVD₁₁ series 1, RH No 80, colored, 2, SB No 55, 3, JC No 10, 4, FC No. 58, 5, TV No 65, 6, FW No 24, 7, JC No 49, 8, IS No 93, 9, GP No 5, 10, VE No 59, 11, CD No 30, 12, MG No 95, 13, CH No 83, 14, CH No 26 colored Five sets of ordinates

Individual.—Figure 142 shows 5 types of age change curves, using different sets of ordinates. Above are 3 curves that show little change in value from juvenility to late puberty. One is of a colored boy with a ratio of over 140 per cent. Next below are 3 curves that have prevalingly ascending slope. Still below are 3 curves that are concave below. Further below are 4 curves that are concave above and finally, at the bottom, one of a colored boy that descends regularly from 9 to 15 years. These types result, of course, from differences in proportional growth of the

two dimensions involved Further analysis must await further study of the individuals involved.

Familial—Figure 143 gives the age change curves for 3 fraternities. The Sti fraternity are all of the descending slope

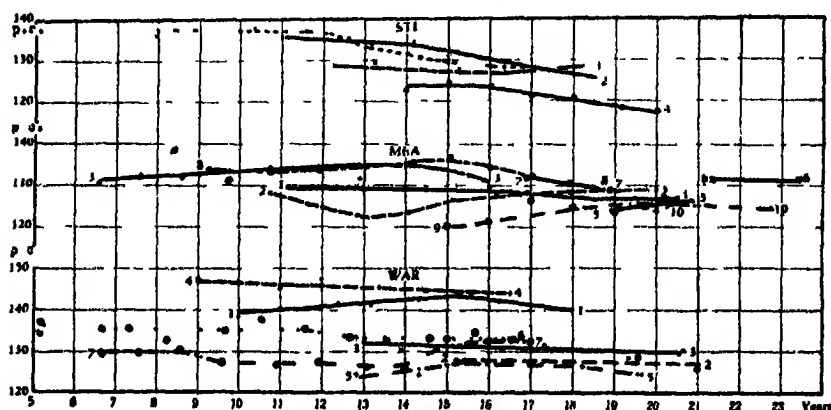


FIG 143 Individual curves of change with age of percentage ratio of Physiognomic Face Height to Bizygomatic Width for three fraternities Sti 1, LS m, 2, ES f, 3, MS f, 4, CS m Mea 1, GM m, 2, WM m, 3, BM f, 4, EM f (one point), 5, HM m, 6, LM m, 7, Ma M f, 8, RM f, 9, NM f, 10, Mi M f War 1, Fk W m, 2, BW f, 3, Fd W m, 4, MW f, 5, WW m; 6, GW m, 7, SW m Three sets of ordinates

type and have a mean position at 16 years of 127 per cent. The Mea fraternity shows various types The mean position at 16 years is 128.6 The War. fraternity falls into two groups of prevalingly descending curves. Their mean value at 16 years is 134.5 Thus in different fraternities the ratios have a different mean value and sometimes characteristic slopes

Twins and Special Cases—Figure 144 gives the age change curves of physiognomic face height/bizygomatic width indices The Gar twins have parallel curves, concave above and about

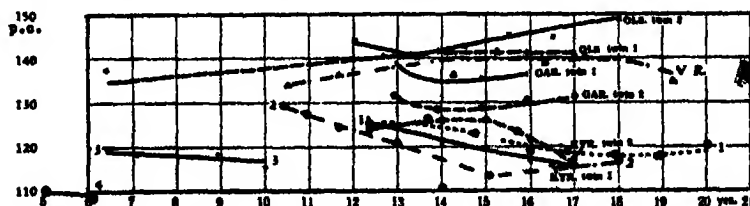


FIG 144 Individual curves of change with age of percentage ratio of Physiognomic Face Height to Bizygomatic Width for twins and special cases 1, A W f, ateliotic; 2, LS f, achondroplastic; 3, A A f, cretin, 4, H L m, cretin V R. m, microcephalic All twins monozygotic and male except Gar

5 points apart. The Kyr. twins have curves that are 6 points apart in the middle and converge at the ends, perhaps due to the above mentioned disturbance in twin 2's growth. The curves of the Ols. twins lie close together and both ascending.

Two cretins in the lower left hand corner have low ratios, and so have the No. 2 and No. 1 dwarfs. The microcephalic, V R., has a rather high ratio and his curve is concave below.

Summary.—The curve of change with age of the percentage ratio of physiognomic face height to bizygomatic width is one that changes slope rapidly during infancy and childhood and more slowly in later years. During the second and third years the sexes develop somewhat differently but their curves are close together after 5 years. The mean social and racial differences depend on the differences of the two dimensions involved in the ratio. The age-change curves fall into different types, level, ascending, descending, concave above and concave below, and these types often have a familial (hereditary) significance. Also, families have differing mean values, at a given age, for the individual curve. Twins show the same type of curves except for disturbing episodes in development. Dwarfs tend to have ratios that approach 100, or the round-faced condition.

5. Morphologic Face Height in Relation to Bizygomatic Face Width

General.—This index is the Morphologischer Gesichtsindex of Martin ('28, p. 260), also Facial Index of Garson and Read ('99).

The dimensions concerned have been sufficiently defined elsewhere in this paper. This index is more precise than that involving physiognomic face height.

Comparative.—This index has been frequently computed for groups of adults of different races, but there are not very many growth series. Cf. Gray and Ayres, Pfitzner, Niggi-Hurlimann, Hoesch-Ernst, Boas, Saller and Bosshart.

Sexual.—Figure 145 shows the age changes in the percentage ratio from mid-gestation to maturity. The index rises slowly from 60 at the 4th fetal month to 64 at birth. This birth ratio agrees with that obtained by Kugler. Increase is more rapid during the first post-natal month. During the second and third year irregularities arise, in all probability associated with the changes of the jaws during tooth development and eruption and with the increase of bizygomatic width resulting from chewing.

and the accompanying pull of the masseter muscles upon the zygoma. The rise of the curve for both sexes at 18 to 22 months is probably causally related to the eruption of the milk canines

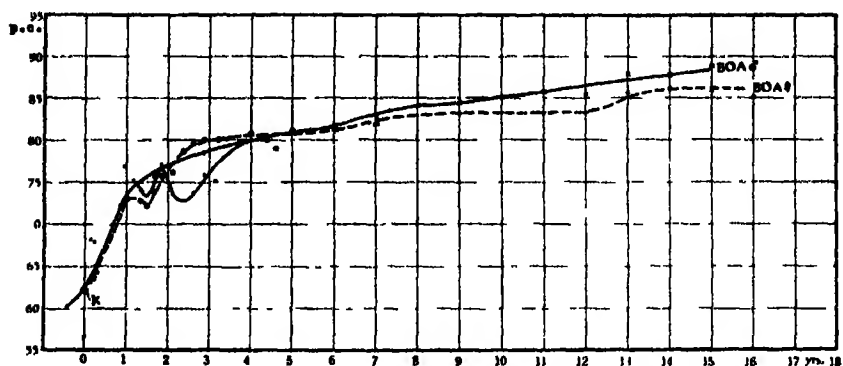


FIG 145 Mass curves of change with age of percentage ratio of Morphologic Face Height to Bizygomatic Width Standard series K, Kugler's birth determination Between ages 1 and 4 years an alternative curve is drawn

and premolars which occur at about that time. The depression of the curve of the male in the third year is of the same type as shown in Fig 141, but its interpretation is uncertain. Possibly the development of the maxillary sinus is involved. After 5 years this index increases slowly to at least 15 years, when it reaches 88. Hrdhčka ('25) finds an index of 86 for adult U S males. Keiter ('33, p. 348) finds an index of from 82 to 91 in various European groups at 15 years. The curve for females lies 1 to 6 points below that of the males. The curves seem to approach temporarily at the 13th year. These results are fairly in accord with those shown in Saller's ('30) Fig 27.

Social.—The mongoloid dwarfs occupy the lowest position while in the physiognomic ratio they occupy the highest. This is because of the high (because relatively hairless) forehead of these dwarfs. With the forehead eliminated the face of the mongoloid dwarf is relatively low. The Idiots, on the other hand, with their high noses have a morphological face that is nearly as high as wide.

Racial.—The 3 racial groups that we are considering are practically indistinguishable in respect to this ratio at all ages.

Individual.—Figure 146 shows the age-change curves of 5 boys. In all cases these curves slope upward, sometimes with a marked increase of slope at 14 or 15 years.

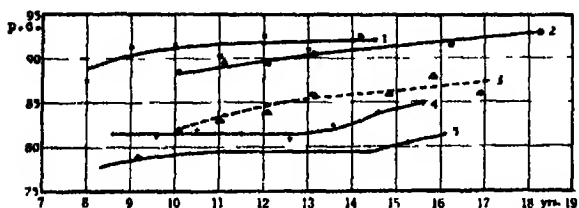


FIG 146 Individual curves of change with age of percentage ratio of Morphologic Face Height to Bizygomatic Width for five boys of LVD series 1, W M No 8 (U S), 2, H M No 43 (U S), 3, T V No 65 (Italian), 4, J C No 10 (U S), 5, M H No 2 (U S)

Familial—Figure 147 shows, for 3 fraternities, the age-change of morphologic face height in relation to bizygomatic width. In the Mea. fraternity the curves are mostly of a wavy type. At age 17 the mean value of the percentage ratio is 84.9.

In the Sti. fraternity also the curves are wavy and somewhat scattered without segregation by sex. The mean value of the percentage ratio at age 17 is 84.6.

In the War. fraternity there seems to be a segregation into the round faced and the low faced, separated by about 10 points,

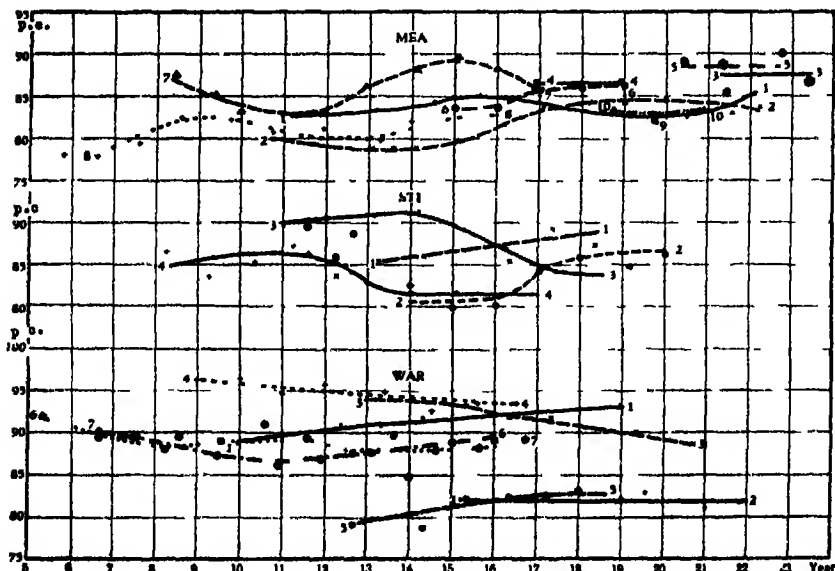


FIG 147 Individual curves of change with age of percentage ratio of Morphologic Face Height to Bizygomatic Width for three fraternities Mea 1, G M m, 2, W M m; 3, L M m, 4, M a M f; 5, M i M f, 6, N M f, 7, R M f, 8, B M f, 9, E M f (one point), 10 H M m. Sti 1, L S m, 2, C S m, 3, E S f; 4, M S f. War 1, F k W m, 2, B W f, 3, F d W m, 4, M W f, 5, W W m, 6, G W m; 7, S W m. Three sets of ordinates

having means at 92.6 and 82.0 respectively. Two curves show increasing slope, two, decreasing slope and one is level.

Twins and Special Cases—Figure 148 gives the curves of the Dot. (dizygotic, colored) twins and the Gar, Kyr., Ols., and

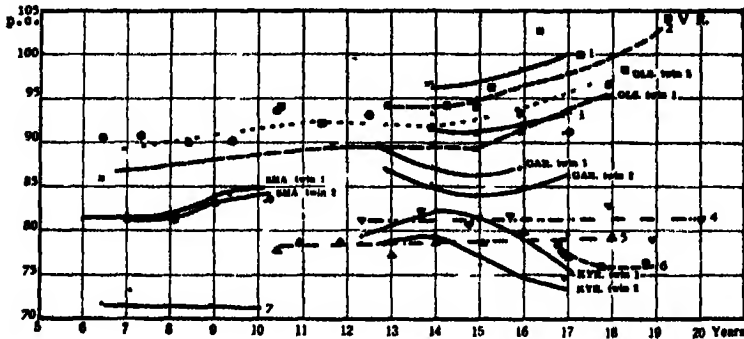


FIG 148 Individual curves of change with age of percentage ratio of Morphologic Face Height to Bizygomatic Width for twins and special cases 1, E D f, fraternal twin to 3, 2, V R m, microcephalic, 3, S D f, fraternal twin to 1, 4, A W f, ateliotic dwarf; 5, L S f, achondroplastic, 6, C B f, cretin, 7, A A f, cretin Monozygotic twins all male except Gar

Sha twins. Of the Dot twins (Curves 1 and 3) the curves are 10 points apart, but run parallel. Of the Gar twins the curves are 2 points apart and parallel. Of the Kyr. twins the curves are 1 to 4 points apart and nearly parallel. Of the Ols. twins the curves are about 3 points apart and roughly parallel. In the Sha twins the curves nearly coincide.

Of the cretins, one, No 7, has a low face at 71.5; and one, No 6, at 78–76. The dwarfs, No. 4 and No. 5, show horizontal curves at nearly 80 per cent.

Summary.—The age-changes in morphologic face height/bizygomatic width somewhat resemble those of physiognomic face height/bizygomatic width. There is the same peculiar difference between the sexes at the third year. The mongoloid dwarfs, however, reverse their relative standing in that the physiognomic face height is abnormally high, owing to sparse hair on the front of scalp. Racial differences are slight. Individual curves may slope upward, downward, or show a wavy type. Particular types are commonly found in particular families. Twins show parallel curves. Cretins and other dwarfs have low faces.

VII. AGE CHANGES IN DEVELOPMENT OF EYE REGION

1. *The Interpupillary Distance*

General —The distance between the pupils on the human face has considerable phylogenetic as well as general developmental interest. As is well known the eyes in fishes, amphibia, reptiles and birds are located on the sides of the head. In mammals, while the eyes are still laterally placed in the lower forms, as in the ungulates, they can generally look forward as well as laterally, as one sees in the dog. In ascending the primate series one notes a clear migration of the eyes to a forward-looking position. This is already marked in the lemurs; and in *Stenops* and *Tarsius* the huge eyes are better fitted for forward than for lateral vision. Finally, in the higher Primates the orbit becomes entirely enclosed and separated from the temporal fossa by the alisphenoid or great wing of the sphenoid as well as by the orbital surface of the zygomatic bone.

Our problem is Does the development of the distance between the eyes in man show any traces of this roughly traced phylogenetic history?

The observations were made for the most part on children between the ages of 6 and 16 years, a few younger ones and a few fetuses.

The technique employed consisted of measuring the distance between the outer angles of the eyes, also the distance between inner angles, and taking half the sum of these two measurements for pupillary distance. It seemed to be impracticable to measure the distance between the center of the pupils directly, especially in babies. Instruments for use with adults have been invented by ophthalmologists (Howe, '07, p. 216).

Two real difficulties arise in making such measurements. First, where is the outer angle? It may be defined with precision as the most lateral point of the line between conjunctiva and the mucosa of the eye lid. The inner angle is harder to define. It might be taken as the medial bounding line of the lacrimal caruncle; and it was so taken by me. Others have apparently used the lateral line of the caruncle.

The second difficulty arises from the inability of the observer to induce the subject to open the lids to a standard degree—say to expose $\frac{3}{4}$ ths of the vertical diameter of the pupil.

Sexual.—Figure 149 represents the mass curve of change from mid-gestation to maturity. Beginning at the 4th month with a mean distance of 23 mm the distance at birth is 46 mm. In the

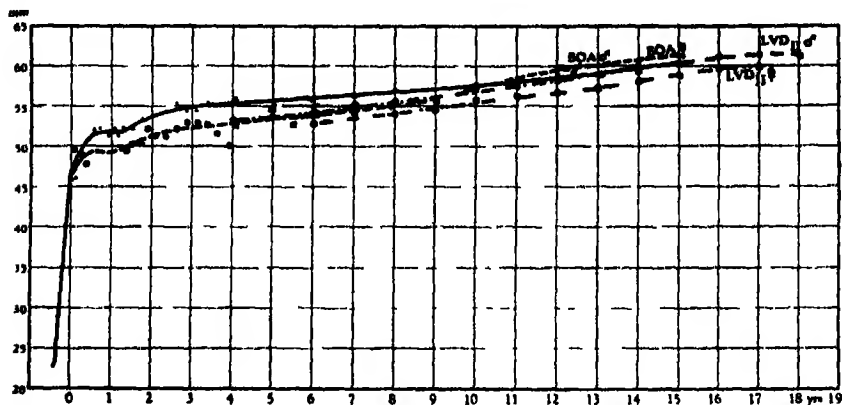


FIG 149 Mass curves of growth of Interpupillary Distance in Standard series, also $LVD_{II} m$ and f . Symbols as in Fig 1

male this mean distance rises in 3 post-partum years to 55 mm, or 3 mm p a. Thereafter, the increase is relatively slow, the dimension reaching 60 mm at 15 years or an increase of less than 0.4 mm p. a. After 15 years the mean increase of this dimension continues for about 2 years before slowing down toward zero at or about 61 mm¹². This last conclusion is based on the mean growth curve of the males of the LVD_{II} series.

In the standard female series the mean interpupillary distance for the first 2 years lies about 3 mm below that of the male. In this BOA series the curve of mean distance decussates with that of the male at 10½ years and lies about 1 mm. above the mean male curve for 4 or more years. I am in some doubt about the validity of this portion of the female curve since the older BOA girls were measured by my assistant. In the case of the LVD_{II} girls, measured by myself, the mean interpupillary distance is about 2 mm less in the female than the male at all ages from 6 to 17 years.

Of special interest is the irregularity in the mean curve of infants of both sexes around 12 months post-partum. This is not due entirely to sampling, for in individuals measured over this period there is in several instances a decrease or cessation of increase of the dimension at or around this period. This may be

¹² Cf the findings of Seggel, 1902

associated with change in form of the cranium on assuming the erect position

Social—Of the 4 groups varying in mean intelligence, the BOA group is uppermost, corresponding with their great frontal width. The mongoloid dwarfs have the lowest position, about 4 mm., on the average, less than the BOA series, corresponding to their small bizygomatic width. The LVD_i and I series occupy an intermediate position

Racial—Of the 3 groups, Negro, Italian and Nordic, here considered, the Negro has the greatest interpupillary distance, the Mediterranean least. This is the order of mean bizygomatic width and of the mean minimum frontal width in the LVD_i series. The great distance between the pupils is, of course, a striking trait of Negroes. Of adult Nordic peoples the Leipzigers have an average interpupillary distance of 65.8 mm (Gunther, '33, p. 262)¹³

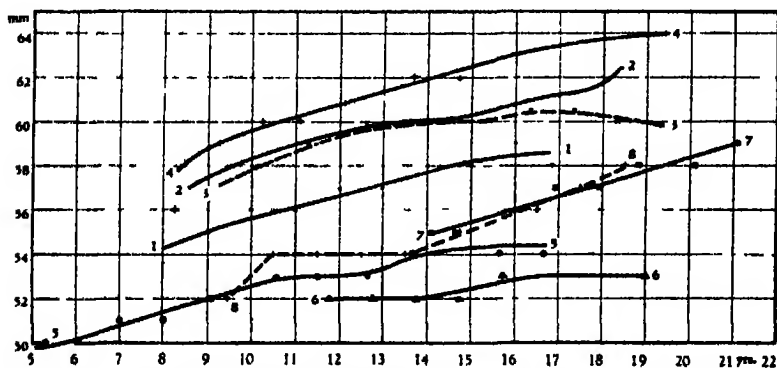


FIG 150 Individual curves of growth of Interpupillary Distance for eight boys of I.V.D. series 1, G.P. No 5, 2, J.C. No 10, 3, F.C. No 28, 4, I.S. No 93, 5, C.H. No 83, 6, P.M. No 10, 7, W.W. No 13, 8, R.H. No 29

Individual—Figure 150 shows 8 individual growth curves of interpupillary distance. All ascend between 5 and 17 years—No. 7 in straight line fashion and Nos 1 and 4 approximately so. Nos 2, 5 and 6 show an adolescent spurt of growth. No 3 alone shows a descending slope after 17 years, and this is apparently due to a reduction in the distance between inner eye angles with widening of palpebral slit. No. 4, who has the largest interocular distance, has also a strikingly wide head, while No 8, near the bottom, has a narrow head (Fig. 10). This illustrates the rule,

¹³ Günther acknowledges that this distance is about 3 mm greater than other authors find. Perhaps he had an excess of broad heads in his population.

cited by Martin ('28, p. 711), that in brachycephalics the interpupillary distance is absolutely greater than in dolichocephalics

Familial—Figure 151 gives the curves of interpupillary distance for the Mea and the Sti families. In the former, there is

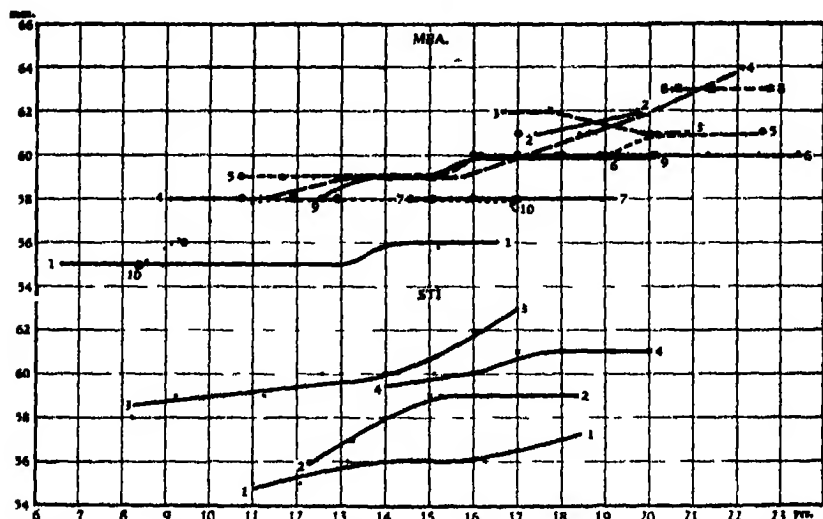


FIG 151 Individual curves of growth of Interpupillary Distance for two fraternities Mea 1, BM f, 2, EM f, 3, HM m, 4, GM m, 5, WM m, 6, LM m, 7, Ma M f, 8, Mi M f, 9, NM f, 10, RM f Sti 1, ES f, 2, LS m, 3, MS f, 4, CS m Two sets of ordinates

a considerable range of distance at 16 years from No 1, female, at 56 mm. to No 3, male, at 62 mm. But some of the curves (Nos 6 and 9, 7 and 10) coincide for some ages. Nos. 1, 6 and 9 show the adolescent spurt of growth at 14–16 years. The mean interpupillary distance in the Mea. family, at 16 years, is 58.5 mm.

The Sti. family shows more dispersion than the Mea family. A change of slope at or about the time of the adolescent spurt appears in 3 of the curves of this family. The mean distance between pupils at 16 years is 59.2 mm.

In Fig 152 are shown the curves of the 7 members of the War family. Aside from No. 2 at 62 mm the curves are rather close together and 4 of them show striking changes of slope at adolescence. The mean distance at 16 years is 57.6, or, omitting No. 2, it is 56.9. This is a striking contrast to the other two families.

Twins and Special Cases.—Figure 153 gives curves of interpupillary distance for the Gar., Kyr., Ols., and Sha. twins. The

pairs of curves of the Gar and Kyr. twins, respectively, coincide, those of the Ols twins intertwine, those of the Sha twins are based on few points, but probably run parallel about 2 or 3 mm apart. No. 1 is from a brother of the Gar twins. His curve is strikingly divergent from theirs before 12 years. No. 3 is the curve of an achondroplastic dwarf and No. 4 of an ateliotic. The

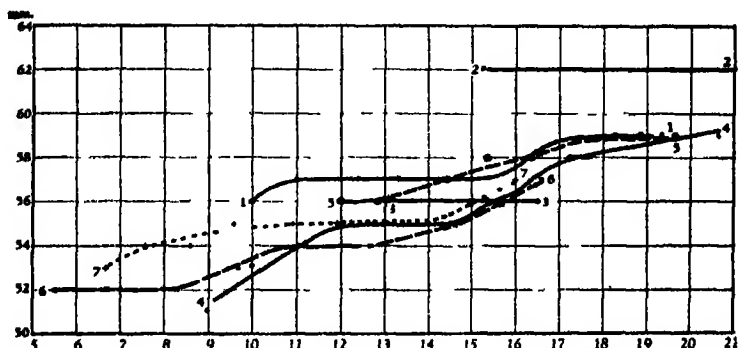


FIG 152 Individual curves of growth of Interpupillary Distance for War fraternity 1, Fk W m, 2, B W f, 3, Fd W m, 4, M W f, 5, W W m, 6, G W m, 7, S W m

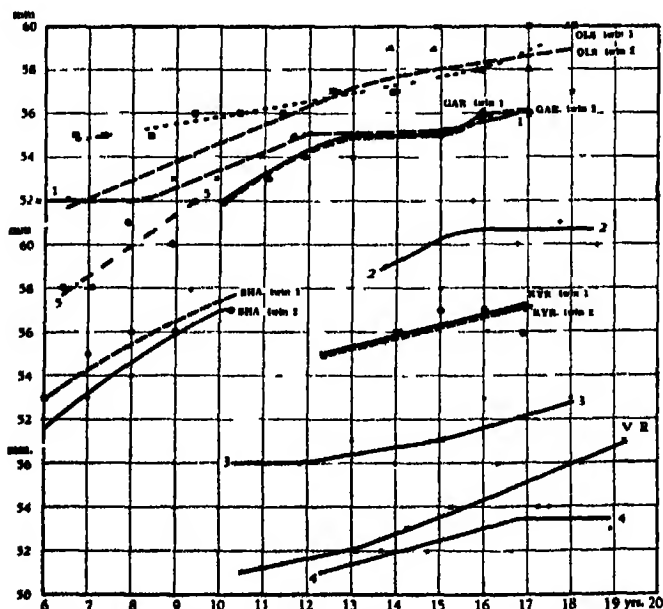


FIG 153 Individual curves of growth of Interpupillary Distance for twins and special cases 1, J G bro of Gar twin sisters, 2, C B f, cretin, 3, L S f, achondroplastic, 4, A W f, ateliotic, 5, A A f, cretin V R m, microcephalic All twins monozygotic Three sets of ordinates

curves of two cretins appear in Nos. 2 and 5, these are very far apart. The curve is shown of the microcephalic V R. He is not a dwarf, but his eyes are almost as close together as in the dwarf, No. 4.

Summary—The migration of the eyes so that they look forward instead of laterally as in the embryos is not completed by birth, but on account of growth of the head as a whole the distance between the pupils is constantly increasing. The increase is rapid to 3 years post-partum and thereafter it is slow, practically ceasing at 17 years. The distance is less in girls than boys. Both sexes show an irregularity of the growth curve during the first year of infancy. The distance among whites is greatest in the standard boys and least in mongoloid dwarfs, but both are exceeded in Negroes. The families show segregation and a mean differentiation. In twins the curves practically coincide. The interpupillary distance is great in cretins, small in microcephalics.

2 The Distance Between Inner Eye Angles

General.—The distance between the inner eye angles (endokanthia) was measured with the sliding calipers (using the pointed ends of the arms) to points described above. The importance of this dimension is in part its correlation with the up-growth of the nasal root.

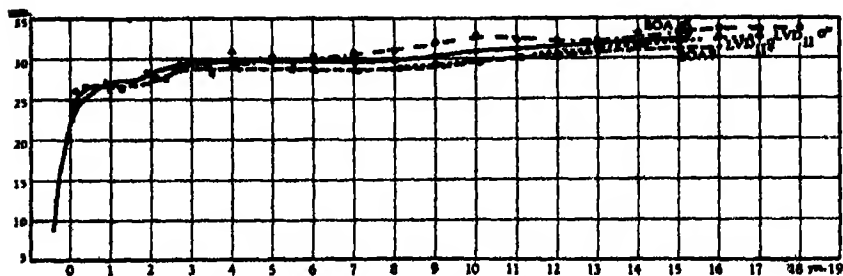


FIG 154 Mass curves of growth of Distance between Inner Eye Angles. Standard series, also LVD₁₁ m and f. Symbols as in Fig 1

Sexual—Figure 154 gives the curve of increase of the distance between inner eye angles from mid-gestation to maturity. From a distance of 9 mm. at the fourth month, the dimension increases rapidly to 22 mm. at birth. It rises to 27 at the end of the first year and thereafter more gradually up to 34 mm. at 17 years.

From 3 to 17 years the increase is about 4 mm, or about 0.3 mm. p a

Certain irregularities, like those of Fig. 149, appear in the course of the curve. First, there is a cessation of growth at between 12 and 18 months (see also '39, p 282). This is the period when several of the growth curves of the head are slowed down—a result that I have regarded as due to the effect of gravity on the shape of the head. In the standard (mean) series growth of this dimension between 3 and 8 years is practically zero, although the interpupillary distance is increasing slowly (Fig 149). The depth of nasal root is increasing slowly, so apparently the cessation of increase of distance between inner angles is due to the growth of the sides of the nose between the inner eye angles. Certain endokanthial tissue goes to build up the depth of the nasal root. A second slump in the growth of the distance between inner eye angles is at 10–13 years, at a time when the nasal root is again growing rapidly¹⁴ ('39, Fig 28).

The curve of growth in the female rises slightly over that of the male in the first 6 post-natal months, then falls to about 1 mm. below the male curve, and persists at this level to maturity. This is undoubtedly a significant result, for also in the LVD_{II} curves of means of children (all measured by myself) the inner distance is the smaller in girls—as much as 3 mm, or 10 per cent smaller than in boys. This is to be expected, since the girls have the narrower heads (cf. also Martin, '28, p 535).

Social.—Of the 4 groups differing in mean intelligence, the standard (BOA) curve lies above the others, corresponding to the great minimum frontal width of this group. The curve of the mongoloids lies near the lowest level. The Idiot and LVD_I groups are intermediate, at least after 13 years of age. That the distance between the inner angles of the eye of the mongoloids, whose facial width is intermediate, is not less is partly associated with the fact that the nasal root is exceedingly low ('39, Fig 29) and its development has, accordingly, not drawn the eyes together.

Racial.—Of the 3 racial groups the distance between inner eye angles is greatest in the U S and Nordic groups, least in the Mediterranean group (narrow head), and intermediate in the Negro group despite the fact that the interpupillary distance in

¹⁴ Saller ('31, p 246) shows similar slow growth at 6–8 and 12 years in North Germans.

the Negro group is greatest of all This is because of the "large eyes" of the Negro.

This dimension has been repeatedly measured and means for adults of different races are tabulated by Martin ('28, p 535). These means are low for Parisians, Basques, Greeks and Badenens (31 to 32.3 mm) and high for Belgians, Eskimoes, Siberians, Bushmen and various Negro tribes (35 to 37 mm.). For 106 Leipzigers Gunther ('33, p 260) found a mean value of 33.02

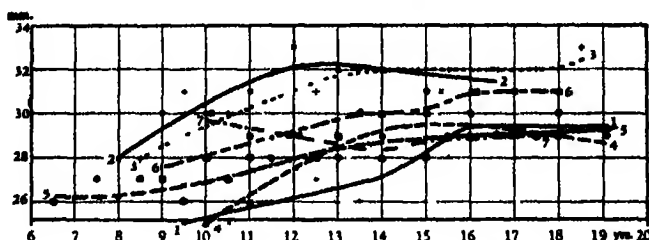


FIG 155 Individual curves of growth of Distance between Inner Eye Angles for seven boys of LVD series 1, M H No 2, 2, W M No 8, 3, J C No 10, 4, H M No 43, 5, J B No 54, 6, T V No 65, 7, M G No 95

Individual—Figure 155 gives growth curves of the distance between inner eye angles for 7 of the individuals pictured on Plate VII of my 1939 publication Though the total range of the dimension in the entire figure is only 7 mm, and, due to facial mobility, a probable error of not far from 1 mm is associated with each reading, still from trends of the readings in successive years one may draw certain generalizations

1 From 7 or 8 to 12 or 13 years, with one exception, the curves trend upward—the face in the region of the nasal bone and frontal processes of the maxillæ is growing broader. The exception is No 7 (LVD, No. 95) in whom, during this period, there was certainly no increase in this diameter ¹⁵

¹⁵ No 95 is figured in lateral view in '39, Pl VII, 10 and Pl VIII, 10 During the 10 years that he has been under observation he alone, of the 9 boys whose facial outlines are shown in Pl VIII, has shown no change either in nasal height or nasal root depth with reference to the subnasal point Moreover he has many traits of the acromegalic, as the following dimensions at 16½ years indicate

Stature	182.1 cm
R hand length	21.5
R hand width	9.4
R foot length	28.5
R foot width	10.8

These are among the largest findings for these diameters obtained in the LVD series No 95's chin also underwent extensive enlargement during the 10 years

2. Beginning usually at about 13 or 14 years the slope of the curve diminishes greatly, approaching zero, except for spurts of growth as in Nos 1 and 6

3 In some cases (Nos 2 and 4) there is a downward slope after 16 years, indicating that the inner angles of the eye lids are approaching each other slightly As pointed out earlier this approach is probably associated with an increase in nasal root depth.

Familial—Figure 156, above, shows the change in distance between the inner angles of the eyes in the Mea family Nos 1

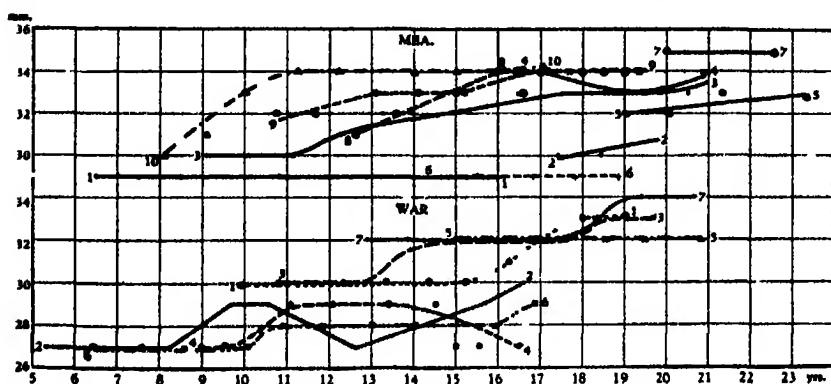


FIG 156 Individual curves of growth of Distance between Inner Eye Angles for two fraternities Mea 1, B M f, 2, E M f, 3, G M m, 4, H M m, 5, L M m, 6, M₂ M f, 7, M₁ M f, 8, N M f, 9, W M m, 10, R M f War 1, Fk W m, 2, G W m, 3, W W m, 4, M W f, 5, B W f, 6, S W m, 7, Fd W m Two sets of ordinates

and 6 show no change in this dimension between the ages measured, also No. 10 from 11 to 17 years was constant Otherwise, there is a more or less regular slope upward to 17 years (Curve No 4, through 5 post-adolescent years, is concave above At 16 years the mean position of the curves is 32 mm.

In the lower part of the figure are given the curves of the distance between inner eye angles for the War family. These curves do not show the nearly uniform upward slope which was found in those of the Mea family. No 5 is level from 15 years onward, No 6 from 11 to 16 years Spurts of increase are found at 10, 13, 16 and 18 years, in different curves, and in No. 2 is found a slope downward from 10½ to 12½ years Some of these smaller changes of slope may be merely postural. At 16 years the mean position of the curve is 29.9 mm.

Figure 157 gives the curves of change in distance between inner eye angles for the 4 members of the Stt family. Three of the four show a small increase with age, in two boys an adolescent spurt in the 16th year.

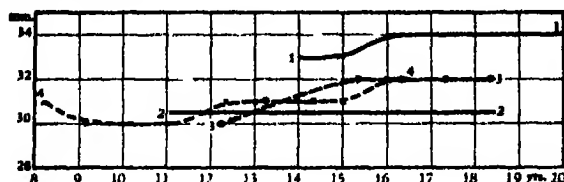


FIG 157 Individual curves of growth of Distance between Inner Eye Angles for members of the Stt fraternity CS m, 2, ES f, 3, LS m, 4, MS f

Twins and Special Cases—Figure 158 gives age-changes of distance between inner eye angles for the Gar., Kyr., Ols., and Sha twins. The curves of the Gar. twins are practically coincident, those of the Kyr. and Sha twins lie about $\frac{1}{2}$ mm apart. The curves of the two dwarfs of different types are widely separated, that of the microcephalic occupies a rather low position. The curves of the two cretin girls (Nos 3 and 4) have rather high positions, i.e., the inner eye angles are far apart, since the nasal roots are shallow.

Summary.—The curve of age-change in the distance between inner eye angles is like that of interpupillary distance, except for

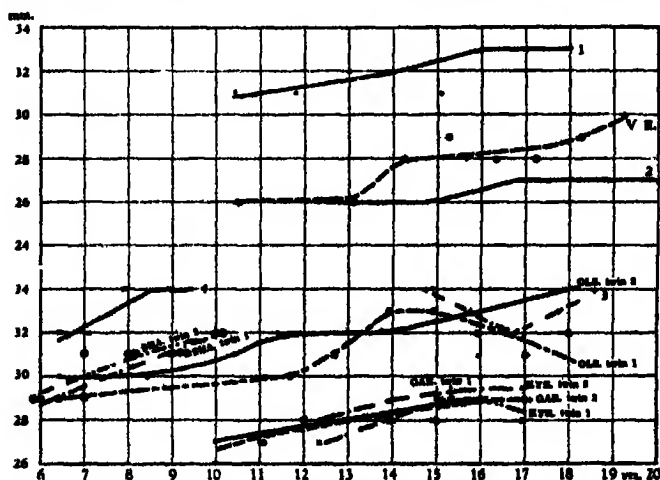


FIG 158 Individual curves of growth of Distance between Inner Eye Angles for twins and special cases 1, LS f, achondroplastic, 2, MW f, ateliotic; 3, CB f, cretin, 4, AA f, cretin. V R m, microcephalic. Twins all monozygotic and male except Gar. Two sets of ordinates

variation in size of palpebral slit. The distance tends to decrease as the nasal root deepens at puberty. In mongoloid dwarfs where the root remains shallow the distance is large. In an acromegalic there is practically no increase in this distance. The monozygotic twins have coincident growth curves.

3 The Width of the Palpebral Slit

General.—The width of the palpebral slit is obtained by taking half the difference between the distances betwixt the outer eye angles and the inner eye angles, as defined above. It has been measured in some adults of various races, but I do not know of studies of the changes of this dimension in children.



FIG 159 Mass curves of growth of Width of Palpebral Slits. Standard series, also LVD₁₁ m and f. Symbols as in Fig 1.

Sexual.—Figure 159 gives the curve of change with age of this dimension from mid-gestation to maturity. In the male the dimension increases from 7.5 mm. in the fetus at 4 months to 23 mm. at birth. The advance is thus very rapid—15.5 mm. in 6 months, or at the rate of 31 mm. p.a. Immediately after birth this rapid growth is slowed up. Palpebral width increases in 8 months post-partum by 3 mm. or at the rate of 4.0 mm. p.a. Then the mean dimension decreases about a millimeter for 8 months, apparently in association with the changes in head form that accompany standing. After 18 months post-partum increase of the dimension continues again very slowly, up to 15 years, at the rate of 0.3 mm. p.a. The advance is not quite uniform, there is perhaps a reduction in rate before and an increase after 11 years.

In girls the size of the palpebral slit is, during the first year post-partum, about 2 mm. less than in boys; it becomes equal before or during adolescence and thereafter is greater in girls than

boys, despite the constantly smaller head width of girls, as Keiter ('33, p 379) has stated. Thus, relatively to face width, the eye openings of girls in our series are strikingly larger than of boys. The mean diameter reached at 15 years is 29 mm. for boys and 30 mm for girls. This difference in size is found also in the LVD_{II} series, although the mean excess of diameter of the girls' eyes is only 0.5 mm.

From Martin's ('28, p. 527) table of width of eye slits it appears that adult Parisian women have, on the average, slits 2.5 mm. wider than men (i.e., male 27.5, female 30.0). Also in the gypsies the eyes of the women are said to be the larger. In 4 other races cited by Martin the female eye slit is, however, on the average, about 1 mm. the narrower.

Social —Of the 4 groups of white children of varying intellectual development, the uppermost curve, including eyes that are 29 mm wide on the average at 14 years, is that of the Idiot series, the lowermost that of the mongoloid series. The children of the standard (BOA) series, despite broad foreheads, have only a mediocre width of eye slit.

Racial —This dimension is largest in the Negroes and least in the Jews, after 13 years of age. The Italians occupy a mediocre position in respect to this dimension.

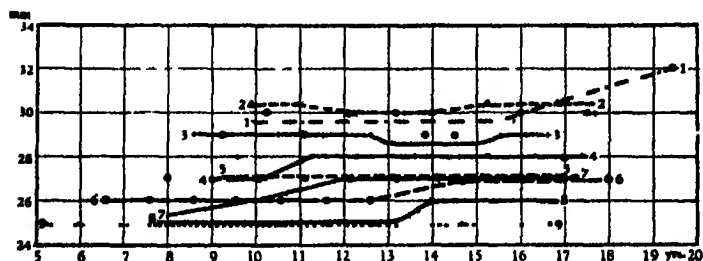


FIG 160 Individual curves of growth of Width of Palpebral Slits for nine boys of LVD_I series 1, IS No 93, 2, MG No 95, 3, JC No 10, 4, FC No 28, 5, CD No 30, 6, JB No 54, 7, GP No 5, 8, WM No 8, 9, CH No 80

Individual —Figure 160 gives the curves of growth of eye slit of 9 boys of the LVD_I series. Since the dimension is a small one these lines show no marked slopes. In some individuals, Nos. 5 and 9, the dimension has not increased 1 mm. in 8 or 10 years. In other cases there has been an increase of 2 mm. in 4 years. Two or three curves show an increase of 1 mm. or so at or about the 14th year. Two show a dip (or reduction of diameter)

at adolescence. In how far all of these deviations from the horizontal are significant is a little hard to say, certainly the dimension, in the mass, tends slowly to increase. Also, the increase takes place more rapidly in some individuals than others, just as the minimum-frontal width does (Figs 24 to 26).

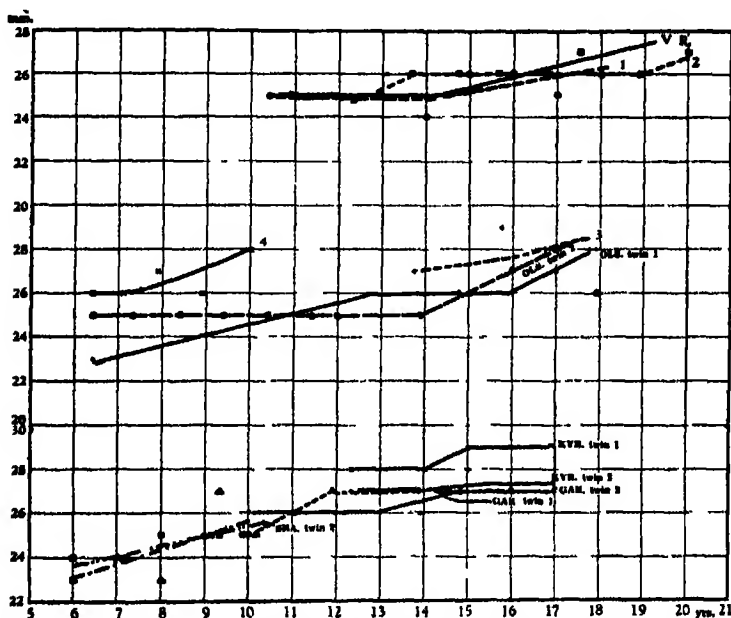


FIG 161 Individual curves of growth of Width of Palpebral Slits for twins and special cases 1, LS f, achondroplastic, 2, A W f, ateliotic, 3, C B f, cretin, 4, A A f, cretin V R m, microcephalic. Twins all monozygotic and male except Gar. Three sets of ordinates

Twins and Special Cases --In Fig 161 are shown curves of age changes of the eye slit in the cases of 4 pairs of twins. In the Gar twins the curves are nowhere more than 1 mm apart. In the Kyr twins they differ from 1 to 2 mm. The Ols twins run roughly parallel courses, 1 or 2 mm apart. In the Sha. twins the curves nearly coincide. Two dwarf girls Nos 1, 2, show mediocre eye slits. Those of the cretins (Nos. 3, 4) are rather large. The microcephalic, V R., has eye openings that are not far from mean size.

Summary.—The width of eye slit increases rapidly during late gestation; slows up after birth. While at birth the slit is less in girls than boys, at 15 years it is about 30 mm for girls and 29 mm. for boys. The children of low intelligence (except mon-

goloids) have a mean larger eye opening than those of the standard series. Individuals vary in slope of the growth curve, from horizontal to rising or falling with age. Twins run parallel curves; our cretins have "large eyes"; our dwarfs and microcephalic mediocre ones.

4 *Interpupillary Angle*

General.—By interpupillary angle is meant the angle subtended by the two lines that pass respectively from the right and left pupils to meet at a point in the mid-sagittal plane where it is penetrated by a transverse line passing through the two porions. Of course, it would be ideal to measure the angle at the optic chiasma as various anatomists have done, but it is not possible yet to do this on living children, so we have to do the next best thing, as indicated.

The measurement of the angle is done by the aid of the depth measurer (occiput touching the wall and the Frankfort horizontal set level) as described by me elsewhere ('37). With the depth measurer the mean distance of both orbits and of both porions from the wall is determined. The difference is the distance orbit to porion, this is the larger of the angle-subtending sides of the right angled triangle, the shorter side is $\frac{1}{2}$ of the inter-pupillary distance. Given these sides the angle at the apex is computed by trigonometry. I cannot find that others have used this method of commuting the interocular angle in the living. Scammon and Calkins ('29, p. 131) give measurements on fetuses of the orbito-auricular distance (see Pl. VI).

The material used consisted of 55 boys and 55 girls at Letchworth Village measured longitudinally, during about 7 years. Also, about 50 babies at Babies Hospital and a few other groups of pre-school age.¹⁶

The problem presented is this. As stated above, at the beginning the eyes develop on the sides of the head, 180° apart. During gestation they come to look forward. Zimmermann, Armstrong and Scammon ('34) have shown how this approach proceeds up to birth, as follows: 2nd lunar month, 180°, 3rd lunar month, 105°.

¹⁶ It was not until the last 6 or 8 years of this research that the horizontal distance from occiput to orbit was measured. In some of the earlier cases the distance from occiput to nasion was measured; also the nasal root depth; by subtracting the latter from the former an approximation is made to the horizontal orbital distance.

CROWN HEEL LENGTH IN CM AND ANGLE IN DEGREES

13.4 cm	99.1°	32.2 cm.	76.8°
17.2	96.5	37.5	80.6
22.3	91.0	41.9	76.3
27.3	86.0	46.5	76.3
		50.1	76.0

Naturally, our angles being taken from a point behind the optic chiasma are of smaller size running from about 50° in late fetal life to about 44° at maturity.¹⁷

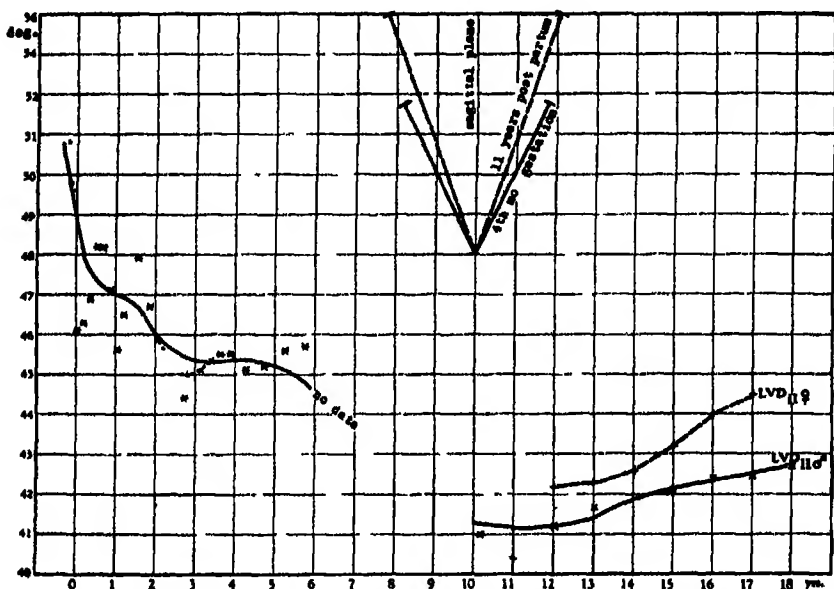


FIG 162 Mass curves of change with age of the Interpupillary Angles Computed from formula

$$2 \times \tan^{-1} \frac{\text{sum of dist bet inner eye angle} + \text{outer eye angle}}{4 \times \text{orbit to tragon}}$$

From fetuses, Babies Hospital and young children, also LVD_{II} series m and f No data for normal children between 6 and 10 years +, male, ×, female, * sexes combined The dotted line is an alternative at the ages indicated Above Changes in mean interpupillary angle between fourth fetal month and 11 years

Sexual.—Figure 162 shows the course of the age-change curve of the interpupillary angle from mid-gestation to maturity The number of determinations before 6 years is small, 3 to 13, at any

¹⁷ I have measured the angle between the pupils with apex at the line drawn between porions from Figs, pp 82 and 84 of the "Manual of Surgical Anatomy" prepared by the Division of General Surgery of the U S Army, 1918 Fig 82 is a horizontal section of the head passing through eyeballs and optic chiasma and about 4 cm above the porion. The angle found was 45°

given year, age and sex. To complete the curve I have had to use data of the LVD_{II} series instead of the BOA, since the BOA series was not measured for this trait. The fluctuation is marked and the curves as drawn do not follow closely the age-means as plotted. But this much seems sure, that from a mean interpupillary angle of 51° before birth there is a mean descent to 46° by 2 years and further to about 45° at 5 years. From 4 to 10 years there are no adequate data. The angle probably diminishes slowly to 11 years, with a mean minimum at about 41.5°. From 12 to 18 years there seems to be an increase of this interpupillary angle of about 15 minutes per year, on the average. Is this in connection with the considerable facial changes that occur at about puberty, especially the elongation of the face? (Figs 146 and 147)

The curve of mean interpupillary angle for girls is quite fragmentary. It seems to indicate that the angle is about $\frac{1}{2}^\circ$ greater in juvenile girls than boys, until after adolescence when the angle in the girls is 1 to 2 degrees greater than in boys. This accords with our findings of the greater palpebral slit of girls from adolescence onward (Fig 159), even in the LVD_{II} series.

The mean angular change from the fourth month of gestation to 11 years post-partum is shown on the upper part of Fig. 162.

Racial — There is no obvious difference in the interpupillary angle between Whites and Negroes.

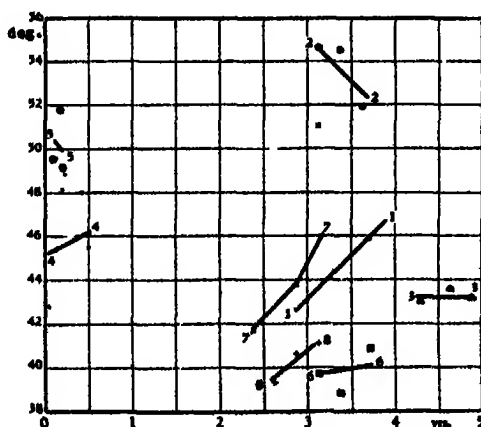


FIG 163 Individual curves of change with age of Interpupillary Angle for eight babies

Individual.—Figure 163 gives for a number of individual babies the curves of changes of the interpupillary angle. Before 5 years two are rising at a steep slope, two show a slight upward gradient, two are horizontal and two decreasing. Notwithstanding, there seems to be, on the average, a declining slope from 20 months onward, though the monthly averages are based on only 4 to 8 individuals.

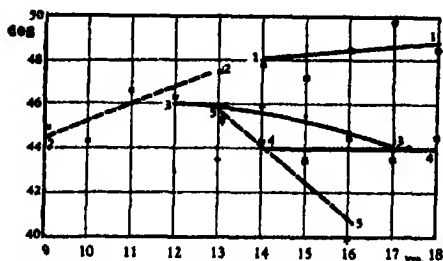


FIG 164 Individual curves of change with age of Interpupillary Angle for five members of LVD₁₁ series. In this figure the orbit distance from tragon is computed by using tragon to nasion minus nasal root depth, and mean results give an orbit tragon distance 2.5 mm. higher than by method of Fig 162. 1, C B m, No 10, 2, C C f, No 133, 3, R J m, No 56, 4, G W m, No 111 (colored), 5, N B f, No 11.

Figure 164 shows, for various LVD₁₁ individuals, the course of change in interpupillary angle. In this series depth of orbit from porion was not used in finding interpupillary angle, but the orbital distance was computed by subtracting nasal root depth from the distance horizontal nasion to occiput. This gives results that average 2.5 mm. too high. The results are plotted without correction. The curve of one colored boy is given (curve No 4).

Familial—Figure 165 gives the interpupillary angle for members of three fraternities. In the Mea fraternity at 17 years the curves are within one point, being centered at 35°. In general, the slope is downward, the interpupillary angle is decreasing. Of Nos 4 and 5 the curve is somewhat wavy. At 15 years the mean angle is 34.06°.

In the War fraternity the interpupillary angle is usually descending before age 15 and ascending afterwards. There is considerable dispersion. At 15 years the mean angle is 33.3°.

In the Sti fraternity also there is a downward slope in one case before 15 years. The other cases are rising after that age. The dispersion is marked. At 15 years the mean angle is 33.3°.

In none of the preceding families is there a segregation on account of sex.

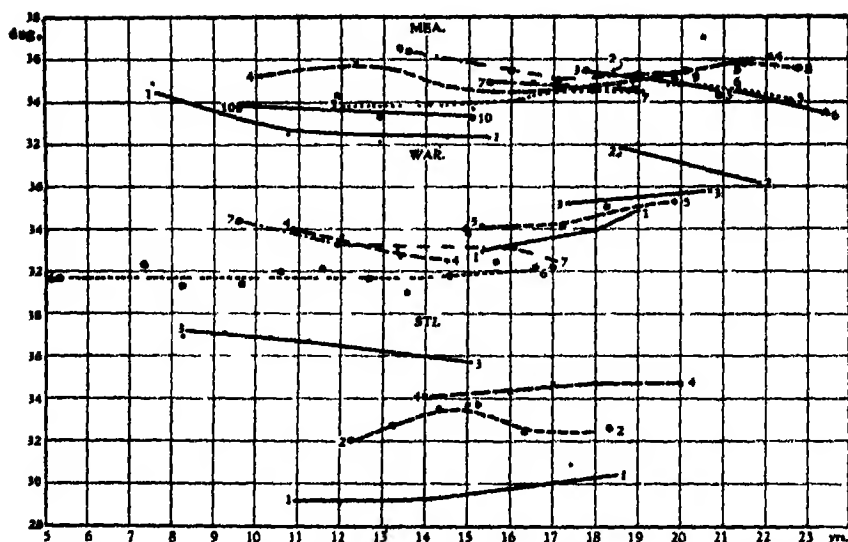


FIG 165 Individual curves of change with age of Interpupillary Angle, computed as in Fig 162, for members of three fraternities. Mea 1, B M f, 2, E M f, 3, H M m, 4, G M m, 5, W M m, 6, L M m, 7, Ma M f, 8, Mi M f, 9, N M f, 10, R M f War 1, Fk W m, 2, B W f, 3, Fd W m, 4, M W f, 5, W W. m, 6, G W m, 7, S W m Stu 1, E S f, 2, L S m, 3, M S f, 4, C S m Three sets of ordinates

Twins and Special Cases.—Figure 166 gives the growth curves of the interpupillary angle for a few years of each of 4 pairs of twins. Of the Gar. twins the curves lie about 1 point apart and run parallel course. Of the Kyr. twins the curves practically coincide. Of the Ols. twins we have only 3 observations in each, and a close coincidence. The Sha. twins are also similar in eye angles.

Of the special cases the locus of the angle curves varies greatly, from that of the cretin, Curve 1, at about 16 years of 54° to the microcephalic, V.R., at the same age of 38.4° . The 6–9 year old cretin, Curve 3, has eye angles at 42.4° to 44° . The ateliotic dwarf, Curve 2, at her 18th year has an interpupillary angle of 49° . Thus in these individuals the interpupillary angle has been stopped at various degrees between its primitive embryonic condition of 180° and the mean adult angle of about 44° , and in some have progressed to much below the mean.

Summary.—The angle made between lines drawn from where the line joining the porions pierces the sagittal plane to the two eye pupils is changing from the first appearance of the eye lenses (180° apart) to maturity. At maturity the angle is, on the

shows some remarkable irregularities but, on the whole, from mid-gestation to 3 years post-partum there is a downward slope, as there also is from 6 to 15 years. On the other hand there seems to be a very remarkable upward slope from 4 to 6 years. As the number of individuals measured longitudinally is very small for these years I am not inclined to lay great stress on this irregularity. It may, however, be noted that bizygomatic width (Fig. 27) has a very slow rate of increase from 3 to 7 years, while that of the interpupillary distance is twice as great as of the bizygomatic. From 22 boys measured as of the age period 4 to 10 years, mostly during a duration of 3 or 4 years, I find that also the ratio distance between inner eye angles — bizygomatic width reaches a maximum at 5 years in accordance with the following means

Years	4	5	6	7	8	9	10
Means	26.20	26.78	26.28	26.11	26.04	25.82	26.51

The results from the means are confirmed from 10 longitudinal series, where the maximum occurs at 6 years in 1 case, 7 years in 8 cases, 8 years in 1 case. That the slope after 10 years is prevalingly downward to the right is shown by individual graphs as indicated below

The curve of the female runs fairly close to that of the male and shows the same upward slope at 4 to 6 years. During infancy and childhood the curve of girls is 1 or 2 points below that of the boys but after about 10 years it moves above that of the boys and stays there till 15 years of age. The two sex curves decussate at between 9 to 10 years. However, in the LVD_{II} series (Fig. 167 below) the curve of the female runs below that of the male curve continuously from at least 8 years, and approaches it at 11 years.

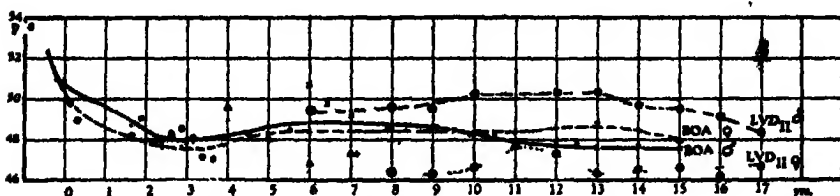


FIG 167 Mass curves of change with age of percentage ratio of Interpupillary Distance to Bizygomatic Width. Standard series, also LVD_{II} series m and f. Symbols as in Fig. 1

Individual—Figure 168 gives individual curves for a few babies and Fig 169 for some older children. The curves of the babies indicate a delicate balancing of the two dimensions, now one now the other changing the faster. But, on the whole, the slope is downward. In Fig 169, after 9 years of age, the slope of the

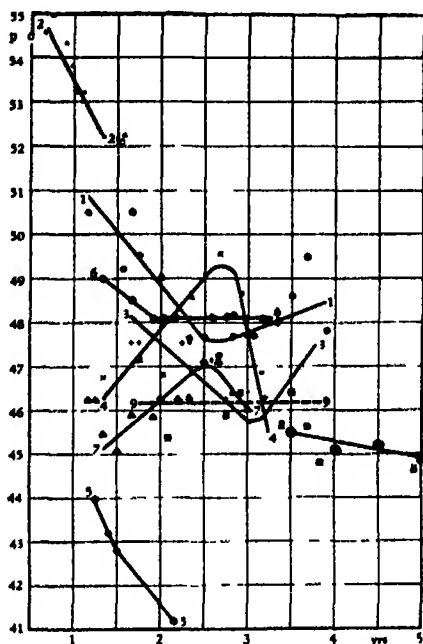


FIG 168 Individual curves of change with age of percentage ratio of Interpupillary Distance to Bizygomatic Width for nine babies, as follows 1, B B f, 2, R L m, 3, R N f, 4, J C m, 5, J M m, 6, D L f, 7, J M m, 8, J W m, 9, M K f

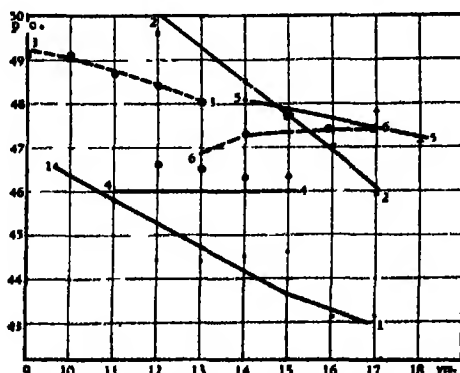


FIG 169 Individual curves of change with age of percentage ratio of Interpupillary Distance to Bizygomatic Width for six members of LVD II series 1, R B m, No 12, 2, R J m, No 56, 3, M. H f, No 51, 4, C A f, No 114, 5, C B m, No 10, 6, E C m, No 58.

changing index is nearly uniformly and definitely downward with age, the bizygomatic width constantly increasing faster than the interpupillary width.

Summary—The percentage ratio of interpupillary distance to bizygomatic width in general decreases with age to 17 years; i.e., the eyes are coming closer together relative to the facial width. Thus the process that begins in the embryo continues to maturity. To this rule there is an exception, namely an increase between 4 and 6.5 years, affecting both sexes

VIII. COMPARISON AND GENERAL DISCUSSION

1. *Growth Curves of the Main Cephalic Dimensions*

The present study of the post-natal development of the head is concerned with cranium, face and eyes. Especially in cranium and face the number of dimensions measured and ratios computed is considerable. Of the cranial dimensions it is characteristic that (in contrast to those of the nose) all grow rapidly in intra-uterine life and for a year thereafter, and then slow down. Thus head width has practically finished its growth at 3 years post-partum, while head length continues its growth at a greatly reduced rate.

The form of vertical *dimensions of the head* is greatly modified by the pull of gravity directly upon the brain and its plastic brain case. The depression of the brain tends to increase cranial maximal width, and especially the minimum frontal width. A real cessation of growth in certain dimensions for two or three years is found, during late childhood,—in head height, head width and postauricular segment. The head girth, though influenced by several linear dimensions, gives a curve that is strikingly free from irregularities.

The growth curves of vertical *dimensions of the face* are far more irregular than those of the cranium. The irregularities are probably due in part to the action of gravity, but more to the numerous and relatively large changes that are taking place in the bones of the face. The principal growth-modifying changes are:

1. Enlargement of the maxillary sinus from a space 5.5 mm. high \times 4.6 mm. wide \times 13.3 mm. long at birth, to one 25 \times 21 \times 35 mm. at about 17 years (Davis, '14, p. 81).

2. The development of the sphenoidal sinus at the base of the cranium must also play its part in the forward protrusion of the face. It increases from a cavity $2.8 \times 2.0 \times 1.6$ mm. at birth to one $20.0 \times 20.0 \times 25.0$ mm. at 17 years (Davis, '14, p. 163) By its development the face must be pushed forward by a good part of an inch

3. Development of the alveolar part of the maxilla and the mandible, first as milk dentition develops and later as this dentition is replaced by the larger teeth of the permanent dentition and the 3 molars come into play with an addition to the length of the jaw of from 25 to 35 mm

4 In response to the pull of the masticatory muscles which causes the zygomatic arch and body of mandible to enlarge

Thus the visceral part of the head increases in height and depth, though to a far less degree than in anthropoids, where the teeth are mostly wider and a fourth molar is frequently present in orang and gorilla

The age changes in *head proportions* are more varied and instructive than those of absolute dimensions

The relation of the 3 major axes of the cranium to each other is complicated by the fact that the skull at birth is so plastic that it is easily distorted temporarily, also that at 9 or 10 months as the baby begins to walk the head height diminishes and width increases somewhat correspondingly, but the genetically determined head form soon asserts itself. There is evidence that pre-natally the form of the cranium adapts itself to the prospective cross sectional form of the birth canal and hence is brachycephalic After birth and reconstruction the cranium elongates and dolichocephaly (if the genes for dolichocephaly be present) is gradually established

To throw some light on the way in which the genes for dolichocephaly do their work I present in Plate II horizontal outlines (based on sets of only 4 measurements) of the heads of 2 boys (Plate I)· (*B*) one with a cephalic index of 78, (*A*) with a cephalic index of 83 In the case of *B*, the boy with narrow head, from 13 to 18 years the length increased 14 mm., the width 6 mm. In the case of *A*, with wider head, from 13 to 18 years the length increased 8 mm. and the width 7 mm. Thus in *B*, as compared with *A*, the increase in length in the 5 years was 2.0 times that of *A*, while increase in width of *B* was only $4/7$ or $5/7$ times that

of A. At least during adolescence and early puberty in the case of the more dolichocephalic boy, growth in length is much greater than in width, while in the more brachycephalic boy this is not the case. As the brain case is, doubtless even at 18 years, being molded to the form of the brain, we have to conclude that a different brain form is developing in these two boys and that, in accordance with all experience, is determined by genetic factors. The beginning of that difference anteceded 13 years of age and,

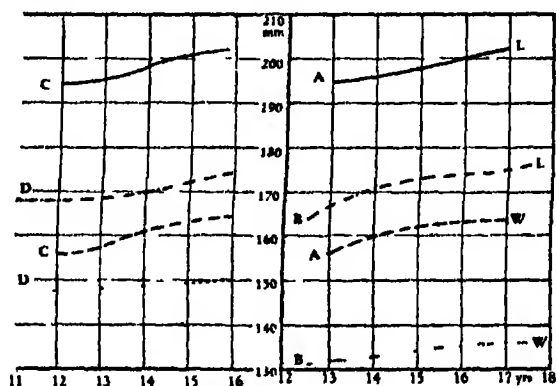


FIG 170 Growth curves of Head Length (L) and Head Width (W) for four boys A, LVD₁₁ No 22, B, No 49 (see Pls I, II), C, EH No 79, D, OR No 90

we may guess, went back to early infancy Fig. 170, right, shows that the curves of growth of the cranial dimensions of these two boys run at quite different levels.

The curves of head ratios involving head height are irregular in middle infancy because of the deformation of head height through the action of gravity. And where vertex height enters into the ratio the reduction of this dimension in passing from the height measurements of early infancy to the measurements when standing results in a notching of the growth curve as we see in Fig. 71. Even later, the activity of boys in jumping, especially off high places, may cause a depression of the skull. But after deformation, if not too severe or prolonged, the genetical factors tend to restore, but frequently fail to restore completely, the family form of head.

The relation of the dimensions of the horizontal segments of the head to total head length, is complicated by the migration of the ear capsule and its external meatus. In infancy the proportion of the postauricular segment tends to increase rapidly,

then diminish to about 10 years and then to increase slowly during adolescence and up to 19 years or older. The infant at birth has a relatively small postauricular segment and resembles in this respect the anthropoid apes. The decrease around 10 years is not easy to interpret.

On the other hand the transverse frontal index decreases rapidly to 4 years as the head increases in width over the ears. After that there is a more rapid increase of forehead width than maximum head width bringing about the typical human broad forehead from the relatively narrow, anthropoid condition—again a case of recapitulation

The curves of both head length and of cranial capacity in relation to stature tend downward with age. This is, of course, a consequence of the rapid growth of the leg bones, which after birth grow the most rapidly of any parts of the body, and while the brain case as a whole (Fig. 102) grows greatly during the first 1 or 2 years post-partum it practically stops growing at 3 years

The face changes, during post-natal development, more than the cranium, in part because the elements that enter into facial form are so numerous. Facial form has undergone a vast number of mutations in the races of mankind, it shows a vast number of familial differentiae. Among them are height of nose, of upper lip, of lower jaw and chin. The relation of these to the width of the face has to be considered. The chin alone deserves a much fuller consideration than has been hitherto given to it, we are able to furnish a few additional data.

The morphological face height (nasion to gnathion) grows very rapidly especially before birth but also until 3 years of age and then the rate of growth slows down for a time. The height of the face is increased strikingly at the time of the adolescent spurt of growth. Indeed, the whole body is affected by that episode, though not all parts to the same degree (Fig. 108)

The three subdivisions of the morphological face height—nose, upper lip, and lower jaw—have different histories of growth. The mean nose height decreases rapidly proportionally pre-natally, but from birth on increases to around 43 per cent of M.F.H. The mean lower jaw increases proportionally pre-natally, decreases a little (perhaps) during the first post-natal year and, with some irregularities, increases slightly (about 1 per cent) to maturity

when it is 38 per cent of M.F.H. The mean upper lip tends to decrease proportionally from birth (at 22 per cent) to 15 years when it is 18 per cent of M F H Thus the vertical nose dimension increases at the expense of the upper lip

Relatively to mean bizygomatic width, the mean physiognomic face height tends to increase from birth (when it is 95 per cent) to 5 years and is thereafter stabilized at about 132 per cent, while the mean morphological face height increases very rapidly from 77 per cent at birth to 89 per cent at 15 years, and probably higher at 19 years. Thus the squat faced baby tends to become longer faced with the development of the visceral skeleton

The mean distance between the pupils of the eyes of course increases as the head enlarges, but relatively to the bizygomatic width of face the eyes approach (except perhaps for a period from 5 to 9 years). The mean angle between the eye pupils, whose apex is at the interporial line, decreases from about 51 degrees at mid-gestation to 41 degrees at 9 or 10 years and thereafter seems to increase slightly. Thus the eyes which are laid down probably over 160° apart have not completed their relative migration toward each other until adolescence begins

2. The Head and Face in the Neo-natal Period

Nothing demonstrates better the plasticity of the skull in the pre-pubertal period than the extraordinary alterations of form that it undergoes in the circum-natal period These alterations are best exhibited in the figures giving changes in proportions in individual children Look, for example, at Fig 51 giving the changes with age of the cephalic index. In Fig. 58, which shows the changes in relation of height to length, the curves for the first 4 post-natal months seem meaningless or fortuitous These changes are associated in part with compressions while passing through the birth canal, and in so far may be regarded as largely fortuitous. The changes in proportion of head height to width are complex for 12 months post-natal (Fig 65) and here gravity plays a part both in reducing height and expanding width We see that after 24 months a comparatively quiet and orderly development again proceeds

The curve of changes in frontal width index at, and for a few months after, birth, as shown in Fig. 79, suggests that the maximum head width is increasing at the expense of the forehead in

part as an adjustment to the form of the birth canal, but that directly after birth the frontal width index is relatively increased during the first year, and then diminishes as the growth of minimum frontal width slows down practically to zero (Fig 23) while head width increases for a while. The result is the frontal index reaches a minimum during the fourth year

It is during the first two post-natal years that the annual increment of the cephalic module falls from 150 to 15 the head growth is nearly completed (Fig 102)

Gravity acts on the face as well as the cranium, when the "first steps" are taken by the infant. Accordingly, as shown in Figs 108, 113, 136, 141 and perhaps 149, the curve of age-change exhibits a notch at this age I can not find that this modification of the growth curve has been hitherto mentioned.

3 Growth of the Head in Relation to Sex

Since the female sex has, on the average, a smaller body than the male it is to be expected, and it is found, that all of the mean absolute head dimensions are smaller in girls than boys (Figs. 1, 9, 15, 23, 27, 33, 42).

In the case of ratios the relation of the sexes is diverse and often changes with age Thus the mean cephalic index and the head height/length ratio are smaller in girls until 6 years of age, larger thereafter, but in the mean head height/width ratio (Fig 64) the decussation occurs at 2 years There is little difference between the sexes in relative head length, but the female curve lies half a point above that of the male after 2 years The mean proportion of the postauricular part of the head is greater in the female after 4 years of age (Fig 74) The mean cranial capacity runs about 100 c c. less in girls than boys from 6 to 16 years (Fig. 89) and is even less in relation to stature (Fig. 94) and to the body modulus (Fig. 100). The girls have the narrower forehead in relation to bizygomatic width.

The mean absolute facial measurements are, of course, smaller in girls than in boys, at least after 7 years. And of the ratios the girls show themselves the smaller in proportion of morphologic to physiognomic face height (Fig. 127) and of lower jaw to morphologic face height; but the larger in proportion of the nasion-stomion distance to morphologic face height. In the relation both of physiognomic face height and of morphologic face height

to bizygomatic width the mean curves of the girls lie below those of the boys.

Of the eyes the mean absolute distance is less in girls than boys. In the case of width of palpebral slit the girls' mean curve seems to lie slightly over the boys', at least after adolescence sets in.

4 Growth of the Head in Relation to Social Type

In head length, head width and head height the most rapid growth occurs in the standard series, the least in the mongoloids. The Idiot and LVD series are intermediate. Likewise the curves of head girth, cranial capacity, absolute and relative to stature, fall into the same order. On the other hand, in cranial capacity relative to body modulus, after 11 years, the standard series lies at the bottom, the mongoloids on top. This means that the trunk proper is relatively less bulky in relation to cranial capacity in the mongoloid dwarfs than in the standard boys. Within the limits of measurement of cranial capacity and of trunk one may say that the cubic capacity of trunk compared to that of the brain in the mongoloid dwarf is smaller than in the case of the standard boys. In other words, in the defectiveness of development of the mongoloid dwarf the brain size is less affected than trunk size. In other words still the mongoloid dwarf is more infantile in head trunk-proportions than non-mongoloids.

Of the head indices the mongoloid has the largest cephalic index and index of height to length, in accordance with the casual observation of their very short heads. Our standard series and the Idiot series have lowest indices (long heads). The height/width indices take various positions in the different social groups at different ages and this is true of the relative head length also. In the minimum frontal/bizygomatic index the standard group is highest. The mongoloids come next below at 11 years and are at the bottom at 19 years. The LVD and Idiot groups are intermediate. The relative width of minimum frontal increases roughly with intelligence.

Of facial measures the LVD₁₁ and Idiot series showed the greatest morphologic face height and upper morphologic face height, while the mongoloids showed the least. In forehead height the mongoloids stand first, on account of the sparseness of hair at the margin of the scalp. In lower jaw height the BOA

group is greatest after 12 years and the mongoloids least before that age, but later the mongoloids attain a higher position. In relation of morphologic face height to physiognomic the mean I series stands highest, the mongoloids lowest. Excepting the latter the BOA group stands at the bottom. In relation of upper to total morphologic face height the BOA, with their considerable nasal height ('39, Fig 2), are intermediate, the I series highest. In chin height relative to morphologic face height the I and M series lie low largely because of the relatively low chins. In the physiognomic face height/bizygomatic index the mongoloids stand on top because of their hairless upper foreheads, and the I series at the bottom. But the position of the mongoloids is reversed when the morphologic face/bizygomatic is considered.

Among eye dimensions the interpupillary distance is greatest for the BOA group and least for the mongoloids. The distance between the inner eye angles, which is largest in the mongoloids of 8½ years, corresponding to their low nasal root and epicanthus, becomes at 17 years for that group smallest of all, corresponding to their small head width.

5 Growth of the Head in Relation to Race

The form of the head is commonly considered an important racial character. In our studies we were confined to a consideration of four groups: 1, U S mixed and North Europeans ("Nordic"), 2, Italian, 3, Jewish, 4, American Negro (Negro × White hybrid). Groups 2 and 3 are sometimes combined as "Mediterranean" where the differences are small (see also '39, p 289).

Means of Absolute Measures of Cranium—Head length is largest in the Negroes and smallest in the Italians. The Nordics are intermediate. In head width the Nordic curve is uppermost, and the Italian lowermost until 15 years when their place is taken by the Negro. This change in position is a curious fact to be noticed in other cases. It is probably due to inclusion of a relatively large number of individual curves that are concave above. In head height the Nordics are above the others at most ages, though slightly overlapped by the Negro at ages 12 to 14 years. The Italian group have the lowest position.

Of the mean absolute dimension of post-occipital part of the head the curve of the BOA group is on top after 12 years, the Italian at the bottom. Before 12 years the Negro curve lies highest.

The mean girth of the head of the Negro in our series is greatest, that of the Jews least

To summarize The mean Negro head is extremely long, rather narrow and fairly high and has the greatest girth of all. The mean Nordic group has the widest and highest crania and the greatest postauricular length

Means of Cranial Ratios—The cephalic index of our series is largest in the Mediterranean group and smallest in the Negro (after 11 years) because of their long heads. On the same account the Negroes' mean height/length ratio is the smallest of all. The Nordics have the highest height/length ratio. In mean height/width ratio the Mediterranean group lies lowest, the Negro, after 12 years, on top. In relative head length the curve of the Nordic group is on top, that of the Negro at the bottom despite their long heads because in our sample the mean statures of the Negroes are greater than that of whites. The ratio of postauricular part of head to total head length is greatest in the Nordic group and least in the Negro, at least from 12 years on. The ratio of frontal to maximum head width is highest in the Negroes after 12 years and least in the Nordic series, which is the reciprocal condition to that of maximum head width. The ratio of minimum frontal to bizygomatic width is larger for the narrow faced Nordic than for the broad faced Negro

In cranial capacity the Negroes and Whites run close together, the Italians are least. In relation to stature and to body modulus, however, the order is somewhat reversed, the Negroes being less than the Mediterraneans. The applicability to Negroes of the formula used is very doubtful.

Means of Facial Segments—The physiognomic face height, morphologic face and forehead height and lower jaw height are comparatively greatest in the Negro after 12 years. In all curves of facial height and proportions the Nordic group occupies an intermediate position. In the ratio of M.F.H. to bizygomatic width the curve of the Negro occupies a high position despite the wide face.

Interpupillary width is highest in the Negro but, rather unexpectedly, the distance between inner eye angles is less in Negroes than in Nordics. The eyes of the Negro show a very wide palpebral slit

In general, the Negro children are differentiated from those of European stock by higher, broader and larger faces and larger eyes

6. Individual Growth and Change in Cranial Form

The absolute dimensions of the cranium advance regularly with age. Head length usually shows an increased upward slope during the 15th year. This may be a part of the generalized growth increment at the time of the adolescent spurt. It is doubtless partly due to the enlargement of the frontal sinuses, or at least to the thickening of the cancellous part of the frontal bone into which the enlarging sinus usually penetrates.

In head width (Fig 11) the spurt of growth seems sometimes to take place earlier than in head length, namely at 12 years.

The head height curves (Fig 17) show great variability of form which is doubtless largely associated with the movements of the external auditory meatus and possibly with the activity of the individual boys.

The great variation in the development of the three dimensions is illustrated in Fig 4 for two boys. In the boy with the shorter head the head length is scarcely greater than the head width of the boy with the greater width.

The differences in growth of the cranium in two other boys is illustrated in Plates I and II and in Fig 170, on page 198. The bulging head width of A in contrast with B is seen in frontal view as well as in Pl. II, Fig A.

In the growth of the minimum frontal width increase is usually rectilinear but the slope varies greatly. In a few cases (Fig 24, Nos 4, female, and 7, male) the growth in width is slowed up prematurely or accelerated during puberty.

Bizygomatic width (Fig 28) rarely advances rectilinearly and the slope tends to increase even after adolescence. It occasionally proceeds in a series of waves. The growth of the maxillary sinus during this period must affect this dimension. The advance of bigonial diameter (Fig. 34) is not so irregular, but the growth curves diverge in striking fashion from 8 years to 18. This divergence is not merely a matter of difference in general body growth, for Nos. 1 and 2 had at 17 years the same stature, viz. 162 cm.

Of the postauricular part of the skull the growth curves vary greatly. There is the regularly and rapidly increasing type (Fig. 39, curve 1), the type with rapid increase of gradient after adolescence (No 2), the type that does not change from 11 to 17 years (No 6). No. 1 has a cephalic index decreasing from 86.7 at 12½

years to 84.4 at 17 years, becoming less brachycephalic. On the other hand No. 6's cephalic index remains constant around 81.5, already almost mesocephalic.

Head girth curves show striking differences in level, with a rise with age that is steady, except for increments at the spurt of growth.

The cephalic index, after violent fluctuations right after birth, reaches a maximum at between 4 and 12 months and thereafter slopes downward. Until maturity the head tends to become constantly more dolichocephalic. This is the direction of phylogenetic progress.

The head height/length ratio curves show remarkable individual differences including rising, falling and U-shaped types. The head height/width ratio is again high and fluctuating in early infancy (Fig. 65). The later course of the ratio shows several types (Fig. 66) slowly rising with age (Nos. 3 and 4), falling with age (No. 8), wave-like (No. 1), and unchanging (Nos. 5 and 7). Obviously the development of these two dimensions is a rather closely balanced one.

Relative head length shows in all individuals plotted a downward curve, owing to control of its course by leg development.

As there are several types of development of the postauricular part of the cranium, so there are of the postauricular/head-length index. Before the onset of adolescence the slope of the curve is downward, thereafter upward. That is, in relation to increase of head length the postauricular segment is at first falling behind, then advancing relatively. The large postauricular segment is a late phylogenetic achievement.

The forehead width in relation to maximum head width is an increasing ratio in the individuals plotted (Fig. 80, curves selected for length of the series). The relatively wide forehead is a phylogenetically late acquisition (compare a recent skull with that of *Pithecanthropus* and *Sinanthropus*). But in relation to bizygomatic width the forehead width diminishes with age (Fig. 86). This change in the ratio seems to be a physiological one due to relative increase of bizygomatic width with increasing use of the masseter muscles (Fig. 28) after infancy.

In all individuals plotted cranial capacity increases with age but decreases sharply in relation to stature.

7 Individual Changes of Face Segments

Physiognomic face height (Fig 104) and morphologic face height (Fig 114) increase with age but at different rates so that a change in relative position occurs among the individuals of a group. Of forehead height the growth curves are usually progressive, sometimes forming a straight line from 9 to 16 years, sometimes forming a shallow U. These changes are due to change in the position of the hair line in the individual, for this is not so nearly fixed as are skeletal traits. The chin height increases more or less regularly but not in all individuals in parallel curves (Fig 124), showing the influence of special glandular activities. This is true also of chin height in relation to morphologic head height (Fig 138) though here different types of curves are apparent, increasing, decreasing or U-shaped.

The relation of morphologic to physiognomic face height is shown in individuals (Fig. 128) as wavy lines which in the mass curve blend into a line that approaches rectilinearity (Fig 127). The maxima in the individual curves indicate episodes in the growth of the middle face, cutting of second molar at adolescence, rapid growth of maxillary sinus at about 16 to 18 years. These facial changes are brought out no less clearly in Fig. 142. The lower morphologic face in relation to total face (Fig. 137) shows varied forms of curves. A delicate balance between this segment and the whole face height exists, but is readily upset.

The individual curves of physiognomic face height to bizygomatic width are of several types as shown in Fig 142, level, rising with age, convex above, concave above—all depending on the interrelation of the growth of the vertical and horizontal dimensions of the face at different ages. The relation of morphologic face height to bizygomatic width (Fig. 146) is less varied.

8 A Comparative Study of Some Twins

The method of growth of cephalic and facial dimensions and age-changes in proportions of twins is shown in Figs 7, 14, 22, 26, 32, 36, 41, 47, 55, 62, 69, 72, 77, 83, 88, 93, 98, 107, 117, 122, 126, 131, 135, 140, 144, 148, 153, 158, 161 and 166. Those whose dimensions are most commonly graphed are designated as Gar, Kyr., and Ols. These three are regarded as monozygotic both because of extraordinary facial resemblance, resemblance in physical dimensions and proportions and similarity of papillary ridges

and dermatoglyphs (see '39, Pl V, Fig 3 for photograph of Gar. twins) The first pair is of Italian origin, the second probably Nordic (Scandinavian?), the third bears a Scandinavian name. The first pair are girls, the others, boys In addition, the dimensions of the Sha. twins are sometimes graphed, though our experience with them lasted only through 4 years. They were Jewish boys and though they heard well they were both mute. In addition, data are given for the She. twins, believed to be dizygotic, the Dot (colored) and Ton twins, both certainly dizygotic.

In some cases, as in Fig 14 (head width), Fig. 26 (forehead width), Fig 32 (bizygomatic width), Fig 36 (bigonial diameter), also Figs. 47, 72, 77, 83, 88 and 153, the curves of monozygotic twins practically coincide through their entire distance. In other cases (Figs 41, 47, 55, 62, 93, 98, 148) the curves of the twins lie further apart but have parallel courses The Kyr and Ols. twins also run closely parallel courses in respect to head development. On the other hand the dizygotic Dot. twins (Figs. 62, 69, 77) run curves that are far apart and frequently far from parallel In one dimension the development of the Gar. twins is quite widely separated, namely head length, also the ratios (Figs. 55, 62, 77, 98) into which it enters. A study of the frontal sinuses of these twins taken during a series of years indicates a markedly larger sinus in No 2

The parallelism of development during many years of the dimensions of monozygotic twins is a striking fact and suggests that were it possible to observe the infantile and even embryonic stages we should find a similar resemblance The resemblance goes back, no doubt, to the implanted egg

9 A Comparative Study of Some Special Cases That Reveal Morphological Abnormalities

Of the morphologically aberrant cases we have considered dwarfs of both the ateliotic (A W. and M W.), and achondroplastic (L S.) types, three cretins (A.A , C B. and H.L), a microcephalic boy (V.R.) from 9 to 8 to 19 to 13 years. Also single observations were made on other types.

Ateliotic Dwarfs —The children studied were not extreme cases of this category, but they were markedly dwarfish and belonged to no other category with which they were compared. They were not achondroplastic, mongoloid or hypopituitary cases.

The A.W. and M.W. sisters were of Irish extraction. They had light or medium brown hair, blue eyes and a slight anterior lumbar lordosis.

A.W., at 12 1 years stature 114.1 cm. (19 cm. below standard), weight, 19.28 kg. (10 kg. below standard). At 22.6 years stature 130.4 cm. (30 cm. below standard), weight 52.47 kg. (3 kg. below standard). Her head length (Fig. 8, No. 5) is only about 2 mm. greater than that of the microcephalic V.R., curve No. 4. Her head width and height are the lowest in the figure except those of the microcephalic. As Fig. 73 shows, A.W.'s head length in relation to stature is very small, lies next above that of V.R., the microcephalic. The cranial capacity absolutely and relatively is also small (Fig. 93). Among non-microcephalics A.W. and her sister have very small heads, even when body size is taken into account (Plate V, also Fig. 78) the postauricular segment of the cranium is small. The forehead is relatively broad (Fig. 88). The absolute dimensions of the lower face are small. In facial ratios her position is intermediate. The eyes are extraordinarily close together (Fig. 153) but the interpupillary angle is fairly large. In a word, the head of the ateliotic dwarf shows many striking infantile characters.

Achondroplasics.—The achondroplastic dwarf, L.S., of German-Jewish origin, at 10 years had a stature of 1098 mm., or 85 p.c. of the Nordic standard. Her relative sitting height was 56.92, or 108 p.c. of standard, due to short lower extremities.

Of L.S. the head dimensions were small except head height. The head girth is 89 p.c. of standard. The relative head length is about 115 p.c. of standard owing to short legs. The approximate relative cranial capacity at 15 years was 65, or about 104 p.c. of standard. The morphologic face height is low, about 80 p.c. of standard. The M.F.H. in relation to bizygomatic width is at 15 years 79 p.c. or 92 p.c. of standard, i.e., the face is wide.

E. and K.S. (Italian) are sisters. At 22 years their statures were 125.5 and 126.8 cm. respectively, or about 79 p.c. of standard. Their relative sitting heights were about 57.5 cm., or 107 p.c. of standard. Of the E. and K.S. sisters both have heads that are about 92 p.c. of standard. Head widths are more nearly standard, head height is 93 p.c. of standard. The bizygomatic width is slightly substandard, minimum frontal width standard, but large for their statures; the interpupillary distance about 96 p.c. of

standard Italians, the morphological face height about 92 p.c. of standard. The relative head length is large because of the small denominator (short legs). The cephalic indices are 82 and 82, not far from the racial mean.

Cretins —Of the two cretinous girls, A A seems not to have received thyroid treatment, the other, C B, has received it intermittently.

A A, measured between 6.5 years and 10 years, had, at the first age, a stature of 99.3 cm., or 88.3 p.c. of standard. At 10 years stature was 114, or 88.7 p.c. of standard. C.B.'s stature rose from 87.6 p.c. of standard at 13.9 to 92.0 p.c. at 18.7 years. The head of A A is of standard length, but exceptionally wide, consequently her head girth is large despite small general body size. C B, also, has a head that is wide and high. The cephalic index of both cretins is in the 80's, or high mesocephalic. The facial measurements are intermediate, the faces narrow, the interpupillary angle large.

Acromegalics —Two acromegalic brothers A D. and V D. were measured in their late 20's. At that time A D. had a stature of 1860 mm., or 105 p.c. of standard, V D. one of 1800 mm., or 101 p.c. of standard. All cephalic measurements were large: length, for each, 204, width, 154 and 158, height, 138 and 132 respectively. The head girth was 580, or about 105 p.c. of standard. The morphological face height of the older brother is 128, which is about 110 p.c. of standard. Especially the height of lower jaw is considerable—4 or 5 mm. (9 p.c. greater than standard). The bizygomatic width is about 105 p.c. of standard. In these young acromegalics the long face and heavy jaws are already striking.

Microcephalics —The development of two brothers, V R. and F.R., of Italian origin, was followed for a time as examples of microcephaly. V R. was followed for 10 years from 10 to 20 years. His brother was of too low grade to cooperate satisfactorily. V R. at 10 years was 118.2 cm. tall, at 20.7, 158.0 cm. These are about 20 (or 85.5 p.c. of standard) and 15 cm. respectively below standard. At 16 years V R.'s head dimensions were as follows: length, 16.0 cm. (or 89 p.c. of Italian standard), width 11.6 cm. (or 77.7 p.c. of standard), height, 10.5 cm. (or 88 p.c. of standard). Thus, head width was the most reduced. Minimum frontal width is 89 p.c. of standard. Bizygomatic width is 86.5 p.c., postauricular cranial segment is 84.5 p.c. of standard. Head

girth is 82 p c and cranial capacity 912 cc. or 73 p.c of standard. Relative cranial capacity is 78 p c , cephalic index, 72.4. V.R. is extremely dolichocephalic and more so than Mesek, since head width is so much reduced (see Plate IV). The interpupillary angle is fairly high. The head of V R is higher, shorter and narrower than Mollison's ('26, p 111) Mesek.

We have already seen (page 29 and Fig 20) how far below standard the three main cephalic dimensions of V R are. One notes (Figs 8 and 20) that in head length and height V R has a steeper upward slope than standard, so that in these respects the head of this microcephalic is in his late years approaching standard. Birkner ('97) noted that in his "Azteken" advance between late childhood and adulthood is relatively greater than normal.

IX. SUMMARY OF CONCLUSIONS

The outstanding result of this quantitative study of the growth of the head and of changes in its proportions seems to be this

The head, which appears to be so resistant to deformation in the adult, is really in the living person a highly plastic organ. The cranium is a membrane surrounding and, with the cooperation of the skin, protecting the brain. It wraps around and is molded by the developing brain. Constantly it is subjected, and responsive, to surrounding conditions, in the uterus, during parturition, in bed during infancy, through the first months of standing, during childhood in consequence of activities.

The development of the cranium shows adaptation at every stage. This adaptation is especially marked before birth in preparation for that event. Perhaps the backward movement of the auricle and the occiput are an adaptation to the balancing of the head on the vertebral axis. The reduced visceral part of the head appears to be an adjustment to replacement of jaws by arms as organs of defense and to a less crude diet.

A part of the changes in the general form of the head and especially of the face are the mechanical result of development and increase of special organs. Such, *e.g.*, are the teeth and alveolar processes, the maxillary sinus and the thickening and sinus formation occurring in the glabellar region.

The development of the head reveals the influence of determiners that were most functional in ancestors of man and that are now

largely inactivated by new and later determiners or genes. Illustrations of this principle are found at every stage: *e.g.*, in the replacement of a small postauricular segment by a large one, in the more rapid post-natal growth of head height than head width; the late backward migration of the auricles giving more room for the frontal part of the cerebrum, the rapid dampening off of growth of the bizygomatic width after birth, the uncertain and futile developments of the frontal sinus.

Thus the adult living head shows itself as the product of both environmental and genetical factors, and the genetical factors are both old and new or recent, and between them there is, as it were, a struggle during development, the new factors coming into function later and having the most influence on the details of the final human form as contrasted with the anthropoid form

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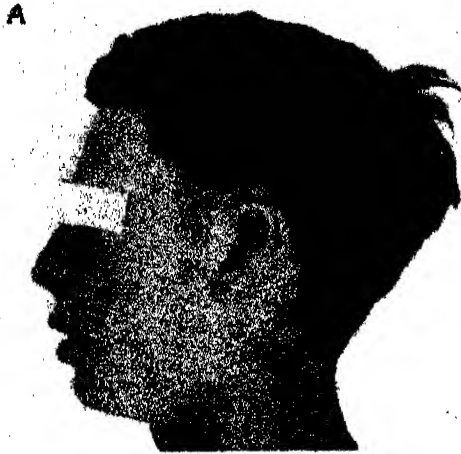
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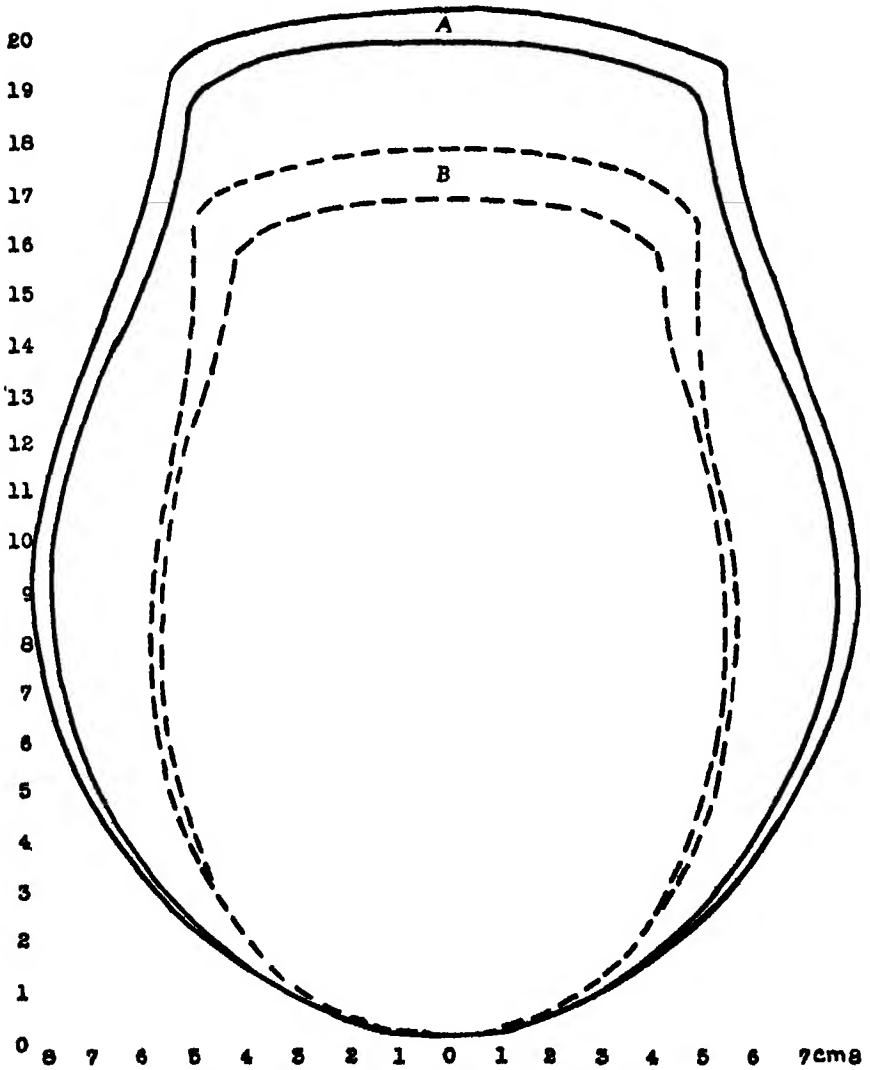
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PLATE I



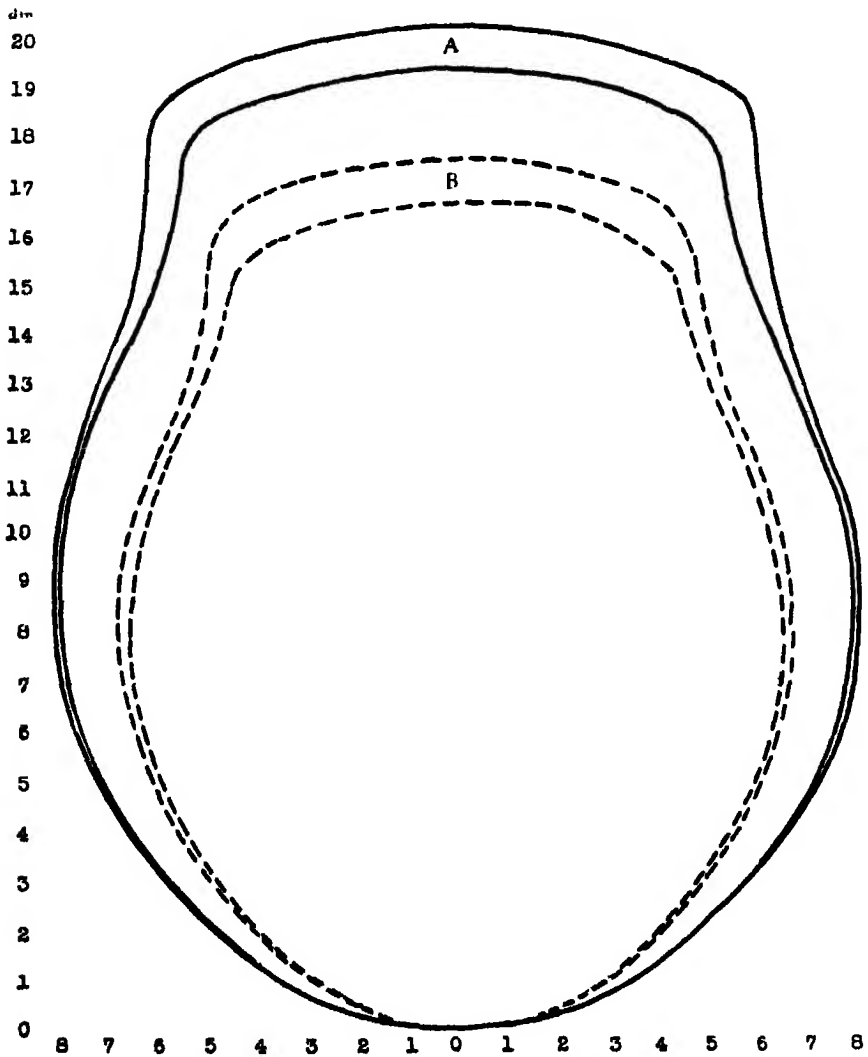
Front and side views of heads of two boys. *A* and *A'*, L.C. No. 22, large headed type. *B* and *B'*, I.G. No. 49, smaller headed type.

PLATE II



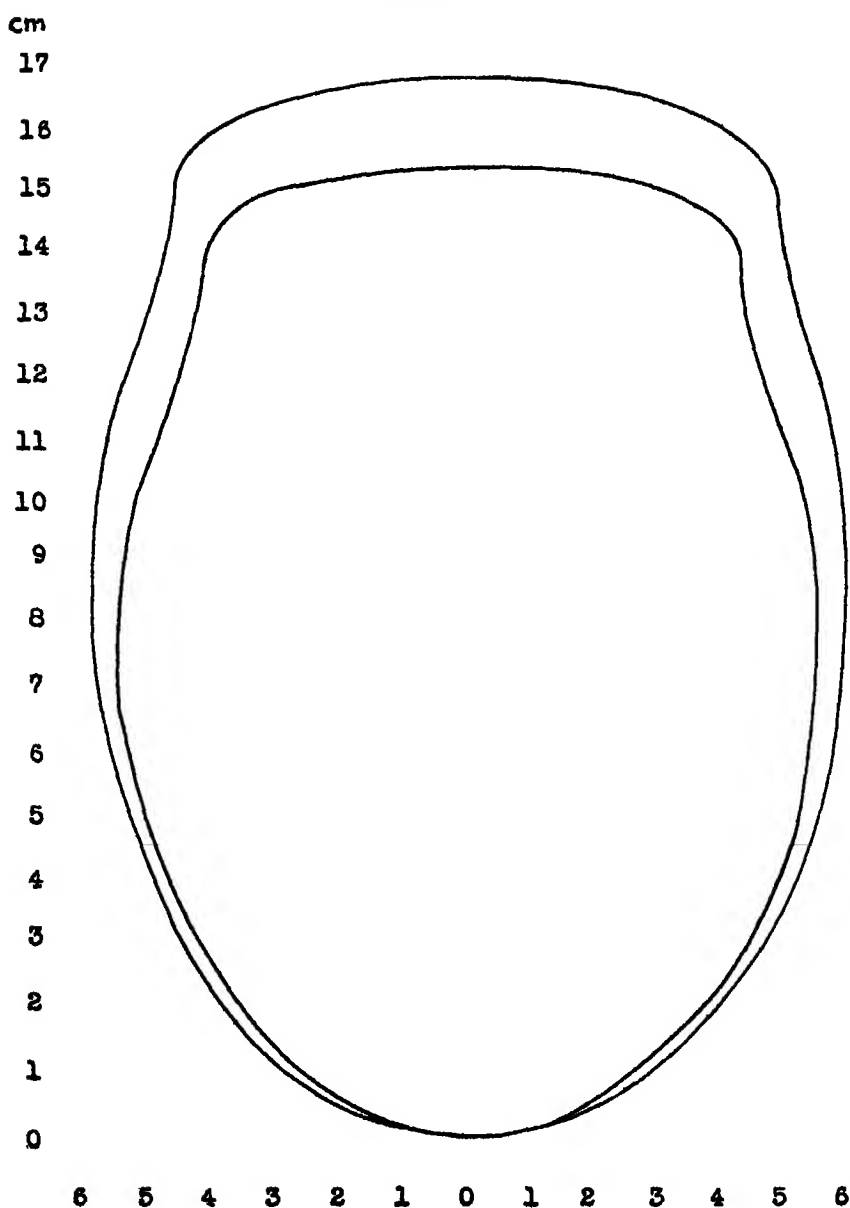
Outlines of horizontal sections of heads of same boys as shown in Plate I. A, L.C. No. 22, at ages 18 years (inside line) and 18 years (outside line). B, I.G. No. 49, at ages 12 and 18 years respectively. Reduced to 66 per cent of natural size.

PLATE III



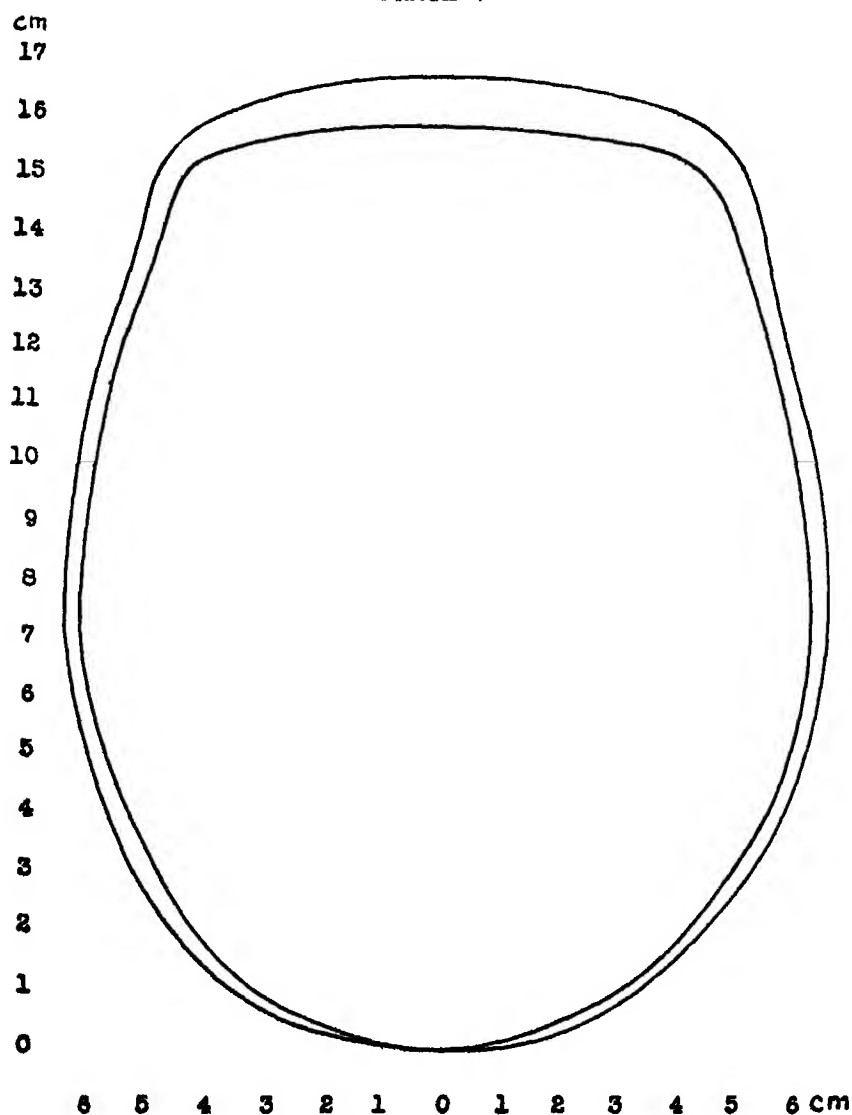
Outlines of horizontal sections of broad heads A, R N (No 79) at ages 10.6 and 17.0 years respectively B, same for D R. (No 90) at ages 12.0 and 17.0 years respectively No 79's stature at 12 years, 143.9 cm, at 17 years, 173.1 cm No 90's stature at 10.6 years, 129.5 cm, at 17 years, 154.3 cm Reduced to 83 per cent of natural size

PLATE IV



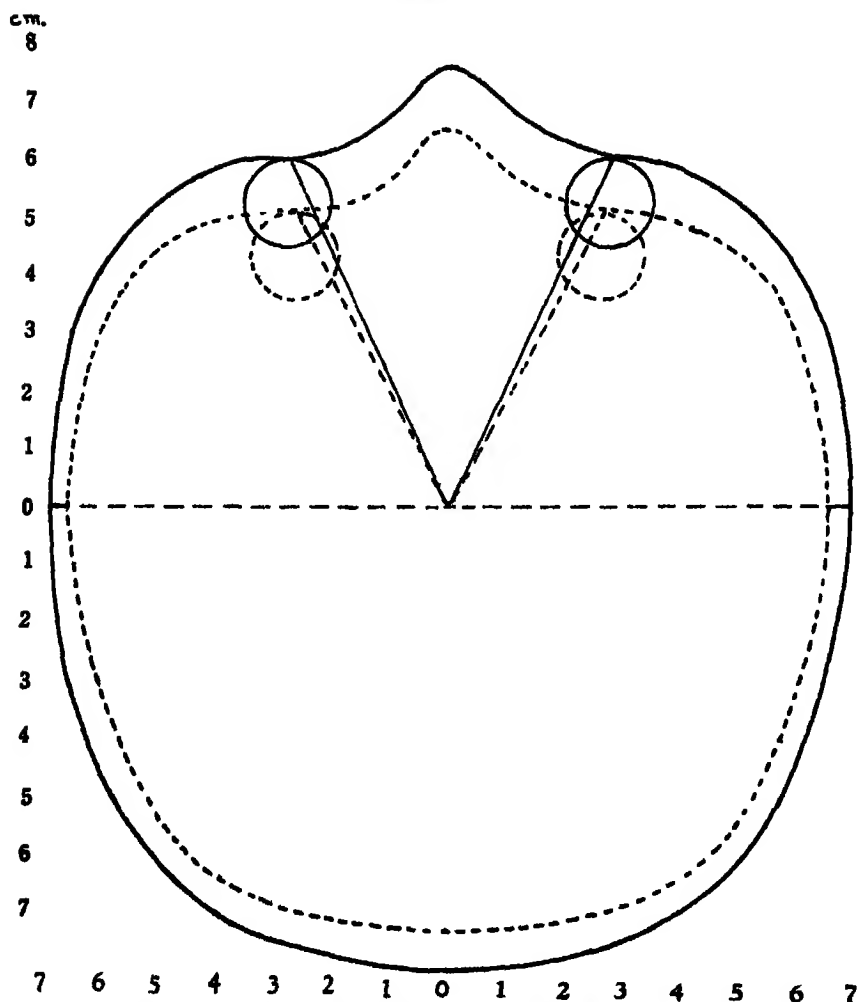
Outlines of horizontal sections of head of V R , a microcephalic boy at 10 6 and 19 3 years respectively, showing rapid growth in length, slow growth in width. Reduced to 85 per cent of natural size

PLATE V



Outlines and horizontal sections of head of a dwarf girl, A W , at 12.4 and 20.0 years respectively, showing slow growth Reduced to 79 per cent of natural size

PLATE VI



Constructive outlines of horizontal sections of head of a baby boy, J S , at the age of 145 days -----, and 711 days (1 yr 11½ mos) ———. The distance from occiput to tragon is taken as constant and the distance from intertragal line to orbit is directly measured or computed from adjacent measurements. Interpupillary distance is measured (half the sum of distances between outer and inner angles of palpebral slits). The tangent of ¼ interpupillary distance divided by tragon to orbit is found and doubled. The angle at 145 days is 55° 4', at 711 days 48° 3'. From a series of measurements at intermediate ages assurance is gained that the difference is not due merely to technical accidents. Reduced to 80 per cent of natural size.

PALEOCENE FAUNAS OF THE POLECAT BENCH FORMATION, PARK COUNTY, WYOMING

Part I

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ABSTRACT

Geological time boundaries can be established more decisively in regions with the least loss of the lithic and biologic records. Thus the temporal delimitations became more definitive as they are more arbitrary. The 3500-foot-thick Paleocene section of Polecat Bench in the Bighorn Basin near Powell, Wyoming, contains approximate equivalents of the major Paleocene mammalian faunas known elsewhere and serves as a useful comparative or reference section in the attempt to define and delimit Paleocene time in interior North America, in accord with the less complete European standard.

In the Polecat Bench formation, between strata of Lance (Cretaceous) age and the Eocene Gray Bull beds, four faunas have been distinguished. The lowest approximates that of the Puerco of New Mexico in time, and occurs in the base of the Mantua sandstone lentil which overlies dinosaur bearing beds. A second fauna consisting of at least five orders, thirteen families and twenty three genera, comes from the Rock Bench Quarry beds and equates with the Torrejon (New Mexico) and the Lebo (Montana) faunas. The third assemblage from the Silver Coulee beds includes nine orders, seventeen families, twenty-eight genera of mammals and correlates with parts of the Tiffany (Colorado), the Paakapoo (Alberta) and the Melville (Montana). The uppermost fauna occurs in the Clark Fork beds and has many mammalian forms in common with the overlying Eocene strata. Both the lower and the upper contacts of the Paleocene strata are discordant in some localities, conformable in others. Five new genera and eight new species of multituberculates are described.

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I INTRODUCTION

A PROGRAM of field work upon the Paleocene faunas and sediments of the Polecat Bench section north of Powell in northeastern Park County, Wyoming, was begun in 1927 and some of the preliminary observations were presented three years later (Jepsen 1930b). Within the Paleocene "Fort Union" formation (above the Cretaceous "Lance" and below the Eocene "Wasatch") four faunal zones were distinguished. The lowest yields genera in common with the Puerco of New Mexico, the second correlates with the Torrejon of New Mexico and the Lebo of Montana, and the third compares in age with Granger's Tiffany of Colorado and its approximate equivalents, the Bear Creek and the Melville in Montana and part of the Paskapoo in Alberta. The strata at the top of the Paleocene section, yielding the fourth fauna, were named "Clark Fork beds" by Granger (1914).

Since 1930 field parties from various institutions have worked in the area each summer, studying different aspects of the unique geologic and paleontologic histories of the region. Polecat Bench has been mapped topographically (Mackin, 1937, Pl. 4), and areally (Stow, 1938, Pl. 2), the sediments have been analyzed for significant heavy minerals (Stow, 1938, and Donald, unpublished thesis, 1936), and several hundred vertebrate fossils have been collected. This report is to describe some of the new fossils, to sug-

gest stratigraphic conclusions derivable from them, and to discuss the lithic and temporal terms that have been applied in the area.

The present project has been supported by a cooperative grant from the Geological Society of America and the American Philosophical Society and by the Princeton Scott Fund

Officers of the American Museum of Natural History, the United States National Museum, the Carnegie Museum, and the University of California Museum of Paleontology have lent material for study Dr W T. Thom and Dr M. H. Stow have been profitably consulted about numerous field problems Mr R. Bruce Horsfall has prepared the plates and many of the text figures, records which will endure through changing interpretations of the objects they represent. Several Princeton students, among them B. M. Hazen, P. H. Holsapple, N. H. Donald, T. G. Gallatin, and J. T. Kinney, have participated in the field program and written theses upon the lithology, stratigraphy, structure, physiography and mineralogy of the region Robert Layton, William Tweedie, and Albert Silberling did much of the delicate excavating and blasting in the quarries To these colleagues as well as to the many residents of Powell who have so generously helped us, I am indebted Princeton University has supplied facilities for the faunal and mineralogical studies and granted a sabbatical term to pursue them

II STRATIGRAPHY

THE POLECAT BENCH SECTION

A few of the errors and inconsistencies in the earlier descriptions (Jepsen 1930b) of the four faunal horizons in the Polecat Bench section can be corrected herein, but the general stratigraphic conclusions have not been fundamentally altered by the recent field work Each problem of stratigraphy that has approached solution has been replaced by others For this reason, and because taxonomic work upon the faunas has not been concluded, the final stratigraphic and facies summaries will be deferred until all of the fossils have been studied as individual specimens and as elements of the faunas. It is expedient to state now, however, why some of the terms such as "Fort Union" which were formerly applied to the rocks and faunal zones of the Polecat Bench section have proved unsatisfactory and are open to still further misinterpretations, and why a new or revised terminology is proposed.

Among the unique conditions in the region which have favored its use as a convenient reference section of the Paleocene, the following are of especial significance. (1) The strata contain the approximate equivalents of the major faunas known elsewhere from the Paleocene of North America and indicate their correct order by stratigraphic position, thus offering an unparalleled opportunity to study the evolutionary changes within taxonomic groups in a single area. (2) Several species are represented by large suites of specimens, fifty or more individuals. (3) The region is about midway between the identified north-south (Alberta to New Mexico) limits of exposures of Paleocene terrestrial rocks, a fact which may be notable in studies of climates and faunal distributions. (4) The lowest (and earliest) fauna in the section, correlating approximately with the Puerco, is closer than the latter to the areas where late Cretaceous mammals have been found. (5) The lower and upper boundaries of the Paleocene strata are sharply defined by faunal changes although, (6) the Paleocene rocks are not separated (except locally) from contiguous strata by nonconformities or disconformities, thus indicating essentially continuous deposition from Cretaceous to Eocene times. This latter condition becomes significant in suggestions for establishing the discretionary boundaries of the Paleocene epoch in interior North America.

There is a widespread belief that, because diastrophic movements affect the facies of rocks and faunas and floras, time boundaries even in sequences of terrestrial sediments can and should be determined ultimately by the lithic records of diastrophic movements such as nonconformities and disconformities. Thus, for convenience, a boundary is "set" where a part of the lithologic and biologic record is most obviously missing. The greatest lithologic or biologic "gaps" are sometimes considered to be the best locations for temporal delimitations. Many geologists formerly believed that the Laramide revolution in the Rocky Mountain area was a relatively short single diastrophism of profound lithic and biologic results. With this premise they correlated unconformities from place to place in the rocks which were deposited near the end of the time that dinosaurs existed. Naturally this made the supposedly single diastrophism appear to be widespread and highly significant as marking one of those much sought but elusive brief times of great and widespread crustal unrest in earth history. In the plains region the Laramide revolution, as it now appears, began

long before the deposition of what are generally called Late Cretaceous sediments and continued well into the Tertiary. Indeed, there may be difficulty, eventually, in naming a time that the Laramide revolution ceased. The confusions in reading the lithologic record of the revolution are especially intricate because from Late Cretaceous time to the present the plains sediments are mainly terrestrial and therefore highly responsive to physiographic and climatic changes

Paradoxically, but actually, the time boundaries become more accurate and definitive as they are more arbitrary. Delimitations are often obvious between lithic units which are separated by great unconformities or between temporal units which have no common biological elements, but such boundaries are of little use in other regions where the elapsed time is represented by rocks and fossils. In a complete record of sedimentation or evolution there would be no very significant lithic or biological interruptions and hence no natural bases for boundaries, but such records are unknown in nature, being only approximated for relatively short time spans. For greatest utility the hypothetical time boundaries should be definable, intelligible, and conveniently applicable to field conditions in mapping the rocks and in calculating correlations.

The Polecat Bench section is not "complete" in the sense that it certainly preserves a record of continuous deposition or of plant and animal evolution. Nor are the Paleocene rocks as thick here as they are in other areas. However, no other known sections represent such a complete span of Paleocene time, as it is conveniently defined.

The geographical distribution of the known Paleocene localities which have yielded large collections of fossil vertebrates is remarkably linear. Nearly all of these major faunas have come from sites that are on or close to a straight line drawn from the center of the Puerco-Torrejon (Nacimiento) area in New Mexico northward and somewhat westward to the center of the fossil localities of the Paskapoo in Alberta. Polecat Bench lies near the middle of this line which is roughly parallel to the Rocky Mountain structural axis and, if continued farther northward, passes down the Mackenzie River valley to the Arctic ocean. The Dragon fauna (Gazin, 1939) of central Utah occurs farther from the line than any other collection of Paleocene vertebrates sufficiently large to be of considerable value in correlation, but future discoveries may still

further extend the known east-west extent of Paleocene mammalian faunas

Mackin (1937, pp 839-841) in his description of some of the physiographic features of Polecat Bench states that it is "a high-level mesa, varying from 3 to 5 miles in breadth and extending for approximately 20 miles in a northeasterly direction on the divide between the Shoshone and the Clark Fork rivers. The bench is completely isolated from the flanks of the ranges that encircle the Big Horn Basin, and stands 200 to 500 feet above the surrounding lowlands, as the single dominating element in the topography of the north-central part of the basin. The surface of the bench consists essentially of what was once a 3-mile wide longitudinal valley, still limited locally by somewhat higher land along its lateral margins. This high-standing valley segment is in line with prominent gaps in the mountain barriers on the two sides of the Big Horn basin. The gap on the southwest is the open upper valley below which the Shoshone River has cut its present gorge through Rattlesnake Mountain. On the northeast, Pryor Gap, a 10-mile long valley, cut directly through the Pryor Mountains, is not now occupied by any through-flowing stream. The abandoned Polecat Valley, in the center of the basin, between, and in alignment with, the two gaps, slopes northeastward at an average rate of 25 feet per mile

"The Polecat Valley floor is everywhere underlain by a layer of coarse river gravel, generally about 16 feet in thickness, which rests on a surface that bevel inclined beds of Cretaceous, Paleocene, and Eocene shale and sandstone. The Polecat Bench surface thus preserves, almost unchanged, a part of the topography of the basin as it existed when the Shosone flowed northeast down the longitudinal Polecat Valley, in a course markedly different from that of the present stream."

Badlands have developed along the sides of the southwestern part of Polecat Bench around and beyond the margins of the erosion-resistant gravels, and these peripheral slopes, almost free of vegetation, have been prospected for fossils over several hundred square miles. Throughout most of the Polecat Bench area however, the rocks are almost barren of fossils, and the few surface fragments which have been found are weathered and disarticulated. Much the greater part of the collection of vertebrate fossils has come from three quarries, one in each of the three lower faunal

zones. In the original descriptions of these sites (Jepsen, 1930b) they were referred to as "Puerco," "Torrejon," and "Tiffany-Bear Creek" to emphasize their correlations with other Paleocene formations or localities. Later, Simpson (1936a) called them "Fort Union Puerco," "Fort Union Torrejon," and "Fort Union Tiffany." These designations of the faunas or the rocks in which they occur are admittedly both clumsy and misleading. It is unlikely that any one of the Polecat Bench faunas is exactly coeval with that from the type locality of the named equivalent. In only one case, from Polecat Bench to Bear Creek, Montana, is there any possibility of correlation by continuously tracing the strata, and even in this instance the continuity is disturbed by intervening structures. The type localities of the Puerco and the Torrejon in New Mexico and the Tiffany in Colorado are several hundred miles from Polecat Bench.

NOMENCLATURE OF "FORT UNION" LITHIC AND TEMPORAL UNITS

Methods of designating late Cretaceous and early Tertiary rock units, faunas, horizons, zones and other of the geologic and biologic elements in the northwest plains region are numerous, and have become incredibly involved, even allowing for differences of opinion that might be expected when the various areas and subjects are studied from different viewpoints by several investigators. Each writer must now propose his own particular system of nomenclature based upon the evidences he most values. Current misusages or inconsistencies in the use of the broader terms such as "Fort Union" and "Wasatch" have been indicated, but no entirely satisfactory solutions have been proposed (Jepsen 1930b, pp 469-472, Nace 1936, pp 66-87, 120-146, Simpson 1937a, pp 20-21). Apparently, until many more data concerning the plants, animals, and local and regional diastrophisms have been collected and correlated, there will not develop any standard system of reference to or interpretation of the rocks or their fossils.

Both terms "Fort Union" and "Wasatch" have by now gained almost as much temporal as areal significance, despite the fact that the names were originally applied to rocks. The type Fort Union (Meek and Hayden, 1861, p 443) is in western North Dakota and vicinity, but the name has been extended to include strata in numerous other regions, with but slight reference to the type area. The Wasatch group of southwest Wyoming and adjacent

Utah was named after Wasatch Station, Summit Co., Utah, and described briefly by Hayden in 1869 (p. 191 of the 1873 edition). Subsequently the name has been used for "groups" and "formations" of variegated beds in Montana, North Dakota, and New Mexico as well as numerous areas in Wyoming and Utah in addition to the type region.

At the present time, after about three quarters of a century of use of "Fort Union" and "Wasatch," it is unusual to find, among stratigraphers and paleontologists, two similar conceptions of the correct applications of the terms. Until standard definitions of the terms are generally accepted, further extensions of them will result in further confusion.

Wilmarth (1938, p. 761) gives formational rank to the Fort Union, classifies the Lebo and Tongue River as members of it, and states that the Fort Union formation is of Eocene age and that it underlies the Wasatch formation and overlies the Lance formation. There are, however, many diversified definitions of the Lance and the Wasatch with no very general agreement as to what constitutes the top of the former or the base of the latter. The type area of the Lebo member (Stone and Calvert, 1910, p. 746) is along Lebo Creek in the Crazy Mountain region, south central Montana, and that of the Tongue River member (Taff, 1909) is in the valley of Tongue River, southwest of Miles City, southeastern Montana.

Simpson (1937a, p. 21) follows the conception that the writer used in 1930 (b, pp. 496-497¹) as a workable definition of "Fort Union." According to this usage the composite Fort Union includes all strata in the northwest plains of ages from Puerco to Clark Fork, inclusive, that is, the entire Paleocene series. As Simpson says, "This usage is very broad . . . but a more restricted usage would be very difficult to frame or to maintain. It at least has the virtue of being on the whole a natural subdivision of the Tertiary and of including practically all the beds that have ever been called Fort Union. . . . For more precise work it will in any case be necessary to define and use more local names for particular formations and members . . ."

It has been suggested that the Polecat Bench section of the Paleocene would make an excellent standard section for a redefined "Fort Union" formation, since it can be defined in terms of rocks,

¹ Misinterpreted by Wilmarth (1938, p. 861) as including Granger's Gray Bull as a member of the Fort Union.

fossils, and minerals, and delimited both at its base and top. Such a transplantation of the "Fort Union," however, now seems inadvisable because the type Fort Union may be defined satisfactorily, by its rocks and fossils, in the future.

Stow (1938, Pl. 2) has mapped late Cretaceous and early Tertiary rock units in the Polecat Bench region, having traced some of them from outcrops near Columbus and Bridger, Montana, where earlier units were recognized by Knappen and Moulton (1931). In his stratigraphic nomenclature he listed the Hell Creek and the Tullock as members of the Lance formation ("other terminology and usages notwithstanding"), and the Lebo and Tongue River as members of the Fort Union formation, following Thom and Dobbins (1924). His Lebo includes both the "Puerco" and the "Torrejon" quarries on the east side of Polecat Bench, although he states that elsewhere "the top of the Tullock . . . may either be equivalent to the sandstone lens yielding the Puerco fauna, or may directly underlie it." On his maps the Bear Creek, Montana, fossil locality and the "Tiffany-Bear Creek" quarry on the west side of Polecat Bench are within the "Tongue River," as are the beds yielding the Clark Fork fauna. Stow states "In general the Lebo becomes more sandy as the top is approached. Apparently there is no sharp lithologic change between the Lebo and the Tongue River members of the Fort Union. For mapping purposes the boundary between the Lebo and the overlying Tongue River has been placed where the beds become predominately sandstone" (p. 736). Stow's terminology follows

Era	Period	Epoch	Formation	Member
Cenozoic	Tertiary	{ Eocene Paleocene	Wasatch Fort Union	{ Tongue River Lebo
Mesozoic	Cretaceous	Upper Cretaceous	Lance	{ Tullock Hell Creek

No one of Stow's four members in the Bighorn Basin has actually been traced by continuous exposures from its type locality.

In his monograph on the Fort Union of the Crazy Mountain area in Montana, Simpson (1937a) reviewed some of the terminologies applied to the rock units of late Cretaceous and early Tertiary age in the northwest plains area. The Crazy Mountain section

of the Fort Union includes three lithologic units which Mr Albert Silberling (Stanton 1909, p. 263) called Fort Union No 1, Fort Union No. 2, and Fort Union No 3 The lower two divisions, 1 and 2, constitute the type Lebo of Stone and Calvert (1910), and Simpson named No. 3 the Melville formation. The fauna from the latter correlates, at least in part, with the Tiffany. In the type upper Lebo (Fort Union No. 2) are located the Gidley and the Silberling quarries which have produced hundreds of specimens that prove the rocks to be close to the type Torrejon in age. The lower Lebo (Fort Union No 1), both from its fossils and its stratigraphic aspects, appears to be similar to the upper Lebo in age and facies

Below the Crazy Mountain Fort Union, a section of about five or six hundred feet of strata previously considered as Cretaceous and referred to the Lance has been named the Bear formation by Simpson. The Bear formation is not decisively dated, although invertebrates from near its top indicate Paleocene rather than Cretaceous age Mr Silberling reported fragmentary and isolated dinosaur bones in the lower eighty feet of the Bear, and Simpson suggests that these may have been redeposited from the underlying Hell Creek.

Two possible conclusions about the age of the Bear are listed by Simpson (1) that it is Cretaceous and that, therefore, Puerco time is not represented by rocks in the Crazy Mountain section, (2) that part or all of it belongs in the early Paleocene In regard to possible correlations of the unfossiliferous Bear beds, he states, "They may perhaps be equivalent, approximately, to the Tullock, but this would be an assumption that might result in serious misapprehension. They are far removed from and discontinuous with the type Tullock, into which it will never be possible to trace them, and their lithologic character is not the same It is improbable that they are exactly equivalent to the Tullock, and even if this should prove to be the case it would seem warranted and necessary to retain for them a local name. At present correlation with the Tullock would be only a guess, which might well prove to be incorrect."

Silberling's statement about seeing dinosaur bones in the lower part of the Bear may be compared with the report of Rogers and Lee (1923, p 34) that a bone fragment found on the surface of a steep slope about 50 feet above the base of the Tullock was identi-

fied by C. W. Gilmore as a section of a supraorbital horn core of a ceratopsian. A few fragments of dinosaur bones in the Bear and in the Tullock cannot be cited as proof of correlation nor even as satisfactory evidence of the age of either unit. Fragments of dinosaur bones were undoubtedly inherited by Tertiary deposits from Cretaceous sources, just as they are now in many places being reburied in recent sediments. In the Polecat Bench "Torrejon" quarry several teeth referred to *Triceratops* have been found, but so have water-worn shark teeth, and in the base of the "Puerco sandstone" at one locality a number of water-worn fragments of large bones were discovered. Even if undisturbed and articulated dinosaur skeletons or dinosaur tracks, or any other strong proof of the actual living presence of dinosaurs, were found above the remains of early Tertiary mammals, there would be no need from these facts alone to re-locate formational or generally accepted time boundaries. A concept that dinosaurs persisted in abundance through an arbitrary temporal unit and then suddenly became extinct, and that their demise everywhere coincided with the top of a widespread lithic unit, is contrary to modern interpretations. The extinction no longer even appears to have been sudden. In the northern Rocky Mountain front region, the erosional and depositional effects of late Cretaceous—early Tertiary diastrophism were probably rhythmic and, in general, progressive from the west toward the east. Thus even a continuously deposited lithic unit might be older in its western than in its eastern limits, and transgressed by faunal and floral horizons or zones. Facies differences in the Paleocene record are much greater from west to east than from north to south, contrary to what might be expected from a consideration of the present day geographic positions of faunal and climatic zones. The Paleocene mammal horizons are fairly satisfactorily correlated for about one thousand miles along the north-south line of their known extent, from Alberta to Colorado and New Mexico, but the west-east correlations are at present less secure for shorter distances. The climatic or physiographic implications of these facts are incalculable. The lack of satisfactory west-east bases for correlation may be due in part, but not entirely, to failures in the search for fossils.

The attempts to fit distinguishable life zones or faunas to formational or member units, and to extend these coincident units from one isolated area to another, are in many cases futile.

Even some contiguous strata which are well accepted as constituting distinct "formations," such as the Puerco and the Torrejon, are distinguished from each other only by their fossils, and are conveniently mapped together, in this case as the Nacimiento group (Reeside 1924, Pl 1, p. 35). Indeed, not one of the important North American faunas which vertebrate paleontologists include within the Paleocene, and which mark significant changes of time or facies, is stratigraphically coextensive with an easily recognizable single lithic unit of sufficient geographical extent to have been called a formation, with the possible exception of the Lebo. This statement is true of the following Puerco, Torrejon, Tiffany, Clark Fork, Bear Creek, Plateau Valley, Dragon, Melville, Paskapoo, and the "Puerco," the "Torrejon," and the "Tiffany—Bear Creek" of Polecat Bench. Each of these is either restricted to part of a formation which is generally designated by another name, or occurs in a formation whose limits are not satisfactorily determined, or is found with one or more other faunas within a single formation.

For mapping purposes alone in small areas, or for studies of the evolutionary changes within biological groups from site to site, the failure of faunal and lithic boundaries to coincide is not especially significant, but the fallacious assumption that they commonly do or should coincide leads to unwarranted conclusions in broader studies.

The strata of the type Lebo (Fort Union nos. 1 and 2) in the Crazy Mountain area have not been and probably cannot be traced along outcrops to other fields to the east because of structural and erosional interruptions as well as lithologic changes. Nor are other named rock units, such as the Lance, the Fort Union, the Tullock, the Tongue River, and the Sentinel Butte, traceable from their typical exposures to the type Lebo. Thus, according to usage cited above, the Fort Union formation embraces two members, the Lebo and the Tongue River, whose type areas are geographically separated from each other as well as from that of the formation of which they are members.

A complete review of the further vagaries of "Fort Union" terminology is not necessary here. Nace (1936, pp. 66-87) gives an excellent review of the literature. The problems of correlation have been approached, in general, by two different methods. Stratigraphers have been assigned or have selected areas to be mapped for coal reports or other economic projects in the north-

west plains area They have of necessity extended the mapping of "mappable rock units" from field to field, sometimes selecting arbitrary stratigraphic limits or even dimensions to define formations in the places where fossils are rare or absent, and have traced single continuous coal beds or groups of interfingering coal beds over wide areas. That the rock units or series traced and mapped by this system in a single area are continuous there can be no doubt, but it is commonly assumed that a widespread unit is everywhere of exactly the same age Justified differences of opinion result with attempts to correlate a single unit or even groups of units in one field with those in another when there is no possibility of actually "walking out the beds"

Paleontologists have been more interested in collecting specimens for correlation, for biological studies, and for museum exhibition than in mapping the rocks Because the "Fort Union" animal remains are usually concentrated in "spots" of limited geographical and columnar extent, and because these rare sites frequently fail to indicate complete incidence of a faunal range with a lithic unit, the paleontologists have given geographical names to "beds" and to "faunas" for locality identifications Gazin (1938, p 272) used the geographical name "Dragon" for Paleocene faunas within the North Horn formation, using the Puerco and Torrejon as precedents "that a geographic name is used to designate a fauna, if only to serve as a handle for paleontological use"

In attempts to use both the lithological and the paleontological methods of correlating and naming the latest Cretaceous and early Tertiary rocks of Polecat Bench, the type areas of the following named formations, members, beds, or faunas have been examined, Lance, Hell Creek, Tullock, Fort Union, Lebo, Tongue River, Puerco, Torrejon, Melville, Tiffany, Bear Creek, Plateau Valley, Gray Bull, Wasatch, and limited parts of the Paskapoo. Fossil mammals have been collected at several localities in the Paskapoo (Simpson, 1927b; Russell 1929, 1932) and probably represent more than one zone of Paleocene age At present it is impossible to recognize any unique lithic qualities of single strata or series of rocks in the Polecat Bench section that provide a satisfactory basis for correlation with typical exposures of any of the named formations or members, although there are many superficial or "gross" similarities. This inability forces reliance upon paleontology for correlations. Vertebrate fossils, however, are so rare in the rocks

of the earliest Tertiary, and are distributed so irregularly, that their major use at present in correlation is to indicate relative ages from site to site and but rarely to delimit rock units. This is true to a degree of the more abundant plants and invertebrates also.

Dorf (1940) has recently studied plants from the type areas of the Lance and the Fort Union, and has discovered that the former widely accepted belief in the essential unity of Lance and Fort Union species is erroneous, having been based upon mistaken identifications of formations in the field. He also cites paleobotanical evidence for the Paleocene age of the Tullock, thus grouping it with the Fort Union rather than the Lance.

Correlation by means of heavy minerals is another promising line of attack. The work of Stow (1938) and Donald (unpublished thesis) indicates that, at least in limited areas within a depositional basin, and perhaps more extensively, "heavy minerals can now be used as satisfactory 'second order' bases for determining the age of unfossiliferous outcrops" (Stow, p. 761). As might be anticipated, however, the heavy mineral zones do not coincide exactly with the faunal zones. Stow's "Lebo member," as stated above, includes the "Puerco sandstone" as well as higher sediments whose fossils correlate with those from the Torrejon of New Mexico and the type Lebo in Montana. His usage of formational and member names, however convenient it may appear, is open to serious, but perhaps pedantic, misconception. Whereas his "Lebo member" does include some beds which are patently similar in age to those of the type Lebo, which in turn correlate with the type Torrejon, it also includes a rock unit which contains Puerco genera that are entirely distinct from those of the Lebo and the Torrejon. Thus, the mere application of the term "Lebo" to these Wyoming sediments may, and probably will, lead to the erroneous assumption that the true Lebo spans Puerco-Torrejon time. The use of "Tongue River" for some of the sediments in the Polecat Bench section is also misleading.

To the suggestion that new names instead of "Fort Union" "Lebo," and "Tongue River" should be applied in the Polecat Bench section, there is the objection that each new name enlarges and complicates the nomenclature. This is true, but an enlarged nomenclature is sometimes less objectionable than the inconsistency and false accuracy in the usage of an oversimplified system of terms. It may be noted here that the naming of units was en-

tirely beside the main points of Stow's work on Polecat Bench, and that he did not intend to imply correlations with the type series of the Fort Union, the Lebo, and the Tongue River

The term "beds" in such geographical references as "Tiffany beds" (Granger 1917, p. 829) and "Clark Fork beds" (Granger 1914, p. 204) has considerable utility but little formal recognition, and its use has often been discouraged (Ashley, G. H., and others, 1933, p. 439, art. 16), or ignored "Faunal zones," as used by Americans, can be applied in the Polecat Bench section to indicate a few of the ranges of the different categories of mammals but cannot have any geographical connotations and is unwieldy, as explained below. The term finds its greatest use when defined by invertebrates where they are abundant, and the ranges of species, genera, and larger categories are well established. These specifications are not likely to be fulfilled in the near future by studies of the Polecat Bench fossils. Some of the European stratal and chronological terms (Arkell, 1933) might apply to the Polecat Bench rocks and faunal ranges, but they have not been generally accepted by American geologists.

In summarizing the attempts to delimit temporal and stratigraphic units in the northwest plains, it may be said that the evidences from lithologic series, heavy minerals, plants, invertebrates, and vertebrates have been construed in diverse and unintentionally contradictory or unintelligible ways. As a result a term like "Fort Union" has a different meaning to each interpreter and can be defined for general satisfaction only as a composite conception of rocks, fossils, and time. A plea to differentiate between stratal and temporal connotations is probably useless, the two are so thoroughly confused. "Lower Paleocene" as a temporal term is as logical as "lower August," and "early Fort Union" may be likened to "early floors" of a building.

POLECAT BENCH FORMATION

It is increasingly difficult to refer, in ways that are incapable of misinterpretation, to the aggregates of specimens that come from the Polecat Bench quarries and to the surrounding sediments at approximately the same levels. Since the terms, "Fort Union," "Puerco," "Torrejon," "Tiffany," "Lebo," and "Tongue River" are more confusing than informative when extended to the Polecat Bench strata, their use in the area should be abandoned and local names applied and defined.

Polecat Bench formation is proposed in place of "Fort Union" for the sediments in the vicinity of Polecat Bench, about 3500 feet thick, bounded below by the *Triceratops*-bearing beds and above by *Homogalax*-bearing beds. For convenience the beds containing *Triceratops* and other dinosaurs will be called "Lance?" herein and considered to be of Cretaceous age. The *Homogalax*-bearing strata will continue to be referred to as Gray Bull beds. It is beyond the scope of this work to attempt to solve the intricate difficulties which have grown from Granger's application and misspelling of "Gray Bull" (1914, p. 203) and from the subsequent use of the term, correctly spelled "Greybull," to a sandstone member of the "Cloverly" formation (See Wilmarth, 1938, p. 878). The differences in spelling should be recognized as a valid (although unintentional) distinction between the terms or one should be dropped. Both are entrenched in the literature. The United States Geological Survey does not recognize "Gray Bull beds" because it, in general, "does not apply geographic names to faunal zones" (Wilmarth, 1938, p. 861).

The Polecat Bench formation is composed of the rocks, at Polecat Bench, mapped and described as "Lebo" and "Tongue River" by Stow (1938) and as "Paleocene" and "Fort Union" by Jepsen (1930b). From its base to its top it includes the *Mantua lentil*, the *Rock Bench quarry beds*, the *Silver Coulee beds*, and the *Clark Fork beds*. The strata at the top of the "Lance?" and at the base of the Polecat Bench formation and the contact between the two have been described (Jepsen 1930b, pp. 476-484). No angular unconformity between the Cretaceous and the Paleocene sediments has been detected at Polecat Bench, although the fossils suggest a disconformity or hiatus. At the top of the Polecat Bench formation there may be locally angular unconformities between it and the overlying Gray Bull beds but in most places where the contact has been determined the units are conformable. Conglomerates and thick beds of coarse buff sandstones mark the base of the Gray Bull in several localities. Mineralogically the Polecat Bench formation may be distinguished from the Gray Bull beds by the abundant presence of green hornblende in the latter and its absence in the former. The formation is composed of terrestrial sediments, sandstones, clays, siltstones, and limestones. Cross-bedding and channel sandstones are commonly observed, and individual strata are rarely traceable for more than a few hundred yards.

The Polecat Bench formation represents the duration of the Paleocene Epoch, as now defined, from Mantua (Puerco) to Clark Fork times, inclusive. Faunally the Paleocene Epoch may be characterized, at present, by the absence of dinosaurs (except through redeposition) and the presence of Tæniodonta, Carnivora, and Condylarthra, to distinguish it from the Cretaceous, and by the absence of Perissodactyla, Artiodactyla, and Adapidae, to distinguish it from the Eocene. In the future some or all of these faunal distinctions may become invalid as the ranges of the groups are extended by discoveries, and the bases of definition will be smaller taxonomic categories. It is conceivable that instead of looking for a lithologic and biologic gap to mark the end of one arbitrary epoch and the beginning of the next, the search will be for a complete uninterrupted and highly fossiliferous rock series within which the boundary can be set arbitrarily and with which other sections can be compared both lithologically and faunally.

The Mantua Lentil

The massive sandstone about 130 feet thick, in the base of the Polecat Bench formation has been traced for several miles along its strike. It is mappable as a rock unit but lacks sufficient geographic extent to have formational rank. It contains a diagnostic vertebrate fauna and its type section in sec 31, T 57 N, R 98 W, has been described, delimited, and figured (Jepsen 1930b, pp 480-484, Pls II and III, Text Fig 3), under the name "Puerco sandstone." *Mantua lentil* is now proposed for it, the name being derived from the nearby Mantua coal mines and railroad siding. The name Mantua as herein applied has appeared in print without definition (Jepsen in Scott, 1937, pp. 99 and 230, Jepsen, 1938). The genera *Loxolophus*, *Eoconodon*, and *Oxyacodon* approximately correlate the Mantua with the type Puerco. Hazen (1937) has described a fossil earthworm (?) from the lentil. Mineralogically the Mantua lentil is not distinguishable from the immediately adjacent beds below or above.

Most of the mammalian fossils from the Mantua have been found within a few inches of its base. More than a hundred specimens including skulls and jaws have been removed from a quarry in the lowest part of the lentil. As described (Jepsen 1930b, pp 478-480) the Mantua is underlain by a thin coal bed below which dinosaur bones, teeth, and tracks are preserved. Numerous

dinosaur bones, *Triceratops* teeth, several jaws and parts of broken skulls were observed, but the dinosaur bones are less abundant toward the top of the "Lance?," and the highest level at which fresh unworn bones and teeth have been found is about six feet below the coal upon which the Mantua rests. At one locality several large reptile bones were found at the bottom of the Mantua, resting upon the coal. They are presumably dinosaur remains but have been so broken and waterworn as to be unidentifiable.

Stow (1938) mapped the Mantua lentil within the "Lebo."

Rock Bench Quarry Beds

The second, the "Torrejon," fauna occurs about two hundred feet above the base of the Polecat Bench formation and about seventy feet above the top of the Mantua lentil in sec 36, T. 57 N., R. 99 W., in a coarse buff sandstone which, as described and figured (Jepsen 1930b, pp 485-87, Pl I, Fig 1) varies from about 9 to 31 feet in thickness. A few fragmentary fossils that serve to extend this faunal level have been found at other nearby localities on Polecat Bench, but the principal collection, consisting of several hundred specimens, representing at least five orders, thirteen families, and twenty three genera, has been secured by quarrying the basal three inches of the sandstone in an area totaling about three hundred square feet. The sandstone yielding the fauna appears to be part of the shale and sandstone series which extends above the Mantua lentil for several hundred feet but is not sharply demarcated from higher sediments. It is impossible to trace the sandstone as a unit for much more than a mile, due to erosional interruptions, and it cannot be distinguished from many other similar sandstones merely by inspection. The fauna from the sandstone includes genera in common with the type Torrejon and the type Lebo, its closest correlatives. Characteristic genera are *Ptilodus*, *Elphidotarsius*, *Plesiolestes*, and the new genus, *Anconodon*. Heavy minerals do not indicate any sudden change in sedimentary facies or in the sources of the sediments at or near the quarry level. For ease of reference, the name *Rock Bench quarry beds* is proposed for the rocks yielding the *Ptilodus-Elphidotarsius-Plesiolestes-Anconodon* fauna. Rock Bench is a local name for this part of the Polecat Bench. The Rock Bench quarry beds are the strata which yield the distinctive Torrejon-Lebo fauna in the Polecat Bench formation. Future discoveries may extend the vertical limits of

the Rock Bench quarry beds down to the Mantua and up to the next named lithic unit but at present the name is intended to indicate only those strata known to contain the fauna. This application of a geographic name to such a limited zone is contrary to general practice (especially in marine stratigraphy) but is a convenient means of indicating the rocks of the level without limiting future upward or downward extensions of the faunal and lithic terms and without implying lithologic correlations that are unjustified or misleading.

The geographic name for rocks that yield a definitive but areally and vertically limited fauna is used in preference to the unwieldy generic names of the vertebrates that at present identify the zone, for several reasons. Actual ranges of the genera are unknown, even as the genera are at present defined. Redefinitions frequently expand or contract generic and specific ranges, the definitions of the taxonomic groups themselves being arbitrary. The selection of categories to define a zone depends upon whether the zone is to be compared with the same zone in other localities or to be contrasted with different, higher or lower, zones in the same or in other regions. Any or all of the taxonomic units used to define a zone locally may be absent from rocks at another locality as a result of facies differences, although the strata may be demonstrably contemporary, by the common presence of other fossils. This makes it advisable to correlate by means of as many groups as possible, not merely a selected few. Generic and specific names may be and frequently are reduced to synonymy,² or become invalid, processes that often go undetected except by specialists.

The Rock Bench quarry beds are composed chiefly of coarse sands and small pebbles and flattened clay balls. They appear to be a local channel deposit (Jepsen 1930b, Pl. I, Fig. 1) thus making the attempts to trace them laterally almost futile. The lateral continuity of the strata immediately above the Rock Bench quarry beds shows either that the channel was not very deep (10-50 feet) or that its banks were lowered before the deposition of the overlying strata.

² Wilmarth (1938, p. 861) follows Osborn (1929, pp. 57-59, 68 seq.) in calling the Gray Bull beds the "*Systemodon-Coryphodon-Botispus* zone." *Systemodon* Cope, 1881 has been reduced to synonymy with *Hyracotherium* Owen, 1840, and Hay in 1899 proposed *Homogalax* for the species, other than the genotype, which had been placed in *Systemodon*. *Botispus* Marsh, 1876 is a synonym of *Hyracotherium*. Thus the Gray Bull beds might be called the *Homogalax-Coryphodon-Hyracotherium* zone, but only the first of these genera is diagnostic of the zone, *Coryphodon* occurs lower and higher and *Hyracotherium* is also found in higher levels.

The name Rock Bench was applied, without definition, in 1937 (Jepsen in Scott, pp. 99 and 230) and 1938 (Jepsen)

Silver Coulee Beds

The fossiliferous localities in the Mantua lentil and in the Rock Bench quarry beds are on the east side of Polecat Bench but the third fauna comes chiefly from the west side, in Sand Coulee basin, where a quarry has produced several hundred specimens including a number of articulated skulls and jaws, vertebral columns, limbs, and complete skeletons. At least nine orders, seventeen families, and twenty eight genera of mammals are represented. Due to dip variations and to numerous small faults it is impossible to measure the exact distance of this quarry level above the base of the Polecat Bench formation but it is in the neighborhood of 2400 feet. The strata several hundred feet above and below the quarry, in sec 21, T 57 N., R 100 W., are sparingly fossiliferous and yield the same forms as those found in the quarry. The area is drained by a tributary of Big Sand Coulee which is locally called Silver Coulee and the name *Silver Coulee beds* is proposed for reference to the rocks yielding the fauna. The Silver Coulee beds as indicated above were included by Stow (1938, Pl. 2) in the "Tongue River." In contrast to the coarse buff sandstones of the Mantua lentil and the Rock Bench quarry beds, the Silver Coulee quarry matrix is a fine grained gray green sandstone. The fossils are well preserved and show but little evidence of post-mortem transportation.

Faunally the Silver Coulee beds may be called the *Microcosmodon-Phenacodaptes-Litolestes* zone. Heavy minerals show few distinctive associations for this zone although Stow (1938, p 753) reports that the "Tongue River" which includes the Silver Coulee beds as well as the Clark Fork beds, "can be identified by the common occurrence of red garnet, the presence of kyanite, and the absence of hornblende."

Along the southeast side of Polecat Bench, although no such highly fossiliferous level as that at the Silver Coulee quarry has been found, the zone has been identified by its fossils. Its lowest reaches roughly correspond with the base of Stow's "Tongue River," about 1200 feet above the base of the Polecat Bench formation and 1000 feet above the Rock Bench quarry beds. The Silver Coulee beds contain more sandstone than the underlying beds and weather to darker color. Some of the sandstones, like that of the Rock Bench quarry beds appear to be channel fills.

Compass readings on several show a general southwest-northeast direction for their greatest linear extent but a few others are nearly at right angles to them

The Silver Coulee beds are about 1800 feet thick on the southeast side of Polecat Bench, and their distinction from the overlying Clark Fork beds is chiefly faunal. Silver Coulee beds were undefined when the name was first applied in print (Jepsen 1937b, 1938)

Clark Fork Beds

Above the Silver Coulee quarry the sediments are relatively unfossiliferous for about 500 or 600 feet. The Clark Fork fauna appears in the uppermost 500 feet of the Polecat Bench formation, and seems to be increasingly abundant toward the top of the Clark Fork beds. Fossils have not been found localized in small "pockets" or areas of great concentration that would justify quarrying in the Clark Fork beds, as they have in the lower three zones. However, as in most localities of the American terrestrial Tertiary deposits fossils are more often found in some areas and levels than in others. Distinctive forms in the Clark Fork fauna are *Dipsalodon* and *Probathyopsis praeursor*.

Sinclair and Granger (1912, p. 60) provisionally referred to these strata as the "Ralston beds or Ralston formation" but upon discovering that the name was preoccupied Granger (1914, p. 204) substituted "Clark Fork beds". Sinclair and Granger described locally angular unconformities between the Clark Fork and the overlying Eocene strata but in most areas the "contact" is transitional with no evidence of interruption of sedimentation. Even the widely used criterion of red color banding for distinguishing the Eocene from the underlying beds is invalid because red strata occur well down in the Clark Fork beds. An arbitrary Paleocene-Eocene boundary is however, well marked by a faunal change, the first appearance of *Hyracotherium*, *Homogalax*, *Diacodexis*, *Hyoposodus*, and other Eocene genera. The earliest appearance of *Coryphodon* was formerly believed to coincide with that of these genera, and to be a good indication of "Wasatch" time (Wegemann, 1917),^{*} but the genus is now known from the earlier Clark Fork (Simpson, 1937d, pp. 3, 23, Jepsen, 1930b, p. 493). In the Polecat Bench area there is no very obvious lithologic change coinciding with the lowest levels at which the Paleocene-Eocene faunal change occurs,

^{*} Wegemann's age assignment of the strata under discussion was probably correct, based upon other genera.

although coarse yellow sandstones with interbedded conglomerates are more numerous in the base of the Gray Bull than in the top of the Clark Fork. The differences in the faunas of the late Paleocene Clark Fork and the early Eocene Gray Bull may be due to migrations, an undetected hiatus, or the fortuities of fossil collecting. In addition to the faunal differences between the Clark Fork and the Gray Bull, Stow (1938) and Donald (1936) found that the presence of green hornblende in the Gray Bull and its absence from the Clark Fork served as a useful local distinction between the two. A diastrophic interpretation of this is expressed by Stow (p 757), "The sudden appearance and high persistence of hornblende in the Wasatch denotes a new source of sediment, rather suddenly made available "

In tabulated form the foregoing discussion may be summarized.

Epochs	Formations	Local zone names	Distance above base of Polecat Bench formation *	Diagnostic fossils	Correlatives
Eocene		Gray Bull beds		<i>Homogalax</i>	Indian Meadows
Paleocene	Polecat Bench	Clark Fork beds	3000-3500	<i>Dysalodon</i> <i>Probathyopsis</i> <i>precursor</i>	
		Silver Coules beds	1200-3000	<i>Microcosmodon</i> <i>Phenacodaptes</i> <i>Lutolestes</i>	Bear Creek Tiffany Melville
		Rock Bench Quarry beds	200-240	<i>Plesiolestes</i> <i>Elphidotarsius</i> <i>Ptilodus</i> <i>Anconodon</i>	Lobo Torrejon
		Mantua lentil	0-130	<i>Mesodma</i> <i>Loxolophus</i> <i>Eoconodon</i> <i>Oxyacodon</i>	Dragon— Puerco
Late Cretaceous	"Lance?"			<i>Triceratops</i>	

* These dimensions are given here and elsewhere in round figures, because the sections are measurable in only a few places and because, like most terrestrial rocks, the strata are variable in thickness

PALEOCENE FAUNAL DATES AND STRATIGRAPHIC POSITIONS

Through usage begun before some of the northern plains Paleocene sediments and faunal sequences were studied, the Torrejon fauna is frequently said to be of "middle" Paleocene age, in distinction from the "lower" Paleocene Puerco and the "upper" Paleocene Tiffany and Clark Fork. In terms of sedimentary thicknesses this practice may be misleading, especially in view of the above cited concept that the Paleocene epoch is coextensive with the deposition of a composite Puerco to Clark Fork "Fort Union." In the incomplete Crazy Mountain section the approximate minimum thickness of the "Fort Union" is 4350 feet, distributed in units as follows, lower Lebo ("Fort Union No. 1") 500 feet, upper Lebo ("Fort Union No. 2") 850 feet, Melville ("Fort Union No. 3") 3000 feet. The top of the Melville has not been determined and although Simpson (1937a, p. 25) proposes the name "to include at least the lower 3000 feet of the No. 3 beds," he also states (p. 24) that the "No. 3 beds are a great mass at least 4000 feet thick and possibly as much as 6000." The Lebo fauna of Torrejon age thus occurs well within the lower one third of the minimum thickness of "Fort Union" group of Paleocene sediments, although the Lebo fossils are found in greatest abundance in the upper 150 feet of the "formation." Not only are strata equivalent to the uppermost "Fort Union," the Clark Fork beds, missing from or unidentified in the section in this calculation but Simpson believes that the Melville may be somewhat older (lower in a complete composite section) than the Silver Coulee beds which lie below the Clark Fork. If the undated Bear (600 feet thick) is included at the base of the Crazy Mountain section, although it is not part of the "Fort Union," the minimum thickness of the Paleocene sediments is 4950 feet and the Lebo fauna occurs below 1950 feet, or within the lower two fifths of the section. If the suggested 4000 or 6000 foot thickness for the Melville be considered, instead of 3000 feet, the Lebo is comparatively lower in the section.

As defined, the 3500-foot thick Polecat Bench formation was deposited during Paleocene time, its rocks and fossils representing the duration of the epoch. Undoubtedly the rate of sedimentation was not uniform, but its irregularities cannot be calculated. The Rock Bench fauna, correlative of the Torrejon and the Lebo, has not been found above a level about 240 feet above the base of the Polecat Bench formation, within the lower one fourteenth of the

series The entire "Lebo" as determined and measured by Stow in the vicinity of Polecat Bench includes only 1200 feet or about one third of the Polecat Bench formation, and there is no unequivocal evidence that the Rock Bench fauna extends upward to that level

In the type locality of the New Mexican Torrejon various estimates have been made of the irregular thicknesses of the Puerco and the Torrejon formations but the faunal levels in the two are separated by barren beds which may be assigned to either formation. Matthew (1937, p. 3) gives an average of 100 to 300 feet for the Puerco and 100 to 600 feet for the Torrejon Nace (1936 pp 60-61) reviews the various estimates of other authors The distance of the Tiffany beds above the Torrejon formation has not been determined, and many writers refer to the Tiffany as a lower zone in the "Wasatch." There are therefore no available figures to compute, in dimensions, the stratigraphic position of the type Torrejon in the Paleocene series

It would be absurd to believe that aggregate thicknesses of sediments from site to site or even in a single locality have much value in computing or comparing the ages of Paleocene rocks, because terrestrial sediments are notoriously variable in thickness However, due to the confusion of stratal and temporal terms, to the widespread application of "Fort Union," and to the practice of referring the Torrejon and Lebo faunas to "middle" Paleocene time, there arises an anomalous and clumsy situation Although Paleocene and "Fort Union" times are defined to be coextensive, the middle or medial Paleocene Lebo fauna is in the lower "Fort Union." Likewise, the use of "late" or "upper" to designate the temporal or stratigraphic position of the Melville is misleading in a trifold division of the Paleocene or the "Fort Union" Expressed in minimum thicknesses, the Melville occupies the upper 3000 of the 4350 feet or nearly three fourths of the Crazy Mountain "Fort Union," although the lower part of the Melville has not yielded many fossils and its age relationships are not entirely clear. ♣

These are additional reasons for the preference to discontinue the use of "Fort Union" in the Crazy Mountain and the Polecat Bench areas. In the former region the names "Lebo" and "Melville" have been substituted and are adequately defined for present references, and in the latter the "Polecat Bench formation" servicably replaces "Fort Union." The Rock Bench fauna, for the

sake of convenience, may continue to be referred to middle Paleocene time although it occurs near the base of the Polecat Bench formation. This usage, however, will not imply unwarranted commitments about the age of any part of the elusive type Fort Union.

One of the greatest faunal gaps in Tertiary history lies between the Puerco and the Torrejon, between the Mantua and the Rock Bench, despite the apparent continuity of sedimentation during this time in New Mexico and in Wyoming. It is bridged in part by the recently discovered Dragon faunas but stratigraphic observations about the latter are not yet available. The biologic gap between the stratigraphically close Mantua and Rock Bench faunas may be as great as that between the Puerco and the Torrejon, or even greater, but the fact that no stratigraphic break is recognized either in New Mexico or in Wyoming is surely significant. It apparently increases the probability that some of the Torrejonian forms which have no possible ancestors among the Puerco or Mantua assemblages arrived by migration and did not evolve in either region. Further evidence on this problem will be supplied by analyses and comparisons of Mantua and Rock Bench and Dragon faunas.

The physical evidences of a hiatus in terrestrial sediments, except where there is actual angularity, may be extremely difficult to detect. The methods which have been successful in discerning gaps in conformable marine sequences are largely inapplicable to the study of terrestrial strata.

ADDITIONAL "FORT UNION" CORRELATIONS SUGGESTED BY VERTEBRATES

A few scattered discoveries of identifiable and diagnostic vertebrate fossils have been reported from sediments of "Fort Union" and Paleocene age. As suggested, however, these serve only to indicate possible site correlations and do not, in any case, establish satisfactory correlations of complete lithic units.

Stanton (1909, p. 268) in dating some formations near Sheridan, Wyoming, quotes Gidley's identification of a specimen from strata interbedded with the Kingsbury conglomerate, about 600 feet above its base, as "closely allied to *Tricenties*, a genus found in the Torrejon of New Mexico." The specimen, U.S.N.M. 16307, is a fragment of a right jaw, with M_{2-3} , of *Diacodexis*, a genus unknown

earlier than the Gray Bull Wegemann (1917, p. 59) quotes Gidley's identifications of some specimens from near the Davis ranch. Gidley mentions three mammal teeth, one (U.S.N.M. No. 6734) of *Pelycodus* sp., an Eocene form. Another (U.S.N.M. No. 6733) is a deciduous left P⁴ of *Ectocion* sp., a genus which ranges from the Silver Coulee Paleocene into the Eocene, and is useless in accurate dating of the strata. Other specimens from the Davis ranch locality were identified by Loomis as *Hyracotherium*, also diagnostic of the Eocene. Thus from the evidence of the mammalian fossils the Kingsbury conglomerate and the Davis ranch locality are Early Eocene.

The only described mammalian fossil from the type Fort Union of North Dakota is the specimen, found near Buford by Dr. Vernon Bailey, which Gidley (1917) called *Titanoides primævus*. Species of the genus are known also from the upper Melville and from the Silver Coulee. As identified the genus is thus confined to a very limited stratigraphic range. Dr. Thom states (personal communication) that the level of *Titanoides primævus* may be either upper "Tongue River" or lower "Sentinal Butte." These members are generally considered to be post-Lebo in age, and the evidence from *Titanoides* supports this view.

In Billings County, North Dakota, T. 136 N., R. 88 W., specimens referred to *Pantolambda* and *Tetracænodon* were found in "an erosion channel from 30 to 50 feet deep in the Cannonball member filled with channel deposits of a Fort Union stream" (Lloyd and Hares, 1915, p. 538). These specimens came from the Tongue River member according to Thom and Dobbin (1924, p. 495), and represent species of genera which are found associated in the Torrejon and the Lebo. *Tetracænodon* is also reported from the Rock Bench, from the lower part of the Melville and, doubtfully, from higher levels in the latter and from the Paskapoo. Merely upon these generic identifications the "Tongue River" of North Dakota might be correlated with the Lebo (and the Torrejon), in contradiction to the more general belief that the Lebo is older than and below the Tongue River. However, since the Billings County vertebrate specimens occurred in a channel they could have been redeposited from the underlying Cannonball although such a suggestion would necessitate the unusual presence of land mammal remains in marine sediments. In short, until more specimens are found in the Billings County and adjacent areas or until the rock

units are further studied and traced the vertebrate evidence can be interpreted equivocally to fit diverse correlations.

The extraordinarily interesting recovery of a specimen of a Paleocene mammal *Anisonchus fortunatus*, from a deep well in Louisiana (Simpson 1932), in an unintentionally taken core, may be tentatively evaluated as an extremely tenuous aid in dating and correlating the Cannonball marine beds of North Dakota. W. C. Spooner believed that the specimen came from the Tokio formation (of Austin age) but also stated that, under the circumstances of its collection, it could have come from the Midway. The Tokio is "early late" Cretaceous in age and the Midway group is early Tertiary. *Anisonchus* is composed of other species from the Puero, the Dragon, the Torrejon, the Lebo, the Rock Bench, and the Melville. It is a long range genus but is particularly characteristic of Torrejonian time, and Simpson (1932, p. 4), from the study of the specimen, states that *A. fortunatus* "seems to be of approximately Middle Paleocene, Torrejon age." It is incredible that the specimen came from the Tokio, and Mr. Spooner's second alternative of its origin, that it came from the Midway, becomes much more probable. Simpson suggests that it may belong in "a post-Cretaceous intercalation not present, or not recognized, in surface exposures of adjacent regions," but this is regarded as unlikely. The Midway, according to Dorf (1940, p. 231), has "yielded a number of [invertebrate] species closely similar to or identical with" those from the Cannonball. This accords with Stanton's observations (1920, p. 13). Thus, through a series of largely unproved possibilities, the Cannonball may be correlated, in part at least, with the Midway, the Midway with the Torrejon, the Torrejon with the Lebo and the Lebo with the Cannonball. Such an extremely tentative and uncertain chain of possibilities may be even absurd, but Thom and Dobbin (1924, p. 494, Table 2, p. 498) also suggested the correlation of the Lebo with part of the Cannonball, from different evidence.

III TAXONOMY

Order MULTITUBERCULATA Cope, 1884

Relationships of the Multituberculates

Affinities of the multituberculates to other mammals have been discussed recently by Simpson (1937a, pp. 71-72, 1937e, pp. 759-762) in considerable detail and the new material from the Paleo-

cene of Polecat Bench adds little to his analyses. The detailed comparisons which have been made in this work among the allotheres themselves, and with members of other subclasses of mammals, reinforce the growing impression that the multituberculates represent an ancient experiment in mammalian structure which started near the very beginning of mammalian emergence from reptilian ancestry and continued until "modern mammals" were firmly established. Indeed, the suggestion that multituberculates had an ancestry distinct from that of other mammals from pre-mammalian time may be reemphasized.

Multituberculates were probably vastly more numerous than even the highest estimates of their Paleocene ubiquity had calculated, both as individuals and as taxonomic groups. Certainly the many known specimens of multituberculates incompletely represent the actual numbers and diversities of populations, but already there is an indication that the chant of "parallelism" by students of fossil rodents may be sung about the multituberculates also. Very likely they occupied several of the niches in nature now held by the rodents. The fact may be of significance that of the Paleocene localities from which large faunal samples have been collected, all save one have yielded multituberculates, and this one, Bear Creek, Montana, is the only place where Paleocene rodents have been identified (Jepsen, 1937a). So untypical is this situation that Simpson (1928a, pp 14-15, 1929b, p 9), working with the fragmentary material from Bear Creek, assigned specimens now known to represent two other orders, primates and rodents, to the multituberculates.

Allotheres attained what are called "specialized" structures early in their long history (from the Triassic to the Eocene) and continued, with differential velocities in various groups, away from "primitive" structures, to the end of their time. However, many of the comparative standards of degrees of specialization may have been misapplied. The study of multituberculates has led to new conceptions of the variety of ways fundamental structural plans can be modified. An attempt to expose the heritage features of allotheres by stripping off habitus masks soon indicates that the conventional ideas of mammalian structures must be broadened or especially altered. Many criteria for the satisfactory establishment of taxonomic categories among other groups of mammals are invalid when applied to multituberculates. Some of the uncertain-

ties and indecisions in the classification and description of the fossil theria are easily solvable when compared to those which confront an attempt to get reasonable notions from the remains of multituberculates. No group even remotely related to them lives today. The attempted application of the usually workable scheme of putting mammals in taxonomic niches whose positions are indicated by related groups either living or extinct, has confused more than advanced the classification of the allotheres. And yet no other system can be proposed. If, at the outset, the study of them had had their isolation as a viewpoint, their peculiarities might have been more readily recognized and indicated.

Study of the Paleocene multituberculates, which were confined to a short span near the end of the vast geological range of the order, indicates their lack of similarity to other mammals. Not one specimen, of all the multitude collected, shows a deciduous tooth in the process of being replaced by a permanent one, although several jaws and skull fragments obviously belonged to young and small individuals with the teeth erupted less fully than in the adults. In other groups of mammals, represented by similar numbers of fossils, there are many examples of the deciduous dentition in various stages of replacement by permanent teeth, except in forms where there is only one set of teeth. The suppression of replacements, or a monophyodont dentition, is regarded as a specialized characteristic far removed from the reptilian polyphyodontism. In some marsupials the dentition is essentially monophyodont, the tooth regarded as the third or fourth premolar being the only one which is replaced. There is still some difference of opinion about the unreplaced teeth, whether they be persistent homologues of milk teeth in other orders, or the equivalent of permanent teeth with no deciduous predecessors.

The upper tooth which is conventionally called the fourth premolar in the ptilodont multituberculates has a unique position and configuration. In some genera, *Ptilodus* and *Ectypodus* especially, the anterior end of P^4 overlaps the posterior end of P^3 whose anterior end in turn overlaps P^2 , which has a similar position with respect to P^1 . P^4 likewise overlaps M^1 which also overlaps M^2 . This characteristic and peculiar overlap in both directions from P^4 (Pl. III, Fig. 2a) may be likened to a line of dominoes all leaning toward one which is flat lying, the end of each block resting upon the end of the next toward the flat one.

Without calling upon miraculous extensions and contractions of the alveolar regions of the bone or upon an almost equally inconceivably diagonal eruption of these teeth which have two or more roots, the overlap indicates that P^4 was the first upper cheek tooth to erupt, and that the other teeth then erupted in series in each direction, M^1 and M^2 toward the rear, and P^3 , P^2 , and P^1 toward the front. The relative rates of eruption of the series anterior to P^4 and of that posterior to it cannot be estimated. The peculiarity of eruption order is probably coupled with the fact that P^3 (like P_2) is, in some groups of multituberculates, extremely variable in dimensions and structure. As Simpson (1937a, p. 87) says, "Reduction in the midst of the premolar series rather than at its ends may be characteristic of the multituberculates."

An apparent aberration in the succession of the lower teeth has likewise been overlooked or underemphasized heretofore, although this anomaly could be more easily analogized among other mammals. P_3 fits under the anterior basal concavity of P_4 in such a manner that it must have grown in after or with P_4 , and not before the latter. Were it not for the above described overlap of the upper teeth, this eruption of P_3 after P_4 could be compared to the retarded appearance of the permanent P_3 in other mammals. It has been observed and described in *Phenacodectes* (Jepson 1930b, p. 519). The late eruption of one tooth may be laid to some not understood and rather unimportant peculiarity, but the unusual attitude of the whole upper cheek tooth series in the ptilodonts seems to be a characteristic of the group and is probably significant although its meaning is unknown.

Techniques of Study and Taxonomy

Most multituberculates, especially of the family Ptilodontidae, were small animals of mouse or rat size. Their prominence in Paleocene faunas is due to their being recovered from quarries, for on the surface of the ground their remains are easily overlooked and rapidly disintegrate. It is safe to say that three quarters of the known specimens of Paleocene multituberculates have been collected by quarrying in such spots as the Mason pocket in the Tiffany beds of Colorado (Granger, 1917, pp. 827-828), the Gidley and Silberling quarries of the Lebo, the Scarritt quarry in the Melville (Simpson, 1937a, pp. 29-35) and the quarries in the Polecat Bench area. In all of these sites the ratio of the mammalian micro

fauna to the larger forms is proportionately much greater than the average recovery in surface collections over a wide field. This fact necessitates a careful consideration of facies differences when the faunas are to be used for correlations

Study of the multituberculates from the Polecat Bench area and their comparison with allotheres from other localities have been attended by many technical difficulties in handling the diminutive specimens. Much of the material consists of fragments. Many specimens are crushed. Wear on the molars during the life of the animals produced changes in configuration, making the comparison of individuals of different ages peculiarly difficult and important. Color variations are extreme. Some of the teeth have petrified black, others are very light in color, and a single specimen may be highly varicolored, misleading the eye. One thickness of thin shellac may obscure a structure or constitute a false one. To avoid some of the errors which these conditions invite, the specimens were cleaned with extraordinary care and studied under several powers of a binocular microscope, illuminated by powerful lamps. An aid of great value was the process and result of drawing outlines of specimens with a camera lucida, using precaution to orient the specimens consistently, and then greatly enlarging the outlines to one size, with a projecting system which eliminates distortion. See Figs. 3 to 12.

There has been confusion in descriptions of multituberculate incisors because of the attitude and curvature of these teeth. The root of the lower incisor, when the jaw is oriented with the molars nearly horizontal, is itself almost in a horizontal line, but the enameled crown is bowed, the tip rising toward the molar plane. Thus a line along the convex border from the root end to the cutting tip is first "posterior," then "lower," then "anterior." And a line along the concave border from back to front is "posterior," "superior" or "upper," and again "posterior," although the latter region is far anterior to the most posterior part of the tooth. Therefore in these descriptions, in the interests of clarity and simplicity if not in conformity with scientific jargon, the concave border of an incisor is called "upper" or "superior," the convex border is "lower" or "anterior," depending upon locality, the surface adjacent to the incisor of the opposite side is "inner" or "internal"; and the surface seen from a labial or outside view of the jaw is the "outer" or "external" side.

The serrated P_4 of multituberculates has also been measured and described in a variety of ways. Fig 1 *B* shows the dimension indicated by length, the greatest antero-posterior extent of the tooth crown. Fig 1 *A* shows how "width" is calculated, the distance between tangents parallel to a center plane through the crest of the tooth.

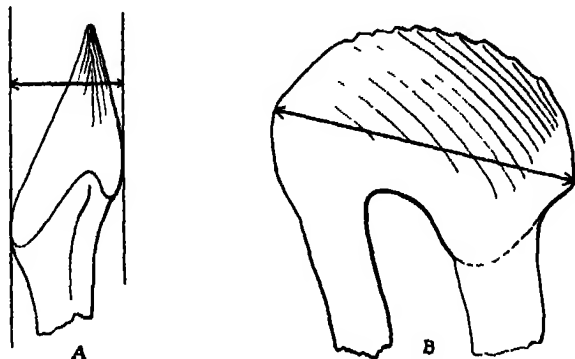


Fig 1 Outlines of P_4 of multituberculate, indicating how measurements are taken
A, width *B*, length

The necessity that the author clean and prepare many of the multituberculates for study has not been an unmitigated tedium. Because most of this work progressed slowly each specimen was subjected to critical examination as it emerged from the matrix, before it could be studied in its entirety, and this has led to a greater appreciation of the minute details of structure than would have been probable otherwise. Many of the small jaws were dissected to a considerably greater extent than is the usual practice, when there was some doubt about the attitude and number of tooth roots, the presence or absence of unerupted teeth, or the disposition of various foramina.

Largely due to the nature of the remains of Tertiary multituberculates, it has been assumed that the apparent similarity throughout the group in the lower jaw and dentition compelled us to rely largely upon the upper teeth for definable taxonomic criteria, although the lower jaws and teeth are far more abundant in the collections. It was believed, of course, that the effects of differentiation which were magnified in the upper teeth, particularly P_4 , had been accompanied by slight modifications of the lowers, but that the latter changes were too subtle for structural analysis. However, even the diagnostic P_4 , by itself, sometimes fails to respond to

taxonomic treatment, as illustrated by Simpson's tentative reference of three P_4 's from the Lebo to *Ectypodus?* (1937a, p 102).⁴ The manipulative difficulties of comparing hundreds of small delicate specimens with each other, two or three at a time, have also discouraged attempts to distinguish and classify the structural peculiarities of the lower jaws and teeth

Simpson (1937a), to solve some of the problems, applied statistical methods to segregate dimensions and ratios which appeared to have taxonomic value. He dismissed the comparison of the lateral contour of P_4 of *Phlodus? douglassi* with that of *P montanus* by saying that this "cannot be adequately checked," and it was to overcome such uncertainties that parts of the present study were initiated. P_4 is the largest tooth in the lower series of the Tertiary multituberculates, and is less affected by individual variation induced by age than are the teeth posterior to it. It is a high, thin, bladelike tooth whose anterior border rarely shows much evidence of wear. Note the worn posterior part of P_4 in Pl II, Fig 1a, and the unworn condition of the front edge. In normal occlusion with

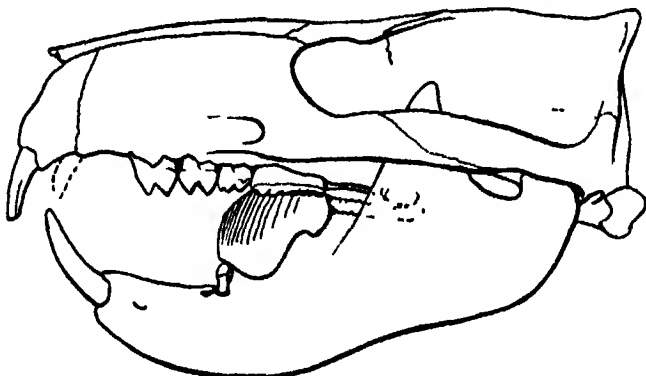


FIG 2 Outline of skull and jaw of *Phlodus*, modified after Simpson, to show occlusion of teeth. About 2 X size of average specimen of *P montanus*

the upper teeth (see Fig. 2) the front of P_4 was far from any contact with the superior series, and even the posterior section of it frequently shows much less abrasion than the lower molars, which were both lowered and shortened by wear.

⁴ Simpson, in this publication, placed a question mark in front of a doubtful categorical assignment. For example, by "*?Ectypodus guleyn*" he intends to indicate that the correctness of the generic, but not specific, name is doubtful. Herein, except in direct quotations, doubtful assignments will be indicated as suggested by Wood (1916, p 42) by a query after "the part of the name to which the doubt relates." A question mark in front of a generic name indicates that both the generic and specific names are in doubt. (See Schenk and McMasters, 1935, p 16.)

The small size of P_4 in most species, its shape, and position in the jaw make comparisons difficult. Its front edge is so greatly curved that part of it is out of focus when it is highly magnified, and the sides of the tooth, being comparatively plane, appear at first to bear few diagnostic structures. Various artifices, such as collodion films or peels and wax squeezes, were used to minimize the difficulties of examination of the specimens themselves, and each of these methods added to the conviction that structural details of P_4 can be used as a key to classification.

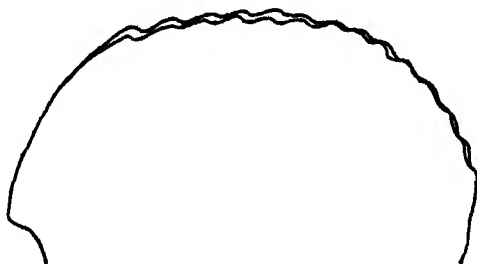


FIG 3 Superposed outlines, about $\times 8$, of the left and the right P_4 s of *Ptilodus montanus*, U S Nat Mus no 6076, to show left-right variation in an individual. Lingual side of teeth, anterior end toward the left.

The advisability of estimating the approximate variation limits of P_4 within different categories of allotheres is obvious if the tooth is to be used in taxonomy. Representative lower fourth premolars of species which have lower and upper dentitions undoubtedly associated in one or more individuals were carefully examined. Each of these described species seemed to have unique and diagnostic characteristics of P_4 structure. Next an attempt was made to see how much variation there is from the left P_4 to the right in the same individual. The few known specimens with both lower jaws preserved show a remarkably close similarity from side to side. One method of indicating this is seen in Fig. 3, where the outline of the right P_4 of *Ptilodus montanus* (U. S. Nat. Mus. No 6076) is superimposed upon the left. These outlines were drawn from the lingual side of the teeth, a view which minimizes differences due to wear, best indicates the profile of the anterior basal concavity, and is least confused by distortion and faulty preservation of the adjacent bone. Fig. 4, where the outlines of 7 specimens of *Ptilodus wyomingensis* are compared, suggests the individual variation range in a sample of a single species.

From these and other methods of study it is possible to distinguish clearly several types of P_4 among the Paleocene multituberculates and to suggest some new and revised generic groupings. In

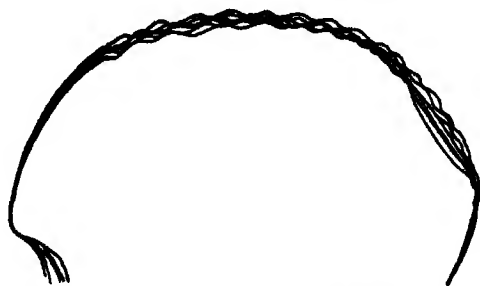


FIG 4 Superposed outlines, enlarged to the same length, of the P_4 s of seven specimens of *Ptilodus wyomingensis*, to indicate individual variation within a species. Lingual side, anterior end toward the left. Note the effects of wear on the postero-superior surface of some of the teeth.

some ways the suggestions herein support and check Simpson (1937a) in his work with the Montana Fort Union multituberculates, but in other respects there is considerably diversity of opinion. His methods were applied chiefly for specific differentiation, whereas the system outlined here was primarily directed toward defining generic categories which are of value in stratigraphy. A recent examination of the most of type material of Tertiary multituberculates reveals some unsuspected facts. The species called *Ptilodus trovessartianus* by Cope has been unquestioned in that assignment since 1882, but it is now seen to be unlike the genotype species *P. medius*. Indeed, *P. trovessartianus* shows many significant differences from most other multituberculates, particularly in the characteristics of its lower incisors and is assigned to the new genus *Mimetodon*.

On the rounded front edge of P_4 in *Ptilodus montanus*, *medius*, and the new species *wyomingensis*, there are three small ridges diverging downward from the first serration. The long middle ridge, exactly in the midline of the superior part of the front edge, directs toward the basal concavity external (labial) to the midpoint of the top of the latter. The other two shorter ridges from the first serration are on the rounded antero-lateral edges of the tooth.

The front view of P_4 of *Ptilodus* shows other distinguishing features. The basal concavity is evenly arched, and the inner leg of the arch of the concavity, although it is much shorter than the outer, does nevertheless descend well below the center of the arch,

in contrast to the structure of the teeth of species in other genera to be mentioned.

The upper row of outlines, drawn to scale in Fig. 6, and the superposition of these enlarged to one size in Fig. 5 show the essen-

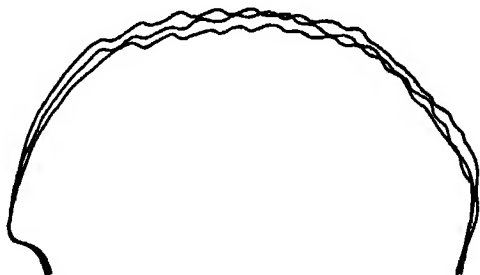


FIG 5 Superposed outlines, enlarged to the same length, of the P_4 s of *Ptilodus mediaevus*, of *P. montanus*, and of *P. wyomingensis*, to indicate interspecific variation within a genus. Lingual side, anterior end toward the left

tial similarities of the shape of P_4 in three species, *mediaevus*, *montanus*, and *wyomingensis* which are herein included in *Ptilodus*. This restricted, *Ptilodus* becomes a sharply defined genus characteristic of the Torrejon and its approximate equivalents, the Lebo and the Rock Bench, although fragmentary specimens which may represent species of the genus are reported from the Dragon, the Melville, and the Paskapoo. Other Lebo species which Simpson tentatively assigned to *Ptilodus*?, *douglassi*, *gidleyi*, and *sinclairi*, are transferred to other genera, as explained below.

The type (and only) specimen which Simpson (1937a, p. 102, Fig. 10, d) called *Parectypodus? jepseni* is a composite, consisting of a small fragment of a left lower jaw with P_4 , and part of an upper molar. Fig. 7 shows the outline of the P_4 compared with that of *Ectypodus simpsoni* (Jepsen). The two teeth have little in common except the lack of the anterior basal concavity which usually indicates an absence of P_3 . The P_4 of *Parectypodus? jepseni* is long and low and may be compared with other species of the genus to which it is herein referred, *Eucosmodon*, in the lower row of Fig. 6. The upper molar has not been identified. Simpson's fallacious association of the lower with the upper tooth may have been due to Gidley's label for the specimens, "Portion of a left lower jaw carrying P_4 (a 1st lower m. associated)."

The new genus *Anconodon* is composed of two species from the Lebo, *gidleyi* and *russelli*, which Simpson referred to *Ptilodus*? and

Ectypodus? respectively. *A. russelli* is represented also by specimens (U.S.N.M. nos 9773 and 9782) which were included in Simpson's *Ectypodus? grangeri*. Both species of *Anconodon* occur

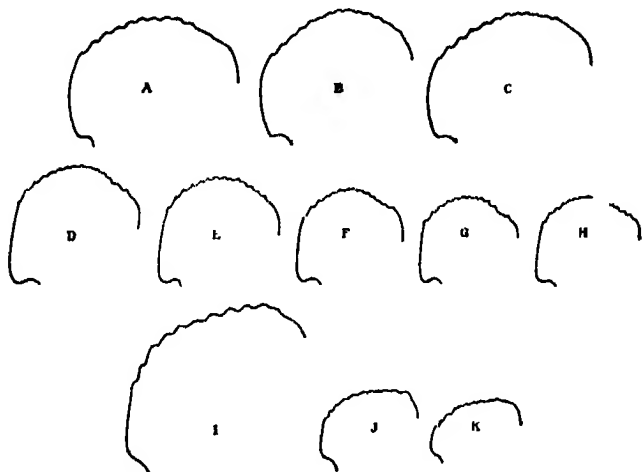


FIG 6 Scale outlines of the lingual side of representative P_4 s of genera and species of ptilodontids, ca $\times 3$. Upper row, *Ptilodus* A, *P. medius* (Am Mus Nat Hist no 3023), B, *P. montanus* (U S Nat Mus no 6076), C, *P. wyomingensis* (Princeton Mus no 14521-4). Middle row, *Anconodon* D, *A. girdleyi* (Princeton Mus no 14524), E, *A. g.* (U S Nat Mus no 9763, type of *Ptilodus? girdleyi*), F, *A. russelli* (U S Nat Mus no 9782, referred specimen of *Ectypodus? grangeri*), G, *A. r.* (U S Nat Mus no 9765, type of *Ectypodus? russelli*), H, *A. r.* (Princeton Mus no 14522). Lower row, *Eucosmodon* I, *E. americanus primus* (Am Mus Nat Hist no 16327, type), J, *E. gratus* (Princeton Mus no 14410), K, *E. jepseni* (U S Nat Mus no 9769, type of *Paractypodus? jepseni*)

also in the Rock Bench. To illustrate graphically some of the differences between this classification and that proposed by Simpson, outline drawings, enlarged to the same size, of the type of



FIG 7. Superposed outlines, enlarged to the same length, of P_4 of *Eucosmodon jepseni* (type of *Paractypodus? jepseni*, U S Nat Mus no 9769), solid line, and of *Ectypodus simpsoni* (type of *Paractypodus simpsoni*, Princeton Mus no 14342), dashed line. Lingual side, anterior end toward the left

Simpson's *Ectypodus? grangeri* and one of his referred specimens, U.S. N. M. no. 9782, are shown in Fig. 8. The outline of the latter tooth is compared in Fig. 9 with *Ptilodus? gideleyi* (type), *Ectypodus? russelli* (type), and two specimens (Princeton Mus. nos. 14522 and



FIG 8 Superposed outlines, enlarged to the same length, of the P_4 of *Ectypodus? grangeri* (type, U. S. Nat. Mus. no. 9801), solid line, and of *Anconodon russelli* (referred specimen of *Ectypodus? grangeri*, U. S. Nat. Mus. no. 9782), dashed line. Lingual side, anterior end toward the left.

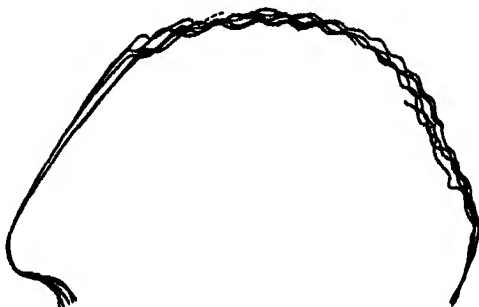


FIG 9 Superposed outlines, enlarged to the same length, of five P_4 s of *Anconodon*, U. S. Nat. Mus. no. 9783 (type of *Ptilodus? gideleyi*), U. S. Nat. Mus. no. 9765 (type of *Ectypodus? russelli*), U. S. Nat. Mus. no. 9782 (referred specimen of *E? grangeri*), Princeton Mus. no. 14522, and Princeton Mus. no. 14524. Lingual side, anterior end toward the left.

14524) from the Rock Bench. The same five teeth are outlined to scale in the middle row of Fig. 6. These drawings are not the basis for reclassification, they were made as a means of indicating the suites of associated characteristics which the practice of citing the serration count and length of P_4 as its sole features in comparing specimens, fails to indicate.

Each of the specimens outlined in the middle row of Fig. 6 has a straight, long front edge below the first serration, the anterobasal concavity is deep and broad from side to side; the inner leg of the

arch does not extend downward as it does in *Phlodus*, the margins of the concavity, when seen from below, are distinctly "squared" and not gently rounded as in *Phlodus*, the lateral ridges leading to the first three serrations are branching, not separate as they are in *Phlodus*. But most distinctive, the straight front edge has a groove below the first serration. This striking feature was originally laid to some peculiarity of wear, only becoming obvious as an important structural element when it was observed in several teeth, some of which are totally unworn, and as it gradually came to be associated with the other features mentioned. Perhaps it was overlooked heretofore because the front edge of the tooth has, like *Phlodus*, three ridges leading to the first serration. These ridges, however, differ greatly from those of *Phlodus*, as described above. They are not symmetrically arranged in *Anconodon*, but two are close together, almost parallel along the antero-external edge of the tooth, and the third is widely separated from them, on the antero-internal border.

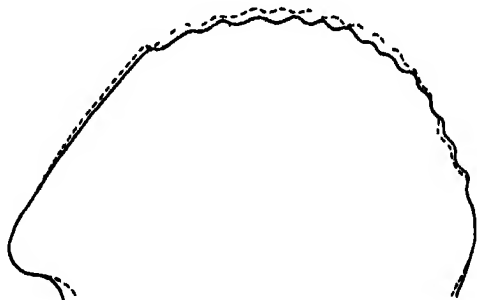


FIG 10 Superposed outlines, enlarged to the same length, of P_4 s of specimens of *Anconodon gidleyi* from different localities. Solid line, U S Nat Mus no 9763 (type of *Phlodus? gidleyi*) from the Lebo of Montana. Dashed line, Princeton Mus no 14524, from the Rock Bench of Wyoming. Lingual side, anterior end toward the left.

In the larger species, *Anconodon gidleyi*, the middle ridge branches into two about midway of its extent downward, and the two anteroexternal ridges (which might easily be mistaken for a single ridge) would, if continued to the lower border, run to the extreme antero-external corner of the tooth. The illustrations of Simpson's *Phlodus? gidleyi* and *Ectypodus? russelli* (1937a, Figs 9B, 10B) show the very highly arched outline of P_4 , the laterally compressed appearance of the tooth when viewed from above, and the distinctive bulge or convexity of the inner side. Fig. 10 illustrates the extreme similarity in the outlines of P_4 of *Anconodon*

gidleyi (type) from the Lebo and Princeton Mus no. 14524 from the Rock Bench. These specimens are similar in size also, being respectively 6.1 mm. and 6.6 mm. long. Structurally the P_4 s of *A. gidleyi* from the Lebo are so much like those from the Rock Bench that, if they came from a single locality, the statement which Granger and Simpson (1929, p. 653) made about a suite of specimens of *Ectypodus musculus* might be applicable here also, "The variation shown is very slight, much less than in most species . . . , an interesting fact suggesting that these remains may well be those of animals which were closely related individually." Also in the smaller species, *A. russelli*, similarity of specimens from the Lebo and the Rock Bench is striking. Fig. 11 shows the outline of the above mentioned specimen (U. S. Nat. Mus. no. 9782) referred by Simpson to *Ectypodus? grangeri*, and that of Princeton Mus. no. 14522 from the Rock Bench. These teeth are each about 5.4 mm. long, outlined to scale in Fig. 6, *F* and *H*.

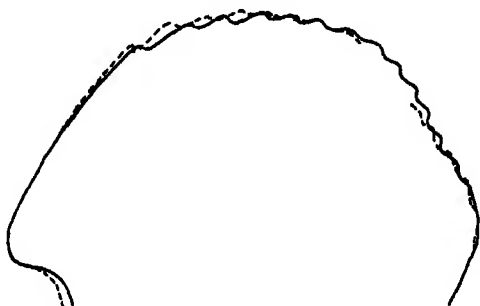


FIG. 11. Superposed outlines, ca. $\times 11 \times$, of P_4 s of specimens of *Anconodon russelli* from different localities. Solid line, U. S. Nat. Mus. no. 9782 (referred specimen of *Ectypodus? grangeri*) from the Lebo of Montana. Dashed line, Princeton Mus. no. 14522 from the Rock Bench of Wyoming.

That the shape of P_4 of the species of *Anconodon* is truly significant as a group characteristic is supported by the structure of the few associated molars. However, some explanation of the distinctions which Simpson noted between *A. gidleyi* and *russelli* in molar dimension ratios and cusp counts may be proposed. The type of the former has less than half of M_1 preserved and cannot be used satisfactorily in specific or generic diagnoses because none of its true dimensions can be calculated accurately. This fact diminishes the value of the ratio of length P_4 : length M_1 . Simpson gives 76 as the cusp count of the external cusps of M_1 , but one cusp had been

obscured by shellac and at least one cusp removed by breakage, making a minimum of 8, and bringing the total closer to the number of cusps, 10, in the external row of M_1 of *A. russelli*. The observations upon structural details of fourth lower premolars lead to the suggestion that *Anconodon* is a distinct genus composed of two species, *gidleyi* and *russelli*, both of which occur in the Lebo (Gidley quarry) and in the Rock Bench quarry beds. This association of two species of a single genus in quarries at two different localities may appear to be contrary to the statement of Matthew (1930, p. 271) that, "We do not, in fact, find two or more distinct species or subspecies of a genus [of mammals] occupying the same area and habitat at the same time," and to the later statement of Cabrera (1932, p. 252) "related animal forms are ecologically incompatible and their incompatibility is the more profound the more directly they are related." This general subject has been recently discussed by Simpson (1937a, pp. 64-69). There is no absolute proof that *gidleyi* and *russelli* were contemporary forms, but their association in two quarries strongly suggests that they were. If the "Law of Ecologic Incompatibility" were universally true of mammals, then the two groups occupied different niches in the same general habitat, or the structural differences would be either of generic or intraspecific, but not interspecific significance. If both groups were found in only one quarry they might be placed in a single species with the proviso that their failure to show overlapping dimensions and intergraded structures might be attributed to mutations or to sexual dimorphism within a species. This latter suggestion may well be the true situation, but the present evidence cannot prove it. Among other groups of multituberculates there is a similar pairing of structures which suggest sexual differences. *Ectypodus simpsoni* and *tardus* from the Gray Bull may be a sexually dimorphic pair, as may *Ectypodus powelli* and *laytoni*, and *Pentacosmodon pronus* and *Microcosmodon conus* from the Silver Coulee, and two groups within the species *Mesodma ambigua* from the Mantua. Additional material of *Anconodon gidleyi* and *russelli* may warrant their separation into two genera, each of which, however, will be sharply distinct from *Ectypodus* and *Plilodus*. Whatever the correct taxonomic explanation may prove to be, these samples indicate that the rocks of the Gidley and the Rock Bench quarries may be nearly contemporary.

No system has been developed to classify satisfactorily the

numerous diverse species which have been assigned to *Ectypodus* and *Parectypodus*. *Parectypodus* has been, as indicated by Simpson (1935a, p. 10), an unsatisfactory genus since its erection (Jepsen, 1930a, p. 120) to receive two Eocene species, *simpsoni* and *tardus*. By 1930 there were confusing statements in the literature about the presence or absence of P_3 in *Ectypodus*. Matthew and Granger, in describing the genotype (1921, p. 1), stated that P_3 was absent. Granger and Simpson (1929, p. 665) later recorded their belief that this tooth was present and, partly upon this analysis, *Parectypodus*, obviously minus P_3 , was proposed. Simpson, in the revision of the Tiffany fauna (1935a, p. 10), says that the presence of P_3 in the genotype, *Ectypodus musculus*, is dubious. As he suggests, the tooth is unquestionably absent, on some specimens, and of doubtful presence on others. The clear absence of this tooth in species assigned to *Parectypodus* was not the sole nor, at the time, the most important reason for proposing the Eocene genus; but since 1930 the situation has been further complicated. Simpson (1937a, p. 99), despite his 1935 statement that the presence of P_3 is dubious in *Ectypodus musculus*, uses its presence in some of the Lebo multituberculates for assignment to *Ectypodus*, and exclusion from *Parectypodus*. The type material has been reexamined in this study and still other complications arise. Although the genotype of *Ectypodus* certainly lacks P_3 in most specimens and probably in all, there are several species represented by lower dentition, from the Lebo and the Silver Coulee, which are similar in many respects to *Ectypodus musculus* but possess P_3 . Other specimens from the Silver Coulee consist of lower and upper teeth which are most likely from a single species and have the typical *Ectypodus* P^4 and also have a P_3 , as does *E. hunteri* from the Melville. Thus the presence or absence of P_3 is of no significance in defining the genus.

The one most logical and least disturbing escape from this confusion is to cashier the genus *Parectypodus* and place its species in *Ectypodus*. This latter genus is already crowded with anomalies and may well serve as a gathering place of similar small species which, when more is known about them, can be generically separated and isolated. This plan does not make *Ectypodus* any more artificial than the genus already had become, and permits a satisfactory restriction of several other genera of ptilodontines. *Ectypodus* thus redefined is composed of the described species *musculus*, *hunteri*, *cochranensis*, *simpsoni*, and *tardus*, and the three new species

TABLE 1

KNOWN DISTRIBUTION OF SPECIES OF AMERICAN TERTIARY PTILODONTIDÆ

	Mantua	Puero	Dragon	Torrejon	Rock Bench	Lebo	Packapoo	Melville	Tiffany	Silver Coulee	Plateau valley	Clark Fork	Gray Bull
Ptilodontinae	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>Mesodma</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>ambigua</i>	x	-	-	-	-	-	-	-	-	-	-	-	-
<i>Kimbetohia</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>campi</i>	-	x	-	-	-	-	-	-	-	-	-	-	-
<i>Ptilodus</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>mediavus</i>	-	-	-	x	-	-	-	-	-	-	-	-	-
<i>wyomingensis</i>	-	-	-	-	x	-	-	-	-	-	-	-	-
<i>montanus</i>	-	-	-	-	-	x	-	-	-	-	-	-	-
spp	-	-	x	-	-	-	-	x	-	-	-	-	-
<i>Philodus?</i> sp	-	-	-	-	-	-	x	-	-	-	-	-	-
<i>Ectypodus</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>cochraneus</i>	-	-	-	-	-	-	x	-	-	-	-	-	-
<i>hunteri</i>	-	-	-	-	-	-	-	x	-	-	-	-	-
<i>musculus</i>	-	-	-	-	-	-	-	-	x	-	-	-	-
<i>laytoni</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>hazeni</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>powelli</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>simpsoni</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>tardus</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>Ectypodus?</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>eilberlingi</i>	-	-	-	-	x	-	-	-	-	-	-	-	-
<i>grangeri</i>	-	-	-	-	-	x	-	-	-	-	-	-	-
<i>sinclairi</i>	-	-	-	-	-	x	-	-	-	-	-	-	-
sp	-	-	-	-	-	-	x	-	-	-	-	-	-
<i>Mimelodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>troessartianus</i>	-	-	-	x	-	-	-	-	-	-	-	-	-
<i>douglasi</i>	-	-	-	-	-	x	-	-	-	-	-	-	-
<i>churchilli</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>Anconodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>guleyi</i>	-	-	-	-	x	x	-	-	-	-	-	-	-
<i>russelli</i>	-	-	-	-	x	x	-	-	-	-	-	-	-
<i>Prochetodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>canis</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
sp	-	-	-	-	-	-	-	-	-	-	-	-	-
Eucosmodontinae	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>Eucosmodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>gratus</i>	x	-	-	-	-	-	-	-	-	-	-	-	-
<i>americanus</i>	-	x	-	-	-	-	-	-	-	-	-	-	-
<i>molestus</i>	-	-	-	x	-	-	-	-	-	-	-	-	-
<i>teihardi</i>	-	-	-	x	-	-	-	-	-	-	-	-	-
<i>jepseni</i>	-	-	-	-	-	x	-	-	-	-	-	-	-
spp	-	-	-	-	x	-	x	-	-	-	-	-	-
<i>Microcosmodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>conus</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>Pentacosmodon</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>promus</i>	-	-	-	-	-	-	-	-	-	x	-	-	-
<i>Neoholomys</i>	-	-	-	-	-	-	-	-	-	-	-	-	-
<i>conventus</i>	-	-	-	-	-	-	-	-	-	-	x	-	-
<i>ultimus</i>	-	-	-	-	-	-	-	-	-	-	-	-	-

named below, *laytoni*, *hazeni*, and *powelli*. Tentatively assigned to the genus *Ectypodus*? are *silberlingi*, *grangeri*, and *sinclairi*. The distribution is listed in Table 1

Wherever possible in studying the multituberculates for this paper, the following dimensions, ratios, and counts have been considered

Numerical

Dimensions

Maximum and minimum diameters of I at alveolus

Length, width, and height of P_4 , M_1 , M_2 , P^1 , P^2 , P^3 , P^4 , M^1 and M^2

Ratios:

Maximum minimum diameters of incisor

Length P_4 . P^4

" P_4 length M_1

" M_1 width M_1

" M_1 length M_2

" M_2 width M_2

Width M_1 width M_2

Counts

Serrations of P_4

Cusps of M_1 , M_2 , P^1 , P^2 , P^3 , P^4 , M^1 and M^2

Roots of teeth

Non-numerical

Shape, enamel distribution, and terminal position of root of I

Presence or absence of P_3

Shape of all teeth

Distribution of lateral ridges of P_4

The various notions which were held about the Polecat Bench multituberculates at the beginning of taxonomic work upon them have been vastly altered to their present form. A statistical approach to their classification appeared, at its first application, to be a highly diagnostic system of discerning order in apparent chaos. The statistical analyses, however, were gradually reduced to a less important role as their applicability diminished and, in many cases, vanished, due, perhaps, to the nature of the specimens. It was discovered that the numerical symbols failed to present to the writer an examinable and objective expression but are as influenced by the personal opinions and the experiences of the compiler and examiner as are other methods of judging the qualities of the ob-

jects, and the statistical method became an interpretive device of doubtful value

Note—After the taxonomic work upon the Polecat Bench multituberculates had been completed for this report, and while the manuscript was being checked for its numerous inevitable errors, it was discovered that there is a remarkably close agreement between Gidley's original taxonomic arrangement, for some of the specimens in the United States National Museum collection from the Lebo, and the system herein proposed. Gidley's labels, for his unfinished work, are still on the trays containing the specimens which were being assorted in groups for return to the National Museum when this observation was made. Simpson's revision of Gidley's work retained all of Gidley's determinations which Simpson, by his statistical analyses, decided were valid, and in other cases his assortment of specimens into species differed from Gidley's arrangement. Simpson grouped U S N M nos 9800, 9801, 9773, 9782, and 9771 together as representing *Ectypodus? grangeri*, and 6088, 9765, and 9766 as *Ectypodus? russelli*. In the study of these specimens and their comparison with others from the Polecat Bench formation I had grouped nos 9765, 9766, 9773, and 9782 (parts of Simpson's *russelli* and *grangeri*) with specimens from the Rock Bench as the species *russelli* of the new genus *Anconodon*. These latter four Lebo specimens had been grouped by Gidley also in a single species which he apparently proposed to call *Ptilodus minor*. On the other hand, Gidley had grouped together as *Ptilodus parvus* specimens which represent Simpson's *Ptilodus? gidleyi*, *Ptilodus? douglassi*, and, in part, *Ectypodus? grangeri*. Omitting specimens, U S N. M nos 9771, 9888a, 9888b, and 6088 which are not adequately comparable with the others, the proposed groupings can be indicated

Simpson (1937a)	Specimens	This paper	Gidley (specimen labels)
<i>Ptilodus? gidleyi</i>	{ 9763 9764 9802	<i>Anconodon gidleyi</i>	} <i>Ptilodus parvus</i>
<i>Ptilodus? douglassi</i>	9795	<i>Mimelodon douglassi</i>	
<i>Ectypodus? grangeri</i>	{ 9800 9801 9773 9782	<i>Ectypodus? grangeri</i>	} <i>Ptilodus minor</i>
<i>Ectypodus? russelli</i>	{ 9765 9766	<i>Anconodon russelli</i>	

Anconodon gidleyi and *A. russelli* are represented also in the Polecat Bench collection, as is a species of *Mimetodon*.

In the belief that future taxonomic work will be facilitated, the method here followed is to describe the members of each order of mammals from the entire stratigraphic section, rather than to discuss each fauna, with its various ordinal groups, separately. However, to help indicate the conspicuous differences within each order from one faunal zone to another, the members of an order from each horizon are treated as a unit. Thus, under the Multituberculata all of the members of this order from the Mantua are described, then those from the Rock Bench, and so on up the stratigraphic sequence. Some specimens are described in greater detail than is necessary to establish their categories, for it is the structural minutia which will be of value in calculating the individual variations within species and the interspecific variations of genera. In most cases a single and usually the most complete, or diagnostic, specimen is selected as the "type" of a species and several other specimens are "referred" to the species. In the analyses of categories these referred specimens are often as significant as the type but such designations as "syntype" and "paratype" have been avoided herein.

Family PTILODONTIDÆ Gregory and Simpson, 1926

The family name Ptilodontidæ appeared in print, but without definition, in Gregory and Simpson, 1926, p. 3, and in Simpson, 1927a, pp. 1 and 2. It was defined by Simpson, 1928b, p. 52, Granger and Simpson, 1929, p. 625, Simpson, 1929c, p. 100, and by Matthew, 1937, p. 289. Matthew explained his reasons for retaining the name instead of those which had been proposed earlier.

A division of the family into two new subfamilies, the Ptilodontinæ and the Eucosmodontinæ, now seems advisable in order to emphasize the observed differences in the Tertiary members of the two groups. This analysis is, of course, based upon incomplete specimens but the structural features listed below appear to be significant and may indicate corresponding or even greater distinctions in other and as yet unnoted details of the teeth and skeletons.

PTILODONTINÆ new subfamily

Type.—*Ptilodus* Cope, 1881.

Distribution.—Late Cretaceous to Early Eocene, North America. Late Paleocene, Europe.

Diagnosis.—Lower incisors slender, with enamel-covered crown. Upper incisors with single tip. Four upper premolars. P^1 - 3 double rooted. Includes the North American Tertiary genera, in addition to the type, *Ectypodus* Matthew and Granger, 1921, *Kimbetohia* Simpson, 1936, *Mesodma*, new genus, *Anconodon*, new genus, *Prochetodon*, new genus, and *Mimetodon*, new genus; and the European Paleocene genus *Neoplagraulax* Lemoine, 1880 *Cimolomys* Marsh, 1889, from Late Cretaceous formations of North America, with 4 upper premolars and a slender enamel covered lower incisor, is also allocated to the subfamily.

EUCOSMODONTINAE new subfamily

Type. *Eucosmodon* Matthew and Granger, 1921.

Distribution.—Paleocene to Early Eocene, North America

Diagnosis—Lower incisors large, laterally compressed, with enamel limited to a longitudinal band on the antero-inferior surface. Upper incisors bifid. Three upper premolars.⁶ P^2 - 3 single rooted. Includes, in addition to the type, the North American genera *Microcosmodon* Jepsen, 1930, *Neohotomus* Jepsen, 1930, and *Pentacosmodon*, new genus

Discussion—The American Cretaceous genera *Meniscoessus* Cope, 1882, *Paronychodon* Cope, 1876, and *Essonodon* Simpson, 1927, cannot be assigned to subfamilies at present, due to lack of specimens or to uncertainty of associations among them. Nor can *Liotomus* Cope, 1884, from the European Paleocene Thanetian, be assigned without question to either of the new subfamilies. Three kinds of P^4 are known from the Thanetian. Teilhard (1921, p. 7) recognized the fact that one of these kinds (Teilhard's F, Fig. 7) has certain structural similarities to the P^4 of *Ptilodus*, and indicated that the others are like teeth of *Cimolomys* and *Ectypodus*. Granger and Simpson (1929, p. 660) wrote that Teilhard's type G "is so like that of *Ectypodus* that true generic separation may be impossible," thus corroborating Teilhard's remarks.

Type F of Teilhard was stated by Granger and Simpson to be typical of *Liotomus marshi*. This is now seen to be like some of the P^4 s from the Lebo which Simpson referred to *Ectypodus*?, and which are herein grouped with a similar P^4 from the Rock

⁶ Due to a typographical error (Matthew and Granger, 1921, p. 1), " P^2 " is stated to be absent in *Eucosmodon*, when " P^4 ," was intended. Matthew (1937, p. 293) repeated the misprint.

Bench and called *Anconodon?*, a new genus based upon species represented by lower dentitions and jaws. Thus, the three kinds of P^4 from the Thanetian of France, types E, F, and G, are more or less simulated by those of the American Paleocene genera *Ptilodus*, *Anconodon?*, and *Ectypodus*, respectively. A lower incisor fragment from the Thanetian may represent a eucosmodontine, as Teilhard suggested, apparently having the characteristic limited distribution of enamel.

*Synopsis of Generic Characteristics of American Tertiary
Ptilodontidae*

Many different arrangements of the American genera of Tertiary ptilodonts were made in order to systematize and explain the methods of classification, but a simple key seems impracticable if not impossible to frame, at the present time, and, as a result, the following rather complicated synopsis of genera is presented.

- | | | |
|---|--|---------------------|
| I | Crown of lower incisor completely covered with enamel | <i>PTILODONTINÆ</i> |
| A | Incisor slender, slightly compressed laterally, oval in cross section, longitudinal groove on internal face | |
| 1 | P_4 low, anterior surface symmetrically rounded (convex) above anterior basal concavity | |
| | (a) No groove on front edge of P_4 | |
| | (1) Lateral profile of P_4 comparatively symmetrical (see Fig 5). Three evenly-spaced almost parallel ridges from first serration, middle ridge long and distinct. M_1 with few cusps. P_4 more than two times the length of M_1 . P^4 short crowned, with two complete cusp rows and one shorter row. | <i>Ptilodus</i> |
| | (2) Front of P_4 higher than rear. More cusps on M_1 . | |
| | a. Two to four strong asymmetrically-spaced divergent ridges from first serration of P_4 . P_4 less than two times the length of M_1 . P^4 arched, bladeliike, high shearing edge with convex lateral profile, one mid row of cusps and a few antero-external cusps. | <i>Ectypodus</i> |
| | b. One or two short weak ridges from first serration of P_4 . Jaw very shallow. | <i>Mesodma</i> |
| | (b) Wide groove on front edge of P_4 . Rear of P_4 higher than front. Three evenly-spaced ridges from first serration, middle ridge very short and small. P^4 with single low concave cuspidate shearing ridge and incomplete external row of small cuspules. | <i>Prochetodon</i> |

- 2 P_4 high; anterior surface "squared" above anterior basal concavity, front edge straight (see Fig 9), with narrow groove below first serration. Three asymmetrically spaced ridges from first serration. Branching anterior ridges on lingual side. *Anconodon*
- B Incisor large, more compressed laterally, plane internal face. P_4 of moderate height, front higher than rear. 1 ridge from first serration branches into three ridges on anterior surface. Few cusps on M_1 . *Mimetodon*
- II Enamel limited to longitudinal band on antero-inferior part of lower incisor, which is compressed laterally. EUCOSMODONTINÆ
- A P_4 absent. No anterior basal concavity on P_4 .
- (1) P_4 longer than M_1 . P_4 wide, with strong and numerous but irregular serrations and ridges. *Eucosmodon*
- (2) P_4 shorter than M_1 . Few serrations on P_4 . Cusps 4 3 on M_1 . *Pentacosmodon*
- B P_4 present. Anterior basal concavity on P_4 .
- (1) P_4 longer than M_1 . Many serrations on P_4 . *Neoliotomus*
- (2) P_4 shorter than M_1 . Few serrations on P_4 . Cusps 7 5 on M_1 . *Microcosmodon*

Kimbetohia is omitted from this synopsis because it is known only from upper cheek teeth. *Pentacosmodon* may prove to have an enamel covered tip of the incisor crown.

Some additional observations which have been useful in the taxonomy of the ptilodontids may be here added, to aid in the identification of fragmentary or incomplete specimens. The distance from the center of the anterior basal concavity of P_4 to the first serration, measured in a straight line, is more than one half the length of P_4 in *Anconodon* and *Prochetodon* and in some specimens of the conglomerate group *Ectypodus? sinclairi*. In *Ectypodus*, *Ptilodus*, and *Mimetodon*, the first serration is between one half and one third of the length of P_4 above the anterior concavity, and in *Mesodma* is less than one third. This comparison may serve to emphasize further the very high front edge of P_4 of *Anconodon* and the low anterior border of the tooth in *Mesodma*. Fig 12 indicates the differences of the outline of P_4 in typical specimens of the American genera of ptilodontines (omitting *Kimbetohia* because its P_4 is unknown) and diagrammatically shows the comparative distance from the anterior basal concavity to the first serration for each genus, placed above a scale of fractions of the total length of P_4 . In all of the ptilodontines except *Mimetodon* the lower incisor has a maximum - minimum diameter ratio of from 1.3 to 1.7. The compressed incisor of *Mimetodon* has a ratio of 1.9 to 2.0. All of the eucosmodontines have a corresponding ratio of more than 2.0.

Eucosmodon can be distinguished from *Neoliotomus* by the few cusps of simple structure on M_1 of the former and the numerous

complicated cusps of the latter. There is also a considerable contrast in P^4 of the two genera, *Eucosmodon* having one complete cusp row and several additional anteroexternal cusps in a second row,

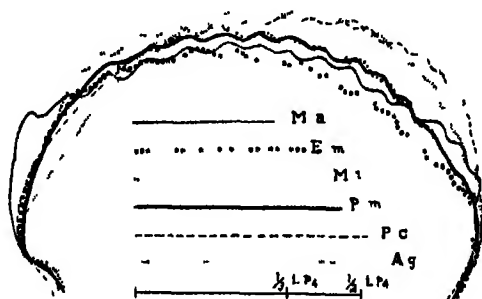


FIG 12 Superposed outlines, enlarged to the same basal length, of representative P^4 s of American genera (except *Kimbelohia*) of Tertiary ptilodontines. Light line, *Mesodma* (*M. ambigua*, type, Princeton Mus no 14414). Circles, *Ectypodus* (*E. musculus*, Am Mus Nat Hist no 17375). Dots, *Mimelodon* (*M. troessartianus*, Am Mus Nat Hist no 3026). Heavy line, *Ptilodus* (*P. medius*, Am Mus Nat Hist no 3023). Dashed line, *Prochetodon* (*P. cavus*, Princeton Mus no 14436). Parallel dashes, *Anconodon* (*A. guleya*, type, U S Nat Mus no 9763). Scale indicates fractions of the length of P^4 , and, above it, the comparative distances from the anterior basal concavity to the first serration of the specimens drawn. Symbols as for outlines.

whereas there is but a single cusp in addition to the serrate blade of the P^4 of *Neohotomus*.

Neohotomus has three symmetrically disposed ridges on the anterior face of P^4 , much as does *Ptilodus*, but the two genera are not likely to be confused.

Mantua Multituberculatae

Two genera of ptilodont multituberculates in different subfamilies can be sharply distinguished and defined on the basis of the specimens from the lowest Tertiary faunal horizon of the Polecat Bench section (see Plate I). These are.

Ptilodontinae

Mesodma ambigua, new genus and species.

Eucosmodontinae

Eucosmodon gratus Jepsen, 1930.

Several species could be erected within each genus, on the basis of minute structural and dimensional differences, but from analogy with what is now known about individual variation in related species, the present system of making as few species as possible to classify all of the specimens seems advisable. The appli-

cation of this principle to multituberculates from other localities and horizons will in some cases reduce the number of species. There is a choice of two methods when many specimens are known from incomparable fragments. As many as possible can be placed in one species with the understanding that future associations may indicate assortments or splits into additional species, or the fragments may be grouped into numerous species tentative to their being bound into fewer categories with the discovery of associations. Of these two guesses, the former has simplicity to recommend it. In the present case, the grouping of many unassociated lower teeth into only two species does not involve much of the usual dilemma, because the two groups are so dissimilar in character and in size. Generic and specific diagnoses are based principally upon associated structures.

No representatives of the multituberculate family Tæniolabididae have been recovered from the Polecat Bench formation. Genera of the family are known from the Puerco, Torrejon, Dragon, Paskapoo, and the Mongolian Gashato. Their absence from the Mantua, Lebo, and Rock Bench collections may be ascribed to climatic or facies differences or to mere failure of discovery by field parties. Typically, the family in America has a southern distribution, in the Nacimiento basin. One tooth is reported from central Utah (Gazen, 1939, p. 275) and one was described from Alberta (Russell, 1926) but was subsequently lost (Granger and Simpson, 1929, p. 624).

Two specimens of Mantua multituberculates, an upper pre-molar (no 14498) and a lower P₄ (no 14499), cannot be assigned with confidence to either *Mesodma* or *Eucosmodon*, and will be discussed below.

PTILODONTINÆ

MESODMA ⁶ new genus

Type — *Mesodma ambigua* ⁶ new species

Distribution.—Mantua lentil of Polecat Bench formation, Park County, Wyoming.

Diagnosis.—Generic characters inseparable from specific, described below.

⁶ In reference to the intermediate age and structures of the specimens, combining characteristics of multituberculates from later Tertiary horizons and from the Cretaceous.

MESODMA AMBIGUA, new species

(Plate I, Figs. 5, 5a)

FIG 13 Lower jaw of *Mesodma ambigua*, approximately natural size

Type.—Princeton no. 14414, left lower jaw with I, P_{3-4} , M_1 .

Referred Specimens —Princeton nos. 14415, right lower jaw with I, P_{3-4} , 14413, right lower jaw with broken I and P_{3-4} , 14412, left lower jaw with I, P_{3-4} , 14411, right lower jaw with broken I, P_{3-4} , 14497, left lower I

Generic and Specific Characters—Tooth formula $\begin{smallmatrix} 1 \\ 0.3.3 \end{smallmatrix}$ Jaw very slender and low. Lower incisor long and slender with enamel covered crown. P_4 low, anterior base (above concavity) rounded. Front of P_4 higher than rear, 1 or 2 short weak ridges from first serration. 13–15 serrations. P_4 length (type) 4.7, M_1 length 2.6, cusps 7 5 (anterior pair very small). Ratio length P_4 length M_1 1.8. Tips of posterior cusps external row M_1 deflected posteriorly. Other dimensions and ratios given in Table 2.

Discussion—There are several possible interpretations of the characteristics listed above and those to follow. This genus is probably of considerable significance in the earliest Tertiary history of the multituberculates, but the usual observation that much remains to be learned from yet unmade discoveries obtains with force here. From what is known structurally of this new genus, it welds *Philodus* and the various groups called *Ectypodus* closer to *Cimolomys* than they have been heretofore. At the beginning of this work, and before the development of a key to the identification of genera by P_4 , the specimens were assigned to a new species of *Philodus* with an accompanying explanation that its placement in that genus was highly arbitrary. At present *Mesodma ambigua* seems close to certain Cretaceous cimolomyids in many ways, especially in the structure of P_4 , and to some ectypods in others, but it combines these structures and possesses others in a way that indicates generic isolation. The genus appears to be a direct derivative of *Cimolomys* and possibly gave issue to one or more of the groups collectively assigned to *Ectypodus* and to *Philodus*.

The suggestion has been made several times that *Taeniolabis*, restricted to the Puerco, is more specialized than the later *Philodus*

and its allies (Matthew, 1897, p. 265, Granger and Simpson, 1929, p. 668; Simpson, 1936b, pp. 2-3) Simpson (1937e, p. 735) even states that "it is clear that *Tæniolabis* is more specialized than *Ptilodus* in almost every respect in which they are known to differ in the skull (and also the dentition and lower jaw). *Ptilodus* is, indeed, an excellent structural ancestor for *Tæniolabis*." These estimates of degrees of specialization are difficult to evaluate and are of little value at present for stratigraphic conclusions. At any rate, the Tæniolabididae and the Ptilodontidae were almost certainly distinct families in the late Cretaceous. For years after the discoveries of *Tæniolabis* and *Ptilodus* no form was known from the Puerco that could have been ancestral to *Ptilodus*. This gap in the record of the Ptilodontidae (from the Cretaceous *Cimolomys* to the Torrejon *Ptilodus*) was partly filled by the recently described *Kimbetohia* from the Puerco (Simpson, 1936b), and now *Mesodma*, from the Mantua, seems also to be a satisfactory intermediate between *Cimolomys* and *Ptilodus*. No comparable parts of *Kimbetohia* and of *Mesodma* are known. When lower jaws and teeth of the former, or upper dentitions of the latter, are discovered, *Mesodma* may prove to be synonymous with *Kimbetohia*, but this seems improbable at present. Until more is known of both genera, their use for stratigraphic correlation is limited.

A lateral view of the jaw of *Mesodma ambigua* (Pl. I, Fig. 5) shows the slender and frail nature of the front part of the bone, which appears to consist only of a minimum structure to support the incisor and the premolars, and the relative elongation of the posterior areas for muscle attachments. In every specimen the bone has probably been somewhat distorted. The bone anterior to the premolars is a short collar around the incisors. On its inner surface the symphyseal scar is a long kidney-shaped area with its greater axis directed obliquely downward and backward. A small mental foramen is located a short distance anterior to the root of P_1 , at about one half the vertical depth of the bone in this region.

Although the coronoid process is crushed and displaced in each specimen, it arose opposite the middle of M_1 and was high and recurved. The sigmoid notch is long. Below the condyle, about which little can be discerned, the bone projects posteriorly a slight distance, but not as much as it does on *Ptilodus montanus*. At the lower border of the deep and long masseteric fossa, the bone forms a shelf or platform which narrows posteriorly. The pterygoid fossa

is very deep, bounded below by a shelf which curves upward along its inner side. This curved ridge diminishes rearward, so there is no true angular process. Anteriorly also this fossa is limited by a ridge which is backwardly curved. One way to describe this fossa would be to compare it to the interior of about half of a flattened conoid figure or paraboloid with the axis directed anteriorly and downward, and with the apex at the lower anterior corner of the fossa, where it becomes the dental foramen, below the posterior root of M_2 .

The lower I is similar to those of several other genera and species of allotheres, but with a few differences. Enamel completely invests its crown. In cross section the tooth has a somewhat distorted oval outline, angulate at the lower inner corner, with a maximum diameter about 1.6 times the minimum transverse dimension at the alveolar lip. The enamel is thicker on the lateral or outer than on the inner face of the unworn tooth and is distributed in a characteristic way, continuing on the lower border into the alveolus, but ending on the superior surface anterior to the socket so that when viewed from either side the outline of its extent makes an oblique line, descending posteriorly. On the lower surface the enamel ends inside the alveolus, slightly anterior to a point below the front edge of P_2 and slightly posterior to a midpoint on the lower convexity of the incisor. The tip is not a point but a fine oblique blade, directed postero-laterally. This edge soon ceases as it descends the superior outer face, and there is no "posteroexternal (buccoproximal)" crest as described for *P. montanus*. The outer face of the crown and root is convex and regular, but the inner surface of the crown is comparatively plane, modified by a longitudinal groove near the anterior-lower edge which extends from the tip nearly to the termination of the enamel. The lower part of the inner face is ridgelike because of this groove above it. The crown has a shorter radius of curvature than the root, which is almost straight and has a closed end. On the root itself there is a longitudinal depression on the outer face.

P_2 appears distinctly more primitive than a similar tooth from most other species of Tertiary multituberculates. It may reflect or preserve a few ancient characteristics that are lacking in most later representatives of the subfamily. It is longer anteroposteriorly than its transverse diameter, as can be seen in Table 2. It is large relative to P_4 and its crown fits into the antero-basal con-

cavity of the latter. Enamel covers the crown and extends downward about halfway to the alveolus, descending farther on the anterior than on the posterior face. Below the crown the tooth is slightly constricted. The root is very long and tapers to a point lower than the termination of the anterior root of P_4 . As indicated in Pl I, Fig. 5, both these roots are external to the horizontal root of the incisor. The crown of P_3 has a minute cusp on its antero-labial slope with a ridge extending downward from it and another directed posteriorly. On some specimens there is also a minute antero-lingual ridge and a distinct but shallow vertical groove on the anterior side of the crown and extra-alveolar part of the root. These features appear to be variable and can be seen only when considerably magnified and properly illuminated. They could easily escape observation.

The height (H) of P_3 given in Table 2 is measured from the top of the crown to the alveolar border.

P_4 is low and long and the outline of the cutting serrated blade is a shorter arc of a larger circle than it is on *Phlodus* and other genera. When M_1 is on a horizontal plane, the highest part of P_4 is slightly anterior to the rear edge, but the curvature here is so gentle that there is no precise apex. The lateral ridges, whose union from opposite sides at the blade edge form the serrations, are clear, regular and sharp, but their curvatures, like the outline of the tooth, have longer radii than they do on most Tertiary multituberculate last lower premolars, being similar in this respect to the ridges on various premolars assigned to the Cretaceous *Cimolomys*. Of the 15 serrations on P_4 no. 14414, the anterior four are somewhat farther apart than the remaining eleven whose apices are equally spaced. Other specimens have 13 or 14 serrations on P_4 . In each case these saw-teeth enlarge progressively toward the rear, the last several becoming distinct cones directed upward and backward. The modifications of the posterior part of the lateral faces seem varied. On all, the lateral ridge from one of the posterior serrations has not only the usual oblique downward and forward direction but a branch which leads downward and slightly posteriorly, the whole presenting an inverted V pattern. The cusps involved is variable, being the fourth from the rear on some teeth and the antepenultimate on others. In each case, however, the ridges from the cusps posterior to the V parallel its posterior branch, and not its anterior leg, there being thus an unusual variation from the stria-

tion pattern of *Phlodus* and *Ectypodus*, but one which is rather vaguely displayed on some teeth identified as *Cimolomys* in the Princeton collection from the Lance

An additional feature of interest and certainly of significance is the presence of a short, narrow, cuspidate shelf on the posterior part of the labial side of P_4 of *Mesodma ambigua*. This row of cuspules is so closely united to the lower part of the external slope of the main blade that it increases the width of this part of the tooth only very slightly. These tiny connules are worn on all the available specimens, being almost obliterated in some but preserved well enough on one (no 14414) to show that there were certainly three and perhaps four or five on the ledge. Other specimens have only two distinct cuspules in this region. This cuspidate shelf is reminiscent of the structure of *Cimolomys* and the earlier *Psilonodon*, and is quite different from the solitary crescentic ridge which modifies this region in unworn teeth of *Phlodus*.

One specimen, no. 14414, has, in addition to the cusps on the labial side of P_4 , a distinct but small cusp on the postero-lingual corner, with a wing ridge curving labially and upward to merge into the base of the last midline cusp. Despite the lateral embellishments on the rear of P_4 , this part of the tooth is comparatively very narrow.

The antero-basal concavity of P_4 into which the crown of P_3 projects is large and lined with enamel which extends down from the anterior edge into this excavation. P_4 has two roots, and the lip of the alveolus for the posterior root is below the level of the molar alveoli.

The specimens, especially the fourth premolars, can be divided into two groups with slightly differing characteristics. One assemblage, as exemplified by nos. 14413, 14414, and 14415, has larger fourth premolars with more serrations than the other group represented by nos. 14411 and 14412. The actual differences are more striking than indicated by the dimensions in Table 2, where measurements are carried only to tenths of millimeters, as the limit to which they are even approximately accurate.

This appearance of two groups within a species of multituberculates may be a great deal more common than has been perceived. The most obvious rationalization is, of course, that the structures showing differences are sex-linked.

M_1 has seven external and five internal cusps in the two main

rows, the anterior pair being small. The postero-lingual cusp is the largest, looking like two conjoined into one. The main cusps are not conical as they are in *Eucosmodon gratus*, but of complex form and the apices, especially of the posterior cusps in the external row, have a backward cant. Each transverse valley between the cusps of each row is crescent- or wing-shaped with the horns or tips deflected backward, embracing the anterior base of the next posterior cusp. The forward bow or convex part of the crescent forms a shallow groove on the posterior slope of the next anterior cusp. An exception is seen in the valley between the second and third cusps of the outer row where the slopes of both adjacent cusps are grooved, and a little longitudinal ridge modifies the transverse valley. There appear to have been many other minor modifications of the intercusp depressions, but their description seems futile until unworn teeth are found.

A discontinuous cingulum-like bulge, more prominent and slightly papillate rearward, appears on the labial side of M_1 . This tooth has two roots. The anterior part of the crown projects anteriorly beyond the anterior root. The anterior surface of the root itself is not a simple pillar-like structure as it is in *Eucosmodon gratus*, but is vertically grooved or fluted and the perpendicular ridge on the lingual side is farther back than that on the outer side, being almost directly under the second cusp of the lingual row, while the first cusp is unsupported below the crown. The vertical groove on the root extends upward on the anterior basal part of the crown as a shallow depression.

This asymmetry of the anterior root is not unique, having been observed, in the course of this study, on other ptilodont molars also. It appears to be another case where great specialization of a single structure affects neighboring units (see Jepsen, 1934, p. 293). The anterior groove of P_4 is functional in that it harbors the pillarlike P_3 , but what purpose the similar although much less obvious grooving of P_3 and M_1 can serve is not known. This may be another case of secondary polyisomerism. This present case appears the more remarkable as it affects the teeth in two categories, premolars and molars. There is as yet, however, little to demonstrate that the teeth of multituberculates can be homologized with those of other mammals.

M_2 of *Mesodma ambigua* has not been found.

One P_4 , no. 14499, differs from those discussed above in several

respects. It is narrower, especially in the anterior part, has only 9 serrations (the posterior two looking like comparatively large curved cones), and a small distinct bulb on the extreme posterior edge. The antero-basal notch is much less developed than it is on the other fourth lower premolars in this species. The range of variation of the premolars of *Cimolomys* is inadequately known for comparison, but some of the teeth placed in that genus, *sensu lato*, have cusps similar in number to those of this specimen. For the present the tooth is not assigned with confidence to any genus or species, but is mentioned for record.

TABLE 2
DIMENSIONS AND RATIOS OF TEETH OF *Merodma ambigua*

Specimens	Lower I			P ₁			P ₄				M ₁				L P ₄ L M ₁
	Max	Min	Max Min	L	W	H	Serra- tions	L	W	L W	Cusps	I	W	L W	
Means	1.45	.9	1.6	.45	.45	.9	12.7	.43	1.5	2.95					
14414 (type)	1.5	Ca .9	1.7	4	4	9	15	4.7			7.5	2.6	1.3	2.0	1.8
14415	Ca 1.4	Ca .9	1.0	5	4	9	14	4.7							
14418	Ca 1.5	1.0	1.5					4.4	1.6						
14412	1.5	.9	1.7				13	3.9	1.4	2.8					
14411	1.4	.9	1.6					Ca 4.0							
14497	1.4	.9	1.6												
<i>M? ambigua?</i>															
14189							9	4.2							

Subfamily EUCOSMODONTINÆ

Genus EUCOSMODON Matthew and Granger, 1921

EUCOSMODON GRATUS Jepsen

(Plate I, Figs. 1-4)



FIG 14 Lower jaw of *Eucomodon gratus*, approximately 1/2 times natural size

Eucomodon gratus Jepsen 1930b, pp. 499-500, Pl. IV Fig. 8.

Type—Princeton Museum no. 13373, incomplete left ramus with M₁ and alveoli of other teeth.

Referred Specimens—Princeton Museum nos. 14419, right lower jaw with I, P₄, and alveoli of M₁ and M₂; 14418, left lower jaw with I and alveoli of other teeth; 14496, left lower jaw with I and P₄; 13374, thin section of left lower incisor, 14495, right lower incisor.

(crushed); 14417, right M_1 , 14416, right $I^3?$; 14494a, right $I^3?$, 14494b, right $I^3?$, 14420, left M^1 , 14420a, right M^1

Distribution — Mantua lentil of Polecat Bench formation, Park County, Wyoming

Generic Characters — Tooth formula $\overline{10112}$ Lower incisor compressed laterally, enamel limited to a band on the inferior surface and lower parts of lateral faces. Pulp cavity high and narrow P_4 relatively low and wide, posterior part of cutting edge comparatively straight, with highest point at posterior edge, anterior ridges irregular

Specific Characters — Comparative dimensions and ratios given in Table 3. Smaller than other species, except *jepseni*, in genus P_4 very small and short in relation to other teeth, but larger, wider, more massive than that of *jepseni*

Other Diagnostic Characters — Eleven serrations on P_4 . Seven outer and 5 inner cusps on M_1 . Four outer and 2 inner cusps on M_2 . Incisor root ends beneath M_2 . Very low coronoid process. Long, broad, and shallow masseteric and temporal fossæ.

Discussion — The type of this species, no 13373, is fragmentary, and many characters were doubtful at the time of its description. New material now permits the alveolar measurements of the type to be replaced by tooth dimensions, and many new observations can be made that substantially redefine the species. The upper teeth listed above are referred to this species because every characteristic of structure and dimension is in harmony with the lower teeth and jaws, as far as can be calculated. Many comparisons and computations, involving statistical and diagrammatic analyses, not detailed here, were made to test the association, and all favor it. These results do not prove the correctness of the assumed association, but strongly support it.

Four lower jaws in the collection show many differences between this and other genera of multituberculates, but until more complete specimens of this and other species of *Eucosmodon* are found, the distinction between generic and specific characteristics will be difficult to make. Because the materials at hand are the most complete jaws of *Eucosmodon* yet reported, some detail in description seems warranted. Insufficient information prevents any very satisfactory comparison of structures or dimensions of the species of *Eucosmodon* known from the Puerco, Torrejon, Rock Bench, Lebo, and Paskapoo, with *E. gratus*. P_4 of *E. gratus* is smaller than

that of the Puerco and Torrejon species, larger than the Lebo form; but the species from the several Paleocene localities do not indicate any consistent evolutionary trends in the genus. As might be anticipated from the geographical distributions, the Puerco and Torrejon species are, in general, more similar to each other than to species outside their range, and the same is true of the Mantua and Rock Bench forms.

Mandible.—The most obvious distinction of *E. gratus* is the extremely long posterior part of the jaw in comparison with other Tertiary ptilotodontids, whereas the portion anterior to the cheek teeth is comparatively short and deep. This anterior depth of the jaw accommodates the high compressed incisor. The unfused symphysis is an ordinal characteristic, and although the symphyseal surface or scar is not rugose, it occupies a greater area than on most ptilotodont jaws, oval, rather than comma, in shape. A low sharp-edged ridge bounds the long shallow masseteric fossa below. The coronoid process is scarcely more than an elevated ridge originating opposite the middle of the first molar. It descends posteriorly to the shallow sigmoid notch and ascends slightly to the narrow condyle in a long curve. Below the posterior surface of the condyle the outline of the bone is smoothly convex, lacking the posterior flare of many multituberculate jaws. The lower border of the jaw is unusually straight and not the customary series of reversed curves. On the inner surface of the jaw the pterygoid fossa is less deep than it is on *Ptilodus* jaws. Anteriorly it extends below the posterior part of M_2 , so that the rear root of this tooth imbeds in a bracket-like structure overhanging the fossa. The lower border of the fossa begins anteriorly as a high ridge which declines to the lower rear corner of the jaw, there being, apparently, no angular process.

The dental foramen penetrates the anterior wall of the pterygoid fossa a short distance below the ledge supporting the posterior part of M_2 , and not near the bottom of the fossa as it does in *Mesodma* and in *Ptilodus*. This is perhaps in accord with the position of the incisor root, to be described. A small mental foramen is situated about one quarter of the distance below a midpoint on the I-P₁ diastema which has its superior border pierced by one or two pits or holes.

Teeth.—The incisor of *E. gratus* is evenly curved, laterally compressed and constricted at its root end. Its maximum diameter

(height) is uniform from the wear surface to the root constriction and averages about two and one half the minimum dimension (width). A section is oval, flattened on the inner face. See Fig. 2, Pl. I. The pulp cavity is very high and narrow. Enamel is confined to a curved band on the lower anterior face, extending around on the inner plane face for about one fifth the height, and on the outer curved surface about one quarter of the height, although the extent of the enamel on the sides varies. On both lateral faces the width of the enamel band increases posteriorly almost to its termination. The enamel does not extend to the end of the root, but ends slightly back of a midpoint on the lower border, inside of the alveolus.

Two jaws with many identical measurements show several differences in the incisors, when the molar alveoli are oriented on a horizontal line, that may be helpful in defining individual growth characteristics of the group,

Characters	14419	14418
Length	shorter	longer
Position of enamel termination	under posterior root of P_4	under anterior root of P_4
Root shape	less constricted	more constricted
Root termination	beneath middle M_2	same

From the way these teeth are closely invested by bone, especially at the tapered root ends, they can have moved in the jaw little or none since death. The roots have encircling waves or growth bands for most of their extent posterior to the enamel. Putting these and other observations together, it would seem that specimen 14418 is of an older individual than 14419, that the incisors grew for a time by the addition of dentine on the root end, pushing the enameled portion forward, but that growth ceased as the root constricted further.

The placement of the tooth in the jaw is quite different from the situation of the lower incisor in *Phlodus*. In the latter the incisor root is enclosed below by bone of approximately the same thickness from the lower border of the alveolus to the end of the root. In *Eucosmodon* there is not such a close conformity of shape of incisor and jaw bone because the root rises a little toward the cheek teeth, as it does in rodent jaws. This may correlate with the elevated position of the dental foramen.

The numerous similarities of the shape of *Eucosmodon* jaws and lower incisors to those of rodents constitute a striking case of structural convergence. There are, however, numerous differences. As far as can be determined from the known specimens of *Eucosmodon*, their incisors did not have the characteristic twist of rodent teeth which causes them to form spirals in cases of malocclusion when growth is not counterbalanced by wear. The tips of *Eucosmodon* incisors are always rounded and not chisel-shaped. The upper incisors are very unlike in the eucosmodontines and the rodents. Dentinal and enamel characteristics are also distinctively different. In *Eucosmodon* the dentinal fibrils abundantly penetrate the enamel, but among rodents only jerboas have been reported to possess this structural peculiarity.

P_4 is very short and low. Its length to that of M_1 has a ratio of 1:1. It is also very wide in terms of its length. Its lowness and comparative width make front and rear views of it have a triangular outline with the apex much less acute than in most other multituberculates. It overhangs the anterior root to a much greater extent than does P_4 of *E. jepseni*. The side ridges leading to the 11 serrations on the cutting edge are small and irregular. The serrations are progressively larger toward the rear, where they become small cones. There was an additional small cone, now worn but evident, rather than the bulge or ridge commonly present on ptilodonts, on the postero-labial slope which widens this region. As stated, the anterior edge overhangs the root, but not to the extent it does in those genera which have P_3 . The front edge of the anterior root of P_4 in *Eucosmodon* is very slightly grooved. When the jaw is oriented, the highest part of P_4 is at the rear, and not near the middle of the serrated edge as it is in *Ptilodus*. The posterior part of the blade is straight. Further, the lips of the posterior alveolus of P_4 of *E. gratus* are on a line with the molar alveoli, instead of being distinctly lower as they are in other genera. Together, these observations emphasize the comparative reduction of P_4 and its distinctive attitude.

The molars indicated in Table 2 were not associated with lower incisors or premolars, but their structures and dimensions indicate their reference to this species. The propriety of calling all of these teeth *E. gratus* has been checked in as many ways as possible.

Although the left lower first molar, no. 14417, is not associated with other teeth or with a jaw, its peculiarities, such as simple cusps

and roots, and its dimensions are in harmony with P_4 and M_2 of other specimens. Further, the impact area, or wear surface, on its front border is of a size and pattern to match that on the posterior edge of the P_4 of no. 14419, both being similarly different from the same feature on other multituberculate teeth.

M_1 has seven cusps in the labial row and five in the lingual. The cusps are bold, comparatively simple and have few of the complicated ridges and furrows so characteristic of most Cretaceous and many Tertiary ptilodonts. This simplicity is especially noteworthy about the antero-lingual cusps which are little cones. This same simplicity marks the molar (Am. Mus. no. 3028) referred to *E. americanus* by Granger and Simpson (1929, pp. 648-649).⁷ Enamel on the anterior face of the tooth is comparatively high and this part of the crown does not extend forward beyond the root to the degree it does in other observed multituberculates, nor does the anterior root show the usual asymmetry or bevel of the antero-lingual edge. The crown of this tooth was probably at the same level as the straight posterior part of P_4 .

M_2 is preserved on the type, no. 13373. The bone of this specimen is distorted and crushed so much that it is of little value in comparisons, but it apparently represents a slightly smaller than average individual. The dimensions of alveoli given in the type description can now be disregarded and replaced by tooth measurements.

M_2 , as previously described, has four external and two internal cusps. The anterior edge is straight, possibly due in part to wear against M_1 , and the external row of cusps is much longer than the internal. The tooth is too worn to reveal other trustworthy characteristics.

Three left upper incisors (see Pl. I, Fig. 3) are referred to this species. They show successive stages of wear. The least worn one has an anterior claw-like cusp with its inferior external surface excavated groove-like, and a smaller posterior cone on which the enamel is rugose. Enamel covers the entire crown and extends farther back on the external than on the internal face. The pulp cavity is high and narrow. The root is not wholly preserved on any of the specimens, but it was obviously long and of uniform diameter for a distance at least as great as the length of the crown. The anterior (superior) border is a smooth curve with increasing

⁷ The tooth is a left M_1 but is labeled in the drawing and discussed as being from the right side. This error is perpetuated in Matthew (1937, p. 294).

TABLE 3
COMPARISON OF DENTITIONS OF SPECIES OF *Eucosmodon**

Horizons and Specimens	Lower I			P ₁	M ₁				$\frac{LP_1}{L M_1}$	M ₂			P ²		M ₃				
	Max	Min	$\frac{Max}{Min}$		Serra- tions	L	Cusps	L		W	$\frac{L}{W}$	Cusps	L	W	Max	Min	Cusps	L	W
MANTUA																			
<i>E. gratus</i> (means)																			
14419	37	15	2 49	11	485					1 1†									
14496	35	15	2 33	11	49														
14406	36	14	2 57	11	48														
14418	38	16	2 37																
13374	38	14	2 71																
14417																			
13373 (type)																			
14416																			
14404a																			
14404b																			
14420																			
14420a																			
PUEBLO																			
<i>E. amercianus</i>	60	27	2 22	Ca 12	106														
<i>E. a. primus</i>	42	19	2 21	9	97														
TORRELLON																			
<i>E. molatus</i>	58	26	2 23	Ca 15	15														
<i>E. testardi</i>	52	16	3 25																
LABO																			
<i>E. jepsoni</i> (means of P's)	40	15	2 70	11	43														
ROCK BENCH																			
<i>E. sp</i> no 14471†	36	18	20																

* Means of ratios are given as means of individual ratios and not ratios of the means

† Ratios of measurements from different individuals or specimens not certainly associated

radius from the tip backward. The crown is similar to the broken incisor, questionably and probably correctly referred to *E. teilhardi* by Granger and Simpson.

M¹ referred to *E. gratus* has a comparatively plane crown. See Pl. I, Fig. 4. It has little of the longitudinal and transverse concavity of most ptilodont first upper molars. Further, the anterolingual cusps are on about the same level as the labial side in this region. The number of cusps is small, there being 7 in the outer and in the median rows, all comparatively simple cones having on their slopes, which are squared by wear, but few grooves and ridges. The inner row is unusually short and, though variable, may constitute a generic distinction. It consists of from 5 to 8 cusps, being on a ledge which is interrupted about midway on the lingual edge of the tooth by a vertical groove or valley. Anterior to this groove the ledge is very short and supports 2 minute cusps. Posterior to it there are from 3 to 6 small cusps. The cusps of the median row increase in size almost uniformly from front to back, whereas those of the labial row increase in size toward the middle or fourth cusp which is the largest. These two rows are almost straight, diverging slightly posteriorly, and the corresponding cusps in each row are opposite. The groove between the rows and those between each two adjacent cusps therefore present a small trellis pattern. One small additional cone appears between the anterior cusps of the median and the outer rows. The tooth has three roots. Its anterior and posterior roots are large and the third, situated near the antero-labial corner of the latter, is very small.

The above-mentioned upper premolar, no. 14498, from the Mantua may pertain to either *Eucosmodon gratus* or to *Mesodma ambigua*, probably the former. It is 1.5 mm long and 1.4 mm wide. An attempt was made to allocate it to one species or the other on the basis of its size as compared with the lower fourth premolars of each species, but about all that resulted from a long analysis is the observation that, structurally, the tooth more likely pertains to a eucosmodontine than to a ptilodontine. Its length compared to the P₄ of *Mesodma ambigua* is within the ratio range of similar measurements for *Ptilodus*, but the upper premolars are small and the third is especially variable in the latter genus. Ratios of these dimensions cannot be listed for *Eucosmodon*.

This small tooth from the Mantua has 5 small cusps arranged

in two divergent rows. Many fine ridges run from the tip to the base of each conical cusp. One end of the tooth is plane and the other tapers slightly to a projection on the midline. Analogy with other multituberculate teeth would indicate that this is a left premolar, with the smaller cusps on the labial side and with the midline projection directed posteriorly. In this orientation, there is an area, smoothed by wear with the next posterior tooth, on the postero-labial side of the posterior projection. The tooth is broken a short distance from the crown, and appears to have had only one root, as do the second(?) upper premolars previously referred to *Eucosmodon* (Granger and Simpson, 1929, pp. 636-637).

In *Ectypodus* all known upper premolars anterior to P⁴ have four or fewer cusps, whereas the cusp number on P³ of *Philodus* is highly variable, ranging from four to seven. P³ of *Eucosmodon* is also variable. The specimen from the Puerco described by Simpson (1936b, pp. 2-3) as the new genus and species, *Kimbetohia campi*, consists of a maxillary fragment with three teeth and part of a fourth in it. It is too small to belong to *Eucosmodon americanus* from the same horizon. It is apparently somewhat larger than *E. gratus*, but here no direct comparison is possible. Tooth no. 14498 is smaller than any of the premolars of *Kimbetohia campi*. As far as known the upper penultimate premolar of *Eucosmodon* has but one root, whereas this tooth in *Kimbetohia* clearly has two, and for this reason, as well as others, the latter genus is at present considered to pertain to the ptilodontinae. The wear habit of the known teeth of *Kimbetohia* is unique among multituberculates but may, of course, be an individual curiosity.

Rock Bench Multituberculates

Excavation and classification of the multituberculate jaws and teeth from this second faunal level in the Polecat Bench Paleocene section have been subject to the customary uncertainties, with some added elements of difficulty. The specimens are brittle and soft and the crumbly matrix is composed of sediments which range in grade from impalpable colloidal substances to large pebbles. There is little association of the bones, but the taxonomic work followed the usual procedure of encompassing all of the specimens, of which there are more than fifty, in the fewest possible generic and specific groups. These are.

Ptilodontinæ

Ptilodus wyomingensis, new species*Anconodon* ^s *gudleyi* (Simpson)*A. russelli* (Simpson)*Ectypodus?* *silberlingi* Simpson

Eucosmodontinæ

Eucosmodon sp.

One upper molar, an upper premolar, and some fragments of P_4 s do not belong in any of the species listed above. *E?* *silberlingi* very probably represents a distinct genus, but the present work does not necessitate its definition.

All of the specimens of multituberculates described herein from the Rock Bench quarry beds have come from the site which was quarried, in sec 36, T. 57 N., R. 99 W.

Subfamily PTILODONTINÆ

Genus PTILODUS Cope, 1881

PTILODUS WYOMINGENSIS, new species

(Pl II, Figs 1-3)

FIG 15 Lower jaw of *Ptilodus wyomingensis*, approximately natural size

Ptilodus sp Jepsen, 1930b, p. 503, Pl. IV, Fig. 10

Type.—Princeton no. 14219, right lower jaw, with P_{3-4} , M_1 and alveolus of I.

Referred Specimens —Princeton nos 14290, left lower jaw with root of I and alveoli of other teeth; 14291, right lower jaw with alveoli of teeth, 14467, anterior part of right lower jaw with broken I, alveolus of P_2 and anterior alveolus of P_4 , 14503, right lower incisor; 13361, left P_4 , 14500, 14501, 14502, left lower first molars, 14469, left M_2 ; 14218, right P^{1-3} , 14428, left P^{1-2} in maxillary fragment, 14504, left P^1 , 14505, left P^2 , 14506, left P^2 ; 14507, right P^2 ; 14508, right P^2 , 14468, right P^4 , and 15 fourth lower premolars

Distribution.—Rock Bench quarry beds, Polecat Bench formation, Park County, Wyoming.

^s New genus

Generic Characters—Lower incisor long and slender, crown covered with enamel and more acutely curved than root. P_3 present P_4 with anterior basal concavity. M_1 short relative to P_4 (ratio length P_4 length M_1 , 2.14)

Specific Characters—Very long I- P_3 diastema. Dimensions and ratios given in Table 5. I crown angulate along superior internal edge.

Other Diagnostic Characters—Incisor crown ridged on superior labial edge. Root ends beneath P_4 . Crown of P_3 has enamel only on supra-anterior face, and 1 ridgeform cusp, no anterior groove. 13-15 serrations on P_4 . Ranges length P_4 , 7.1 mm.-8.5 mm.; width P_4 , 2.3 mm.-3.1 mm, length M_1 , 3.5 mm.-3.9 mm; width M_1 , 1.7 mm.-1.9 mm

Discussion—The association of the above listed lower and upper teeth is, of course, not certainly correct, but the type lower jaw is similar to *Ptilodus montanus* and the upper teeth likewise approach those of the latter species so closely that the assignments seem secure. Because *P. wyomingensis* is so closely related to *P. montanus* from Montana and *P. medreus* from New Mexico, this new species has been subjected to particularly critical analysis to determine whether or not the three species could supply information about the relative ages of their respective horizons. It was believed that if the more ancient *Mesodma* is ancestral to any of the species of *Ptilodus*, a comparison of them in great detail might show evolutionary sequences of value in correlation. The correctness and validity of any conclusions, however, must be qualified by so many known and unknown factors that the results of the study are of little importance and are omitted. It is common practice in studying multituberculates to group together fragmentary specimens, representing different parts of the dentition, to form a species even though the association is not demonstrable. Further, some teeth are more readily preserved, or observed, than others, for instance, many more fourth lower premolars of *Ptilodus* have been found than other teeth in the lower or upper series. Thus, the averages or means of measurements of some structures are based upon larger samples of populations than others, and ratios of means may be computed from the mean of the dimensions of thirty specimens of one tooth for one factor and the measurements of a single tooth for the other. Misleading as this situation may be in some cases, it is nevertheless occasionally a useful artifact for studying the classification categories.

Little can be added to what has already been said of other species of *Ptilodus* in describing the lower jaw of *P. wyomingensis*, but this new species has a few proportions and configurations which differentiate it. The bone collar around the incisor root is long and slender in front of P_3 . The symphyseal scar is comma shape with the tail directed posteriorly. A small mental foramen appears slightly below the superior surface of the long I- P_3 diastema, somewhat posterior to a midpoint of the diastema. The upper surface of the bone in this diastema has one small hole at about three-quarters of the distance from the incisor to the premolar. The masseteric, temporal, and pterygoid fossæ are deep, and the dental foramen is at the anterior basal corner of the latter. The coronoid process is apparently stronger, more robust, than in most species of *Ptilodus*, and rises rearward at a comparatively low angle.

Dentition —The lower incisor is less curved than it is in *P. montanus*. The crown is covered with enamel and has the common basal internal longitudinal ridge as well as the less distinct superior labial ridge. There is a pronounced angularity along the inner superior edge where the superior and inner surfaces meet almost at right angles, instead of being smoothly curved with one surface sweeping into the other as they do in all other species of *Ptilodus* in which this region has been studied. The root is expanded (has a greater height or maximum diameter than the crown) in the alveolus, and its end, beneath P_4 , is constricted. The enamel ends under P_3 .

P_3 is club shaped at its crown and has a very long root. The total length from crown top to root tip is 4.9 mm, and the enamel on the supero-anterior surface of the crown extends downward for 1 mm. One small ridgeform cusp modifies the thin enamel on the supero-anterior part, a little labial to the midline. Some specimens of *P. montanus* also show this cusp on unworn teeth, but on them it is relatively smaller and less distinct. Seen from the side, the root of P_3 is convex posteriorly.

P_4 of *wyomingensis* is so similar to some of the homologous teeth of other species of the genus from New Mexico and Montana that, were they all found in the same province or station, their separation into species would be highly arbitrary. As analyzed by Simpson, the distribution of the number of serrations on P_4 of *P. montanus* and *P. medius* shows no bimodality when all of the

teeth are considered together, but, when separated provincially, the Montana species clearly has a central tendency toward 14 serrations, and the *mediævus* group has a median of 12 serrations

TABLE 4

SERRATION COUNTS OF P_4 OF *Pholidus mediævus*, *P. montanus*, AND *P. wyomingensis*

Locality \ Serrations P_4	12	13	14	15
New Mexico	5	1	0	0
Montana	0	8	19	2
Wyoming	0	2	5	4

The distribution of serrations on P_4 of *P. wyomingensis* is shown in Table 4, in comparison with similar counts of the *montanus* and the *mediævus* groups. These data indicate a separation of the New Mexico from the Montana and Wyoming species but the analysis does not indicate any significant difference between the latter two species. Generalities based upon such small samples are insecure, because although the samples are necessarily if peculiarly random, it is impossible to say that they represent the true range of variability or the class distribution which much larger suites would have. Vaguely, however, the data indicate that the *montanus* group has fewer serrations than the *wyomingensis* group. The means are 13.8 and 14.2 respectively, and the mean of the *mediævus* group is 12.2. Although it is doubtful that these differences are significant, they may be interpreted as showing that the two northern species are more similar to each other than either is to the southern species, a suggestion which might be inferred from the geographical distribution of the three groups.

P_4 of *P. wyomingensis* has its 13 to 15 serrations arranged in a straight row, without as much curvature as they show when seen from above in *montanus*. Further, the crest of the P_4 of the latter is more arched, and comparatively higher than that of the former, when viewed laterally. In other respects there are no obvious differences. Even minute details of structure seem to range through the same variations in the two species. The anterior and the posterior roots are connected by a continuous bar or strip along the midline. This must have served to anchor the tooth more securely and to resist sideward and diagonal thrusts.

M_1 of *P. wyomingensis* has a cusp formula of 6.4. The outer row of six cusps is longer than the inner, and the rows diverge

posteriorly The cusps themselves are quite dissimilar on different parts of the tooth. Those of the inner row progressively increase in size posteriorly. The anterior two of the labial row are simple, sub-conical. Although the sixth cusp is the longest of this row, being a ridgeform structure, the third in this series is the most robust and has a small buttress at its anterolingual base. The posterior surfaces of cusps three, four, and five of the outer row are concave because of the crescentic transverse valleys. The external face of the fourth is slightly concave. Grooves, irregular and of varying depths, modify the medial slopes of the posterior cusps. The longitudinal groove between the cusp rows is not straight but deflects around the base of the large third external cusp. The eccentricity described for *Mesodma* modifies the anterior of the two roots.

M₂, likewise two-rooted, has four external and two internal cusps, the former united into a ridge of comparatively small cusps. The two internal cusps are large, their tips bend rearward, and the crescentic groove between them makes the posterior face of the front cusp deeply concave. Complex, deep furrows crenulate the slopes toward the longitudinal valley of all cusps.

P¹ has three cusps arranged in a triangle. See Pl II, Fig 3. The single anterior cusp sets upon a lobe-like extension of the base. Each cusp is somewhat blade-like at its tip, having prominent sharp ridges on opposite sides. The ridges elongate the anterior cusp laterally, as they do the posterointernal cusp, but on the posteroexternal cusp they are diagonally disposed, running antero-internally-posteroexternally. As the ridges descend the slopes of the cusps they curve posteriorly. They were, apparently, soon obliterated by wear. Unworn enamel is rugose, with numerous papillations around the borders of the tooth. It is implanted by two roots. The mean of the length width ratio is about the same as it is in *P. montanus*, and the actual dimensions are similar in that species.

P² is subquadrate in shape, slightly longer along the lingual than the labial side. The two external cusps are ridged as are the anterior and the posteroexternal cusps of P¹, but the internal cusps of P² have three ridges each. This tooth shows remarkable uniformity, all five specimens being very similar. Their length. width ratios are consistently greater than the similar ratio of *P. montanus*. P² of the latter is, therefore, comparatively broad in terms of its length.

Only one P_3 , in specimen no. 14218, has been found. It is 2.7 mm long and 2.1 wide, exactly the dimensions of the tooth in the U.S.N.M. specimen of *P. montanus* no. 6076, but the latter has six cusps whereas no. 14218 has only five. Five is within the range of *P. montanus*.

P_4 , Pl. II, Figs. 2, 2a, is very like that of *P. montanus*, but is slightly more slender, with less robust and more uniform cusps in the inner row and fewer and stouter cusps in the median row. The antero-external shelf or outer "row" of cusps is a papillated expansion of the crown, much as it is on most specimens of *P. montanus*. The cusp formula on the sole specimen is 0 7 9.

ANCONODON, new genus

Type.—*Phlodus? gidleyi* Simpson, 1935.

This genus also includes *Ectypodus? russelli* Simpson, 1935, as herein redefined.

Distribution — Middle Paleocene. Upper Lebo formation ("Fort Union no. 2"), Crazy Mountain field, Montana, Rock Bench quarry beds, Polecat Bench formation, Park County, Wyoming.

Diagnosis — P_4 highly arched, with long straight front edge. Distance from top of deep broad anterior basal concavity to first serration more than half the length of the whole tooth (Fig. 12). Anterior surface "squared" above basal concavity. Three asymmetrically disposed ridges on anterior surface below first serration, two close together and almost parallel along anteroexternal edge, separated from the third (on the anterointernal edge) by a narrow groove. Ridges, on lingual side, leading to second and third serrations, branch from ridge to first serration, or are incomplete anteriorly. Serration row curved, convex side lingual. Internal leg or arch of anterior basal concavity descends but short distance below the apex of the arch. Base of tooth robust, thick. Eight or more cusps on external row of M_1 .

Discussion.—The branching or incompleteness of the ridges on the internal side of P_4 is in sharp contrast to the even separate ridges which continue to the anterior margin of P_4 in *Phlodus*. The greater number of cusps on M_1 of *Anconodon* species is another criterion of distinction between the two genera. Other comparisons of the two genera have been indicated above, on page 252. The lateral curvature of the crest of P_4 may be seen in Simpson, 1937a, Figs. 9b, 9c, p. 96.

Although the known species of *Anconodon*, *gidleyi* and *russelli*, are represented by a relatively small number of specimens, the genus is so clearly distinct from other genera, and so stratigraphically defined, being known only from the Lebo and the Rock Bench, that it is a good index genus

ANCONODON GIDLEYI (Simpson)

Pl II, Fig 4

Figs 6D, 6E, 9, 10, 12

Ptilodus? *gidleyi* Simpson, 1935b, p 225, 1937a, pp. 95-97, Fig. 9b, p 96.

Type —U S N M no. 9763, left lower jaw with P₄ and broken M₁, Gidley quarry, upper Lebo formation ("Fort Union No 2"), Crazy Mountain field, Montana.

Referred Specimens —Princeton no 14524, left P₄, and two broken P₄s, Rock Bench quarry beds, Polecat Ranch formation, Park County, Wyoming U S N M nos 9764 and 9802, Lebo formation

Horizon —Middle Paleocene

Specific Characters —Length P₄, 5.9-6.6, larger than *A. russelli*, 14-15 serrations on P₄. On internal side of P₄ the ridge to the first serration bifurcates into two branches, posterior one forks to the second and third serrations (See Pl II, Fig. 4) M₁ with at least eight, and possibly more cusps in external row

Discussion —As indicated above, pp 253-257, the specimens herein referred to *A. gidleyi* are remarkably similar to those of this species from the Lebo. If they were all from the same station they would be regarded as a singularly unvaried and uniform group. More complete specimens may show distinct groups of variations associated with each locality, but at present the close resemblance appears noteworthy

Princeton no. 14524, left P₄, is 6.6 mm long and has 15 distinct serrations and a very minute posterior one.

ANCONODON RUSSELLI (Simpson)

Figs 6F, 6H, 6G, 8, 9, 11

Ectypodus? *russelli* Simpson, 1935b, p 226, 1937a, pp. 99-101, Fig. 10b, p. 100. *E?* *grangeri* Simpson [part], 1935b, p. 226, 1937a, p. 99. [U S N M. specimens nos 9773 and 9782. Not

nos. 9800 and 9801 which remain in *E.?* *grangeri*] See p 261 above.

Type —U S N M no. 9765, left lower jaw with P_4 - M_2 , Gidley quarry, upper Lebo formation ("Fort Union No 2"), Crazy Mountain field, Montana

Referred Specimens —Princeton no 14522, right P_4 , 14522a and b, left P_4 s, Rock Bench quarry beds, Polecat Bench formation, Park County, Wyoming

Horizon —Middle Paleocene.

Specific Characters —Length P_4 , 4.9–5.4, smaller than *A. gidleyi*. Ridge on internal side of P_4 to the second serration is very short and branches either from the ridge to the first serration or from the ridge to the third. Eleven external and six internal cusps on M_1 of the type and other specimens from the Lebo. Five external and two internal cusps on M_2 .

Discussion — The great differences in configuration of P_4 of some of the specimens which Simpson assigned to *Ectypodus?* *grangeri* have been indicated above, p 254. See Fig 8. Of the five specimens in the U S N M collection which Simpson referred to as *E.?* *grangeri*, the two consisting of isolated P_4 s, nos 9773 and 9782, belong to this new genus. Their length dimensions are greater than any P_4 s that Simpson assigned to *E.?* *russelli* but not greater than a normal range of individual variations within a species. The smallest specimen of *A. russelli* from the Lebo measures 4.9 mm. long, and the above noted two P_4 s are each 5.4 mm long. The residuum of specimens which Simpson (1937a, p 99) referred to *E.?* *grangeri* consists of U S N M nos 9800, 9801 (the type), and 9771. In these M_1 has 8 or 9 external and 7 or 8 internal cusps, and is strikingly different in other respects from the M_1 of *Anconodon russelli*. Other specimens in the American Museum of Natural History and the Princeton collections have the distinctive P_4 of this species.

Rock Bench specimens assigned to *A. russelli* have the following dimensions

TABLE 6

SERRATIONS AND DIMENSIONS OF P_4 OF *Anconodon russelli* FROM THE ROCK BENCH

Specimens	Serrations	Length	Width
14522 (right)	15	5.4	1.8
14522a (left)	15	5.4	1.8
14522b (left)	16	5.1	

ANCONODON? species

No. 14470, a left upper last premolar, Pl. II, Figs. 5, 5a, is tentatively assigned to *Anconodon*. It has twelve small cusps on the main ridge or crest, which is elevated and rounded to a degree almost midway between the structure of typical *Ectypodus* and *Neohotomus* teeth. The cusps are less separated, and their lateral extensions do not continue toward the base of the crown as far as they do in the later *Ectypodus*, and in this respect, P⁴ no. 14470 is like the type P⁴ of *Liotomus marshi* from the Thanetian of France. As in *Neohotomus*, the single outer cusp opposite the third in the main ridge is strong and distinct, and its base widens and deepens the anterior part of the tooth. The seventh cusp of the main row is the highest part, or apex, of the tooth, and the other cusps are progressively lower in both directions. This is unlike the structure of typical *Ectypodus* where the most posterior cusp is the apex, but it is also dissimilar to the known structure of *Neohotomus* P⁴ where the anterior six cusps have only a very slight progressive elevation posteriorly. The tooth is 5.1 mm long and 1.8 mm wide.

Structural features so far mentioned could pertain to either *Ectypodus* or *Neohotomus* with about equal inflation of generic boundaries beyond those of the typical species, but in several other respects, especially in size, the specimen resembles *Ectypodus* more than it does *Neohotomus*. The two roots are close together, not far separated by a long area of crown base as they are in *Neohotomus*. A crown view shows the cusp row to be gently curved, not as straight as that of *Neohotomus*. This tooth probably, but not certainly, pertains to the new genus *Anconodon* whose lower fourth premolar is so distinctive. If it does, the three very similar upper fourth premolars (U S N M no. 9772) which are from the Lebo and which Simpson referred to *Ectypodus*? are likewise to be relegated to *Anconodon* and the genus is represented by both lower and upper teeth from the Lebo and the Rock Bench in a quite ordinary recovery proportion of several lowers to one upper. At present the generic assignment of these upper teeth is of little significance and scant stratigraphic use can be made of them. As Simpson stated, the upper premolars from the Lebo (no. 9772) are distinguished from any comparable teeth and probably represent a species whose lower teeth or jaws he described. The absence in the Rock Bench collection of any lower teeth, except those of *Anconodon*, with which no. 14470 could be associated adds weight to Simpson's further

suggestion that the lower teeth of *Ectypodus? grangeri* may be appropriately associated with the upper premolars under discussion because, as explained above, some of the specimens which Simpson called *E.? grangeri* are now referred to *Anconodon*, on the basis of structures of the diagnostic P_4 . It is possible, of course, that the upper teeth from the Lebo do pertain to the species which remains as *E.? grangeri*, and that the lower teeth of this or a closely related species have not yet been found in the Rock Bench deposits.

The removal of the teeth from the overburdened *Ectypodus* helps purify this genus, at least as far as upper teeth are concerned, and also strengthens and refines the use of the last upper premolars for taxonomy and stratigraphy

Genus *ECTYPODUS* Matthew and Granger, 1921

ECTYPODUS? SILBERLINGI Simpson, 1935

An isolated right P_4 , no 14539, given to Princeton by Dr H E Wood, and a fragment of another right P_4 , no 14523, are the smallest allothere P_4 s in the Rock Bench collection. No 14539 has eleven serrations and measures 3.2 mm. long, this dimension being within the range of Simpson's *Ptilodus? sinclairi*, and very close to the type and only described specimen of *Ectypodus? silberlingi*. To the latter the Rock Bench specimens have many close resemblances. The type of *E.? silberlingi* lacks the antero-superior edge of P_4 but enough remains to show that, on the inner side, the ridges leading to the second and third serrations branch from a single ridge, as they do on Princeton nos. 14539 and 14523. This characteristic has been noted above in the much larger *Anconodon*, but in this latter genus the ridge to serration one joins the common stem of those to two and three.

The two small specimens from the Rock Bench are too worn for comparison of extremely minute details, but are very similar to *E.? silberlingi* in configuration as well as size and ridge distribution. The species will almost certainly prove to belong to a genus distinct from *Ectypodus* when better and more complete specimens are found. On M_1 of the type of *E.? silberlingi* there are ten external and five internal cusps. The posterior two of the labial row are solidly conjoined into a long ridge with only a faint surface indication of its bicuspid nature. Wear, however, indicates the presence of two hillocks in the dentine, which although not a trustworthy criterion of the cusp placement or number, is checked in this case

by the examination of the dentine through the enamel, by strong and colored light

This species' poor representation in the Rock Bench makes it weak for correlating purposes, and for this reason Simpson's questionable generic assignment is retained. Although the antero-superior edge of each P_4 assigned to this species is broken or worn, enough remains to indicate that the structure of this part of the tooth is distinctive and will provide accessory characteristics to classify the genus when this can be done with more complete specimens

Subfamily EUCOSMODONTINÆ

Genus EUCOSMODON Matthew and Granger, 1921

EUCOSMODON species

A left lower jaw, Princeton no 14471, with a much worn incisor and the alveoli of the cheek teeth is preserved in the collection of multituberculates from the Rock Bench. It is not a good specimen upon which to base a species because it has been abraded and distorted and crushed. Recording it is important, however, because its presence in the Rock Bench helps delineate the range of the genus which is restricted, so far as known, to the Puerco and the Torrejon and their approximate equivalents, the Mantua, the Lebo, and the Rock Bench. It is recorded (Russell, 1929, p 166) from a locality of the Paskapoo which correlates with the Torrejon.

In working with this specimen, the idea was entertained that it might be a jaw of *Eucosmodon gratus* which had been partially fossilized and then washed into the Rock Bench channel. There is no real evidence for this, however, and many of the Rock Bench specimens which show little or no evidence of transportation are fossilized to the same pale yellow color and are equally fragile. The minerals of the sand filling the alveoli were analyzed for clues to the history of the jaw previous to its deposit in the Rock Bench, but no minerals surely diagnostic of either horizon were obtained.

The jaw is slightly smaller than any specimens of *E. gratus*, but the differences of dimensions and shape, as far as they can be compared, are specific or less in value. What was at first believed to be the alveolus of P_3 turned out to be, upon more complete investigation, a canal ascending near the front of the alveolus of P_4 . It branches just beneath the surface of the bone, one division bending sharply forward and the other continuing but constricting to make a small hole in front of the anterior socket of P_4 .

The worn incisor has a maximum dimension of 3.6 mm. and a minimum of 1.8 mm., with a maximum-minimum ratio of 2.0, which is smaller than the corresponding ratio of any other *Eucosmodon*. The specimen is close to the size of *E. jepseni* from the Lebo, but too fragmentary to be assigned to that species with conviction. No other specimens in the Rock Bench collection pertain to this genus.

Silver Coulee Multituberculates

A greater number of specimens of multituberculates has been collected from the Silver Coulee quarry than from any other level in the Polecat Bench section, and they represent more genera than are known from any other horizon. All specimens except two are included in the following categories:

Ptilodontinae

Ectypodus laytoni, new species

Ectypodus hazeni, new species

Ectypodus powelli, new species

Prochetodon cavus, new genus and species

Mimetodon churchilli, new genus and species

Eucosmodontinae

Microcosmodon conus Jepsen, 1930.

Pentacosmodon pronus, new genus and species

One of the exceptions noted above, no. 14466, represents a different species and, probably, genus, being much larger than any of those listed. It consists of two upper premolars whose discovery under extraordinary circumstances led to the development of the Princeton Quarry. The other specimen, no. 14465, which is not included in any of the species to be described, consists likewise of two upper premolars which may belong to *Prochetodon cavus* but are smaller than the remarkably uniform lot described in that species.

Another pleasing feature of this collection is the association of lower and upper teeth, definite in the case of *Ectypodus laytoni* and strongly suggested by measurement, structure, and proximity in *E. hazeni*. The collocation of lowers and uppers of *Prochetodon cavus* is extremely probable for a number of reasons, to be explained in their description.

Ectypodus laytoni and *powelli*, although they are two sharply distinct groups, may represent the sexes of a single species, but there is insufficient evidence to establish such a theory. They are

represented by about equal numbers of individuals. They are similar in many respects but their ecological compatibility or incompatibility cannot be conjectured, and they are therefore treated as separate species, defined by structure. *E. hazeni* is a third species in the genus, all from a single quarry, contrary to the Law of Ecological Incompatibility. It is clearly a distinct species, being twice the size of *laytoni* or *powelli*, and has the diagnostic structures of the genus.

Unless otherwise indicated, all of the Silver Coulee multituberculates have been collected from the "Princeton Quarry," sec 21, T. 57 N., R. 100 W.

Subfamily PTILODONTINÆ

Genus ECTYPODUS Matthew and Granger, 1921

ECTYPODUS LAYTONI,⁹ new species

(Pl. III, Figs 2 and 3)



FIG 16 Lower jaw of *Ectypodus laytoni*, approximately natural size

Type.—Princeton 14464, crushed maxillary part of skull with right premolars and left cheek teeth, left lower jaw with all teeth.

Referred Specimens.—Princeton 14162, right lower jaw with premolars and M_1 , 14334, left lower jaw with P_4 and M_1 ; 14338, right lower jaw with premolars and M_2 , 14335, left lower jaw with P_4 , 14337, left lower jaw with P_3 ; 14078, right lower jaw with P_4 , 14424, right P_3 and P_4 .

Distribution.—Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Generic Characters.— P^4 trenchant, with one high, arched shearing row of conjoined cuspules and a few (4 or 5) small antero-external cusps. P_4 low, higher in front than rear, with 3 or 4 asymmetrically spaced divergent ridges from the first serration.

Specific Characters.— P_3 present. Five outer, nine inner cusps on P^4 , no posterior basal cusp on P^4 , M^1 shorter than P^4 ; cusp formula of M_1 , 7.4, of M_2 , 3.2, smaller than other described species. Dimensions and ratios given in Tables 7 and 8.

Other Diagnostic Characters.— M^2 longer than wide. Four cusps

⁹ To recognize further the value of the work of Robert Layton in the Princeton Quarry.

on P². Lower incisor crown covered with enamel and more acutely curved than root

Mandible—The slender lower jaw has several differences in shape and proportion from other species of *Ectypodus*, but is, in general, very similar to all of them. Several of the extremely varied group of specimens from the Lebo, which Simpson collectively called *Phlodus? sinclairi*, and which are herein referred, with considerable doubt, to *Ectypodus?* are similar in size and certain structural characteristics to *E. laytoni*, but all of them differ specifically from the latter. Upper teeth of a size small enough to accompany the lower jaws of the mixed group *E? sinclairi* have not been found. The posterior external cusps on some of the lower molars of this latter indefinite species are solidly conjoined and on other specimens the P₄ has a unique shape. There is little to be gained, of benefit to the present study, in a detailed account of the numerous novel structural features in the group.

The mental foramen in *Ectypodus laytoni* is beneath P₃, and the comma-shaped symphysial scar is unusually distinct, protruding slightly from the adjacent bone. Apparently high, the coronoid process is also broad from front to rear, and the masseteric fossa is deep, as is the pterygoid fossa. Seen from below, the lower border of the jaw is somewhat hourglass-shaped, being constricted below M₁, slightly expanded in the region of the incisor, and broadly flared in the area of the masseteric and pterygoid fossæ. The dental foramen is in the anterior inferior corner of the latter, which does not continue forward under M₁, as it does in *Eucosmodon*.

Lower Teeth.—The incisor, unusually slender and acutely curved in the crown area, has an especially broad and deep internal concavity, giving a kidney shape to a cross section of the tooth in the mid-region of the crown. The closed root terminates beneath the middle of P₄. On the type, the lower incisor has a maximum diameter of 1.0 mm. and a minimum of 0.7 mm.

P₃ is very small in proportion to P₄, being a simple peg with enamel covering its crown top and, in some specimens, extending down the sides nearly to the alveolus. The top is concave on a couple of specimens and has one or two minute cuspules. The tooth averages about .7 mm. high and .2 mm. wide.

P₄ on most specimens has 12 serrations, one has 13, and one 14, (in sharp contrast to the uniform presence of 10 in *E. powelli*). The anterior face of P₄ of *E. laytoni* is very broad at the base,

slightly convex anteriorly, and has one strong ridge on each side of the front edge, leading to the first serration. On most specimens this anterior serration leans forward, making the anterior border concave just below it, when viewed from the side. The anterior basal concavity is broad and enamel lined. On the external side of the last serration a small ridge descends for a short distance and then turns forward, assuming a crescent shape. The highest part of the tooth is in the region of the middle serrations, those behind this region being in a straight line, and not bowed as they are in *Phylodus*.

M₁ has seven cusps in the external row, the anterior two being connected, and four in the internal row, the last one long and ridge-like. There is a small bulb on the front slope of the anterior cusp of this row, but it is not distinct enough to be a true cusp. The two rows diverge posteriorly. The cusps are crowded and simple but appear to be bent backward because the posterior surfaces are concave. There is a single valley on the labial slope of each of the anterior three cusps of the inner row, and three similar grooves on the last cusp. The M₁ cusp formula of *E. powelli* is 8 4.

M₂ has three external and two internal cusps, the anterior cusp of the inner row being the largest. In addition to the longitudinal valley between the cusp rows, a deep wing-shaped groove traverses the tooth laterally between the first cusp of each row and those posterior to them. The third cusp of the outer row has a deep groove on its lingual slope. *E. powelli* has four external cusps on M₂.

Upper Teeth —P¹, rounded triangle in shape, has three cusps and two roots. The single anterior cusp and the external cusp of the posterior pair are made bladelike by lateral ridges.

P² has four cusps arranged at the corners of a square, and an irregular oval outline. The anterior edge overlaps the posterior slope of P¹. The anterior external cusp is smaller than the others. Anterior and posterior ridges make small blades of the external cusps. There are two roots.

P³ overlaps P² as the latter does P¹ (Pl. III, Fig. 2). This tooth is longer and narrower than P², but likewise has four cusps and two roots. The posterior lingual cusp is a little larger than the other three. The cusps are not near the anterior or posterior borders but at the corners of a square which occupies scarcely more than the middle third of the tooth. The posterior third is a plane platform

sloping rearward under P^4 . Between cusps the enamel is plane, but there is an anterior marginal ridge. Enamel extends down over the anterior edge into the under surface of the shelf that overhangs P^2 , and the anterior face of the front root is eccentric, similar to the peculiarity noted in the description of M_1 of *Mesodma ambigua*.

P^4 , the most diagnostic tooth of the genus *Ectypodus*, shows differences, in *E. laytoni*, from other species. It has a single shearing blade with nine serrations or cusps along its edge, and there is an external row of five small basal cusps, the fourth being the largest of the series. In this outer row, the first three cusps are opposite, or at the base of, the first three cusps of the main row, but the fourth (and largest) and the fifth of these small cusps are at the bases of the fifth and sixth, respectively, of the shear row. This main blade rises from front to rear, but the posterior four cusps are of about the same height, and when viewed from the side the blade is gently arched, not as straight and uniformly increasing in height from front to back as in *E. musculus*. *E. musculus*, *hunteri*, *cochranensis*, and, probably, *hazeni*, have a posterior basal cusp and ledge on P^4 . There is no evidence of such a cusp on the unworn surfaces of the P^4 s of the type specimen of *laytoni*. Viewed laterally, P^4 of *laytoni* has a flatter and less regular triangular outline than *musculus*. Its possession of five cusps in the external row is a sharp contrast to the one or two in *musculus*. Presumably this tooth was the first of the upper series to erupt because its front and rear edges overlap the adjacent parts of P^3 and M^1 respectively, as seen in Fig 3, Pl. III.

M^1 has two complete rows of nine cusps each and an internal row of five cusps which are progressively larger posteriorly. This inner row is incomplete anteriorly, its most anterior cusp being opposite the fourth cusp of the median row. The largest cusps are in the middle row, and the posterior half of each cusp, except the last, is conical, whereas the anterior face of each is concave because of the crescent-shaped intercusp grooves. This construction makes the cusp tips appear to be bent forward whereas the cusps of the lower molars have a backward attitude. The crown surface of M^1 is concave from front to rear.

M^2 is widest at its anterior part and narrows posteriorly. Its three rows of cusps are in line with those of M^1 . The external row is hardly more than a low ridge on which the three cusps are distinguishable partly because their enamel coat is translucent and

the dentine cones can be seen under it. From the posterior of these cusps a ridge extends backward and then curves lingually to ascend the labial slope of the posterior cusp of the middle row. In this central row of three cusps the middle one is the largest and is closely connected to the front cusp which is small and laterally extended. Three subequal cusps compose the inner row. Several small grooves on the cusp slopes give the whole tooth a complicated pattern, and there is a small lump or cuspule at the antero-labial base of the middle cusp of the central row.

The zygomatic arch arises opposite P^3 and P^4 , and the infra-orbital foramen is above the anterior part of P^4 .

Discussion —It was hoped that the completeness of information about the teeth, derivable from the several specimens of this species, the smallest of the genus, would help clarify many of the doubtful features about the related genotype *Ectypodus musculus*, such as the presence or absence of P_3 in the latter.

TABLE 7
MEASUREMENTS OF LOWER TEETH OF *Ectypodus laytoni*

Specimens	Serrations	P_1		M_1				$\frac{LP_1}{LM_1}$	M_2		
		L	W	Cusps	L	W	$\frac{L}{W}$		Cusps	L	W
Means	12.4	3.1	1.2	7.4	1.7	0.8	2.1	1.8	3.2	1.0	1.0
14464	13	3.0	1.2	7.4	1.7	0.8	2.1	1.8	3.2	1.0	1.0
14162	14	3.0	1.2	7.4	1.7	0.8	2.1	1.8			
14334	12	3.1	1.1	7.4	1.7	0.8	2.1				
14338	12	3.2	1.2						3.2	1.0	1.0
14078	12	3.2	1.3								
14335	12	3.1									
14424	12	3.1	1.2								

TABLE 8
MEASUREMENTS OF UPPER TEETH OF *Ectypodus laytoni*, No 14464, Type

Side	P ¹			P ²			P ³			P ⁴			M ¹			M ²				
	Cusps	L	W	Cusps	L	W	Cusps	L	W	Cusps	L	W	Cusps	L	W	Cusps	L	W		
Left	3	10	08	4	09	09	4	11	07	59	26	09	99	5	23	11	33	3	12	11
Right	3	10	08	4	09	09	4	11	07	59	26	09								

The type of *E. laytoni* has associated upper and lower dentitions, and lacks only the upper incisors. The designated type of *E. musculus* is Am Mus. no 17373, an "upper jaw," and must therefore be the principle basis for comparison, even though there is no doubt that the "topotypes" of Matthew and Granger, consisting of "a series of seven upper and lower jaws and many isolated teeth and jaw fragments," all pertain to this single species.

Parectypodus was proposed (Jepsen, 1930a, pp. 120-122) for two species based upon lower jaws and teeth from the Gray Bull. These species, *simpsoni* and *tardus*, appeared to be distinctive enough, and far enough separated from *Ectypodus* in time, to represent a different genus. The absence of P_3 in this genus was used as part of the diagnosis along with several other features, but now the species of *Parectypodus* are transferred to *Ectypodus*,¹⁰ as explained above.

The discovery of associated upper and lower teeth in the Silver Coulee collection permits a direct comparison with the type Tiffany material. Using the designated type upper teeth, especially the diagnostic P^4 , of *Ectypodus musculus* for generic criteria, there can be no doubt that this genus is abundantly represented in the Silver Coulee by two species, and perhaps three. All of the lower jaws of the group of the smallest species, *laytoni*, in which the P_3 region is preserved, clearly show the presence of this tooth. It is in place on four specimens and its alveolus large and obvious on four others. The larger species, *Ectypodus hazeni*, is founded upon an upper dentition with the diagnostic P^4 , and several lower jaws, all having P_3 , are assigned to this species. Another group of lower jaws, having no positive association with upper teeth, lack P_3 . They are assigned to the new species *powelli*. Five specimens show the absence of this tooth.

The *Ectypodus* type of P^4 is certainly associated with lower jaws having P_3 in *hunteri* and *laytoni*, and the association is highly probable in *hazeni*. The upper P^4 is unknown in *tardus*, *simpsoni*, and *powelli*, and in the species questionably assigned to the genus, *grangeri*, *silberlingi*, and *sinclauri*. Thus, the genotypic species, *musculus*, is the only one wherein the absence of P_3 is known to be associated with the typical P^4 .

¹⁰ The types of *Ectypodus simpsoni* and *E. tardus* are the last known survivors of the allotheres, having been found in Gray Bull strata at a level considerably higher than *Neohotomus*. Granger and Simpson (1928, p. 2) placed this later species in *Eucoemodon*, but it was transferred later to *Neohotomus* (Jepsen, 1930a, p. 123-4).

The presence of P_3 in the group of specimens here described as *Ectypodus laytoni* is associated with 12, 13, or 14 serrations on P_4 . P_4 of the Silver Coulee group lacking P_3 invariably has 10 serrations although the dimensions of P_4 are highly variable. Each member of each of these two groups also has many additional consistent differences from any specimen of the other group.

Another test which this Silver Coulee *Ectypodus* material underwent was a lengthy analysis in comparison with all other known species of the genus to see if there might be among their characteristics any basis or indication for estimating the relative ages of the respective deposits from which the specimens were collected. The localities and rocks suspected of being approximately contemporary with the Silver Coulee are (1) Paskapoo (in part), Alberta, (2) Tiffany, Colorado, (3) Bear Creek, Montana, and (4) Melville, Montana. The third, Bear Creek, has not yielded any multituberculates, but the other three have each produced a species of *Ectypodus*. If the series of species (omitting *E. powelli*, which is not certainly represented by upper teeth, and *E. hazeni* which is much larger) is arranged in the order—*laytoni*, *cochranensis*, *hunteri*, and *musculus*, the following measurements, counts, and ratios are, as far as known, in decreasing or increasing order, cusps of P^4 , median cusps M^1 , internal cusps M^1 , cusps M^2 , length M^1 , width M^1 , ratio length M_1 width M_1 , ratio length P_4 length M_1 , ratio length P^4 : length M^1 , ratio length M^2 width M^2 , and cusps of M_1 , as shown in Table 9. Another arrangement order, *laytoni*, *musculus*, *cochranensis*, and *hunteri*, seen in Table 10, places the following measurements and ratios in increasing order;—length P^4 , ratio length P_4 : length P^4 , length and width M_1 , M_2 and M^2 , and the serrations of P_4 . Other arrangements of increasing and decreasing measures assort the species in diverse orders. Summarizing the results of the observations listed above, there are nine counts, two dimensions, and four ratios which consistently increase, decrease, or show no change from species to species when they are arranged in the order *laytoni*, *cochranensis*, *hunteri*, and *musculus*. The second order indicated, *laytoni*, *musculus*, *cochranensis*, and *hunteri*, shows consistent change in one count, seven dimensions, and one ratio.

Assuming that these arrangements might have significance and that the time difference between the ages of the deposits was sufficient to have allowed temporal variation to affect the populations

TABLE 9

COMPARISON OF TEETH OF SMALL SPECIES OF *Ectypodus* * ARRANGEMENT A

Species	Locality	Cuaps P ⁴	Median Cuaps M ¹	Internal Cuaps M ²	Cuaps M ³	L M ¹	W M ¹	$\frac{L}{W}$ $\frac{M_1}{M_1}$	Cuaps M ₁	$\frac{L}{L}$ $\frac{P_1}{M_1}$	$\frac{L}{L}$ $\frac{P_1}{M_1}$	$\frac{L}{L}$ $\frac{P_1}{M_1}$	$\frac{W}{L}$ $\frac{M_1}{M_1}$
<i>laytoni</i>	Silver Coulee	5 9	9	5	3 3 3	2 3	1 1	2 1	7 4	1 8	1 13	1 09	
<i>cochraneensis</i>	Paskapoo	2+ 8	?	5	1 3 3	?	1 4	?	?	?	?	1 00	
<i>hunters</i>	Melville	1-2 8-9	11	8	1 3 4	3 05	1 4	2 25	8 4	1 8	1 0	1 00	
<i>musculus</i>	Tiffany	1-2 7-8	13	10	1 3 4	3 1	1 4	2 5	8 6	1 6	0 87	0 92	

TABLE 10

COMPARISON OF TEETH OF SMALL SPECIES OF *Ectypodus* * ARRANGEMENT B

Species	L P ⁴	$\frac{L P_1}{L P_4}$	L M ₁	W M ₁	L M ₂	W M ₂	Serra- tions P ₁	L M ²	W M ²
<i>laytoni</i>	2 6	1 2	1 7	0 8	1 0	1 0	12-14	1 2	1 1
<i>musculus</i>	2 7	1 5-	2 5	1 0	1 4	1 1	13	1 2	1 3
<i>cochraneensis</i>	2 8	1 5+	?	?	?	?	14-15	1 6	1 6
<i>hunters</i>	3 0	1 75	2 6	1 3	1 5	1 3	14-16	1 6	1 6

* *E. powelli* is omitted because its P⁴ is unknown

of all the areas, either by genetic creep or by migration, it would be interesting to see which of the orders indicated above is the more likely and which end of this arrangement is the older. A long tedious analysis of the various counts and dimensions of the specimens assigned to species of *Ectypodus* that are younger and older than those from the Tiffany and its approximate equivalents resulted in nothing except the above noted conclusion that the genus is composed of too many diverse and numerous elements for any satisfactory analyses of this nature.

It may appear, however, from this attempted analysis that counts, structural features, constitute more secure bases for calculating order in time than do dimensions. If this is so and, as Simpson suggests, the Scarritt Quarry fauna of the Melville is "at least as old as the Tiffany and possibly older, although this is uncertain," order A, above in Table 9, has more evidence in its favor than arrangement B in Table 10. The propriety of this arrangement will be strengthened or weakened by similar analyses of species of other genera common to the Silver Coulee, Paskapoo, Melville, and Tiffany.

ECTYPODUS HAZENI,¹¹ new species

(Pl. IV, Figs. 1-3)

FIG 17 Lower jaw of *Ectypodus hazeni*, approximately natural size

Type—No 14432, right maxillary fragment with P^4 , M^1 , M^2 , and part of P^3 .

Referred Specimens—Nos. 14323, almost complete right lower jaw, lacking M_2 , 14422, fragment of left lower jaw containing P_3 , P_4 , and M_2 , 14340, left P^4

Distribution.—Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Generic Characters— P^4 have diagnostic characters of genotypic species, *E. musculus*, as listed above for *E. laytoni*

Specific Characters— P_3 present, 3-4 outer, 9 inner cusps on P^4 . Internal surface of lower incisor comparatively plane, cusp formula of M_1 , 9 6-7; of M_2 , 5 3 Dimensions and ratios given in Tables 11 and 12.

Discussion—Association of lower teeth and jaws with the upper teeth of this largest species of the genus *Ectypodus* is, as in many species of allotheres, not by articulated occurrence in the field, but by laboratory analysis, comparisons with other species where the lower and upper dentitions were found articulated or positively associated, and by comparison of ratios. The type maxilla, no. 14432, and the referred lower jaw, no. 14323, were found close together, but this proximity merely suggests the propriety of putting them in the same species, the more forceful evidence comes from the structural and dimensional analyses. Future discoveries may correctly assort the upper and lower dental elements if they are incorrectly associated here. The ratio of length P_4 P^4 is unusually low, for a ptilotontid, in both this species and *laytoni*, being 1.24 and 1.19, respectively, whereas the ratio in *musculus* is 1.48, 1.5 in *hunteri*, and 1.75 in *cochranensis*.

Mandible.—The lower jaw has a heavy and powerful appearance, especially beneath P_4 , and is extremely short anteriorly, in

¹¹ In honor of Burchard Miller Hazen, gifted collector

considerable contrast to species of other genera such as *Phlodus*. Below P_4 , on the external surface, the bone is rugose. The coronoid process arises farther forward than it does in *Ectypodus laytoni*. It is high, and long from front to rear. The internal and external muscle fossæ are deep and long, there is no angular process, and the posterior inferior border is angular when seen from the side, not gently rounded as it is in species of *Phlodus*. Another clear distinction from the latter is the greater size of the condyle. It is extremely wide and gently curved, having a lateral width almost as great as the length of M_1 . The entire posterior region of the jaw differs notably from that of *Phlodus*, being heavier and of contrasting outline, more angular, less curved. These differences imply corresponding or even more contrasting dissimilarities of skull structure. The small mental foramen penetrates the bone in front of P_3 .

Lower Teeth —The incisor is long, heavy, and acutely curved. Its internal side is plane, almost lacking the usual longitudinal concavity in ptilodontines, nor is there a ridge on the superior surface. No 14323 has very rugose enamel on the lower external side. The closed root ends beneath M_2 .

P_3 is a comparatively stouter tooth than that of *laytoni*. Enamel coats its tip and it has a few indistinct ridges on it.

P_4 , with its twelve or thirteen serrations and lateral ridges, is low, with its apex in the vicinity of the middle serrations, the posterior six or seven being in a straight line which declines gently toward M_1 . In the customary study orientation of M_1 with its cusps on a horizontal line, the crest of P_4 does not project above the forward continuation of this line. This method of orientation and comparison of P_4 is apparently of some value in generic analyses.

M_1 has 9 external and 6 or 7 internal cusps, the anterior one in the external row and the posterior one in the interior row being very small. The only example of this tooth, on specimen no. 14323, is much worn, but the cusps are as crowded as they are on M_1 of the typical *Ectypodus musculus*, and those of the external row show similar dimples or depressions on their external slopes.

M_3 , preserved on only one specimen, has five external and three internal cusps, the former being united into a ridge. Between the two rows the valley is deep and V-shaped, and several grooves on each wall produce a herringbone pattern with the apices of the chevrons pointing rearward.

Upper Teeth —Only the posterior part of P^3 is preserved, and from this little can be determined except that the tooth has a much shorter posterior shelf under P^4 than it does in *laytoni* and that the posterior cusps are comparatively high.

P^4 has three to four external and nine internal cusps and two roots. The cusps of the shorter row increase in size posteriorly and occupy about the anterior third of the length of the root, being much shorter than in *laytoni*. In the main row, which constitutes the shearing blade, the last cusp is the highest and is set in from the posterior border, as in *musculus*. Both ends of the crown overhang the roots, and enamel extends down around these terminal shelves, the anterior one forming a basal concavity somewhat like that of P_4 . From a crown view, the whole tooth is of elongate kidney shape (see Fig 3, Pl. IV). The serrated blade has a slight convexity externally. It begins near the anterior inner border of the crown and is directed diagonally toward the posteroexternal margin. As in *musculus*, the outer face is slightly concave while the inner is nearly plane.

Ectypodus musculus has, on unworn P^4 's, a prominent posterior basal cusp although this has not been mentioned heretofore in the descriptions of the specimens. A similar cusp is present in *E. cochranensis*, as observed by Russell (1932, p. 50), and can be seen also on unworn specimens of *E. hunteri*, although Simpson (1936c, pp 6-7) did not note its presence in the type descriptions of the latter. *E. hazeni* also may have had this posterobasal cusp. The area of its position is worn on the specimens but part of a cingulum-like expansion, similar to that on the other species, remains to indicate its presence.

M^1 is worn on the type, but has a cusp formula of 10:12.6. Of the ten outer cusps, the first and eighth are larger than the others, and the ninth and tenth are conjoined. The anterior slope of the first is modified by a transverse roll of enamel with a slight tubercle at its summit. Several small cuspules complicate the pattern on the lingual slopes of the anterior cusps of this row. Anteriorly the cusps of the middle row are reduced by wear to a low ridge dotted by craters worn through the enamel into the dentine. Each cusp has the remnants of a valley on each side. The tips of the posterior cusps direct forward. The inner row occupies the posterior three-quarters of the lingual border of the tooth. Although the anterior

cusp is a long cuspidate ridge, the other five cusps are separate and distinct. The whole tooth looks as if it had been twisted by pulling the anteroexternal corner away from the alveolar border, the external row is strongly concave from front to rear, the middle row is less hollowed, and the inner row is convex

M², heart-shaped, narrows posteriorly. Its concave anterior edge underlaps the posterior border of M¹. The external cusp row is composed of one distinct anterior cusp and a long ridge extending posteriorly with six minute bulges on it, like a low shelf along a little more than half the length of the tooth. The three cusps in the median row have convex posterior and concave anterior slopes, and the posterior cusp is the largest. A ridge ascends the antero-lingual slope of the anterior cusp and the anterolabial surface has a small cuspule upon it. The posterior cusp of the four in the internal row is long and ridge-like with several small enlargements upon it

Skull.—From the small fragment of skull it can be said that the anterior pedicle of the zygomatic arch arises opposite P³ and P⁴, slightly farther forward than in *laytoni*.

TABLE 11
MEASUREMENTS OF UPPER TEETH OF *Ectypodus hazeni*

Specimens	P ⁴			M ¹			M ²		
	Cusps	L	W	Cusps	L	W	Cusps	L	W
14432	4 9	4 2	1 6	10 12 6	4 4	2 0	1 3 4	2 1	1 8
14340	3 9	4 2	1 6						

TABLE 12
MEASUREMENTS OF LOWER TEETH OF *Ectypodus hazeni*

	I		P ₁			M ₁				$\frac{LP_1}{LM_1}$	M ₂			$\frac{LM_1}{LM_2}$
	Max	Min	Serra- tions	L	W	Cusps	L	W	$\frac{L}{W}$		Cusps	L	W	
14323	2 6	1 8	13	5 1	1 8	8 6	3 2	1 4	2 3	1 7				
14422			13	5 0	1 8						5.3	2 2	1 7	1 5

ECTYPODUS POWELLI,¹² new species

(Pl. III, Figs. 1, 1a)

FIG 18 Lower jaw of *Ectypodus powelli*, approximately natural size

Type—Princeton no 13979, left lower jaw with cheek-teeth.

Referred Specimens—Princeton nos 13836, left lower jaw with incisor and P_4 , 14425, left lower jaw with P_4 and broken incisor, 14339, left lower jaw with P_4 and incisor, 13319, left P_4 in jaw fragment, 14423, left P_4 in jaw fragment, 14463, right M^1 .

Distribution—Silver Coulee beds, Polecat Bench formation, Park County, Wyoming

Generic Characters—As listed above for P_4 of *Ectypodus laytoni*.

Specific Characters—10 serrations on P_4 . M_1 comparatively short and wide, length width ratio 1.8. Dimensions and other ratios indicated in Table 13.

Other Diagnostic Characters— P_3 absent. Anterior basal concavity of P_4 small, but larger than in Eocene species of the genus.

Discussion—This species cannot be adequately defined until specimens of associated upper and lower dentitions are found. The absence of P_3 may appear to place the group closer to the Tiffany *E. musculus* and the two species from the Eocene, *tardus* and *simpsoni*, than to the species which have P_3 , *hunteri* from the Melville and *hazeni* and *laytoni* from the Silver Coulee. The older species which are questionably assigned to the genus, *E? grangeri*, *silberlingi*, and *sinclairi*, all possess P_3 .

Mandible.—The differences in the lower jaw of this species and that of *Ectypodus laytoni* appear to be conditioned in part by the presence of P_3 in *laytoni* and its absence in *powelli*. The bone collar around the incisor is deeper in *powelli* but lacks the external expansion which accommodates the root of P_3 in *laytoni*. On the superior surface of the bone between the incisor and P_4 of *powelli* there is a distinct sharp-edged ridge or keel. The corresponding area in *laytoni* is smoothly rounded.

Lower Teeth.—In lacking P_3 and in the accompanying reduction of the anterior basal concavity of P_4 , *powelli* resembles *musculus*. The general shape of P_4 is similar in the two species, being low but

¹² From the town of Powell, Wyoming, whose citizens have materially aided work in the Princeton Quarry

considerably higher in front than toward the rear. P_4 is higher and less robust in *laytoni*. The latter species has 12 to 14 serrations on P_4 , whereas *powelli* has but 10. The lower molars of *powelli* are larger in all dimensions than those of *laytoni*. The cusp formula of M_1 of *powelli* is 8 4, and the length-width ratio is 1.8. M_2 is the same width as M_1 .

Upper Teeth — A single right M^1 (Princeton no. 14463) may pertain to *powelli*. It is heavier, wider, and longer than M^1 of *laytoni*. The anterior edge is slightly concave, not rounded as it is in *laytoni*. There are seven cusps in each of the three rows, although the inner row is but a short cuspidate ridge, as compared with the 9.9.5 formula of *laytoni*. M^1 of the latter species has two roots, the anterior one occupying more than one half the subcrown length. No. 14463 has two large roots, one at each end of the crown, and four smaller roots between them, in two pairs. The tooth is 1.4 mm wide and twice as long.

TABLE 13
MEASUREMENTS OF LOWER TEETH OF *Ectypodus powelli*

Specimens	I		P_4			M_1				$\frac{L P_4}{L M_1}$	M_2			$\frac{L M_2}{L M_1}$
	Max	Min	Serrations	L	W	Cusps	L	W	$\frac{L}{W}$		Cusps	L	W	
Means	1.1	0.87	10	3.2	1.3	8.4	2.0	1.1	1.8	1.7	4.2	1.3	1.1	1.5
13979 (Type)			10	3.4	1.4	8.4	2.0	1.1	1.8	1.7	4.2	1.3	1.1	1.5
13836	1.2	0.9	10	3.0	1.1									
14425	1.1	0.9	10	3.2	1.4									
14339	1.1	0.8												
13319			10	3.4	1.3									
14423			10	3.0	1.1									

PROCHETODON,¹³ new genus

Type — *Prochetodon cavus*, new species.

Distribution — Silver Coulee beds, Polecat Bench formation, Park County, Wyoming, Gray Bull beds.

Diagnosis — P_4 with groove on anterior edge above the large basal concavity. Eight cusps on P^3 . P^4 with one complete row of cusps and an incomplete external row. Extremely long, slender, lower incisor. Other generic and specific characters indicated below.

¹³ In reference to the groove on the anterior surface of P_4 .

PROCHETODON CAVUS,¹⁴ new species

(Pl. IV, Figs. 4-7)

FIG 19 Lower jaw of *Prochetodon cavus*, approximately natural size

Ptilodus sp Jepsen, 1930b, p 510, Pl IX, Fig. 6 (Princeton no 13362)

Type — Princeton no 13925, right lower jaw with incisor, P₄, broken P₄, and anterior root of M₁.

Referred Specimens.—Princeton nos 13362, right P₄, 14436, right P₄, 14527, left P₄, 14034, right maxilla with P¹⁻³ and alveolus of P⁴, 14434, right P¹⁻²; 14336, left maxillary fragment with P²⁻³, 14433, right P³ in bone fragment, 14435, left P⁴.

Distribution —Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Discussion —It is impracticable to distinguish generic from specific characters in this single species of *Prochetodon*, but the genus is apparently related to *Ptilodus* and to *Neohotomus* more closely than to other genera. One tooth of this new species was at first referred to *Ptilodus* sp. (Jepsen, 1930b, p. 510), an error which became obvious only with the discovery and study of better specimens and the probable collocation of lower and upper teeth. The specimens were extensively dissected to make certain that the teeth of *Prochetodon cavus* were not deciduous or merely bizarre variants of descendants of some species of the geologically older *Ptilodus*. Recently a P⁴ of *Ptilodus* has been found in a collection from the Melville formation of the Crazy Mountain field in Montana. This tooth, of nearly the same geological age as *Prochetodon cavus*, clearly shows that *Ptilodus* continued with but little change from the Lebo to the Melville, and that the trend in the genus was away from, rather than toward, the structure of *Prochetodon*. The unclassified ptilodontid material which Simpson recorded from the Melville (1936c, p. 8, 1937c, p. 3) may belong to the same group as the recently discovered P⁴. The new species surely represents a distinct genus, and when more complete specimens are found they will undoubtedly emphasize the isolation of the group.

¹⁴ To indicate the large anterior basal concavity of P₄.

Mandible —The incisor- P_3 diastema of *Prochetodon cavus* is very short as compared with species of *Ptilodus*, and this region is comparatively heavy. A large mental foramen appears high on the external side of the collar surrounding the incisor, midway between it and P_3 . Above this, on the superior surface of the bone in the diastema, is another foramen. Although the posterior part of the jaw is missing, the anterior border of the pterygoid fossa remains and shows that the fossa was very deep, with the dental foramen not at the bottom, as it is in *Ptilodus*, but a short distance above the floor of the fossa. The entire jaw must have been deep and abbreviated at least anterior to the pterygoid fossa. Near the alveolar border on the outside of the jaw between the anterior and the posterior roots of P_4 there are numerous small holes.

Lower Teeth —The incisor is so extremely long and slender, being more so than that of any other known allothere, that it was believed to have slipped part way out of the alveolus, even though the wear on it near its alveolus contradicted this. The root was excavated and seen to be, if anything, unusually closely invested by bone. Next the idea was entertained that the tooth might be deciduous, and that its permanent successor might erupt from a socket at least partly independent of this one. The excavation revealed no sign of a successor, but showed an extraordinarily large chamber behind the root of the incisor, below the roots of the cheek teeth, and in front of the anterior wall of the pterygoid fossa. The jaws of other species were then excavated, and all of them show a similar but smaller vacuity. The incisor crown was apparently covered with enamel, but in both specimens in the collection wear has exposed the dentine in a long streak along the upper half of the inner side. A V-shaped area, with its apex forward, is apparently worn bare of enamel on the outer side of the incisor of the type, as shown on Fig. 4, Pl. IV. Whether this is due to typical structure of the upper anterior teeth, or to individual malformation, is not known. There is a ridge along the inferior inner side of the incisor. The enamel ends a short distance inside the alveolus. In section the tooth is much more compressed laterally than lower incisors of *Ptilodus*.

P_3 is of characteristic ptilodont shape and proportions, has thin enamel and two or more diminutive cusps on its crown.

P_4 is distinct among ptilodonts, having an anterior border different from other known species, in that a prominent but shallow

groove or trough begins a short distance above the anterior basal concavity and continues its ascent almost to the first serration, looking as though a rounded trowel had been drawn up over the front edge when it was in a plastic state. Midway along its extent, a ridge appears in the center of this groove and leads to the first serration. This unique concavity was at first laid to the effect of wear, but when it was found on all of the teeth, some of which show no signs of wear, it had to be accepted as a structural feature of importance. The anterior basal concavity is much broader than that of *Phylodus*, and when seen from in front, the middle of the overhanging canopy descends forward over the top of the crown of P_3 . P_4 has twelve or thirteen serrations, and is set in the jaw at an angle, which accounts for the foreshortening¹⁵ that gives it a deceptive appearance of being very high and short in Fig 4, Pl. IV. The tooth is low for its length when compared with P_4 of *Phylodus* (Compare Fig. 5, Pl. IV, with Fig 1a, Pl. II.)

The various peculiarities of the lower jaw and teeth indicated that there were corresponding or greater novelties in the upper teeth. The collocation of upper and lower teeth of this species had been made first, however, on the basis of size. The only possible confusion was between this species and *Ectypodus hazeni*, but the latter is smaller. A numerical consideration supports the allocation of lower and upper dentitions to these two species. There are parts of nine lower and five upper dentitions assigned to *Prochetodon cavus*, and three lower and two upper to *Ectypodus hazeni*. It is, of course, possible, but highly improbable, that there are as many as four species represented only by lower or by upper dentitions.

Upper Dentition — P^1 is very similar to that of *Phylodus*, having three cusps arranged in a triangle. P^2 also has three cusps, like *Ectypodus musculus*, not four as does *Phylodus*. These cusps are nearly identical in structure with those of P^1 , but the tooth is oval in outline rather than triangular. This tooth overlaps the rear border of P^1 and underlaps the front edge of P^3 .

P^3 is unique in structure, and each of the three specimens at hand looks as though two single-rooted four-cusped upper premolars had been welded together. It is wider than P^2 , in contrast to the opposite situation in *Phylodus*. The eight cusps are in two straight rows of four each. Those of the outer row have anterior

¹⁵ Specimen dimensions should not be taken from the drawings of Mr. Horsfall because they so faithfully represent the actual appearance of the specimens and are not distorted to bring structures into a single plane.

and posterior ridges which convert the cusps into minute blades whose attitudes can be seen in Fig 6, Pl IV. Three radiate ridges appear on the anterior two cusps of the inner row. The crown is pinched or narrowed between the anterior and the posterior roots, and the anterior lobe is slightly wider than the posterior. Not only does this tooth have one more cusp than the maximum of any *Ptilodus* P³ known, but it is of uniform structure among the three known individuals, differing greatly in this respect from the highly variable P³ of *Ptilodus*. Six cusps on P³ is the median and norm for *P. montanus*, and four for *P. medrævus*.

The P⁴ assigned to his new genus and species is represented by only one tooth in the collection, and the evidence for its allocation is entirely presumptive, there being no association. The tooth is no 14435, left, and has been reversed in Figs. 7, 7a, Pl. IV, for comparison with Fig 6, the drawing of specimen no 14034. One of the reasons for placing this tooth with the other upper teeth herein assigned to *Prochetodon cavus* is the fact that the roots exactly conform to the unique shape of the alveoli of P⁴ in specimen no. 14034 (right maxilla). The roots of no 14435 are so nearly symmetrical on either side of a midline that they fit in the alveoli of no 14034 even though the two specimens are from opposite sides. Reversed, the roots would be exactly accommodated by the alveoli. Comparisons of the P⁴ alveoli in specimen no. 14034 with other Tertiary allothere specimens having this region preserved, show the sockets to be different from all others known. The anterior alveolus is much smaller than that for the posterior root of P⁴.

In some respects P⁴ of *Prochetodon* resembles that of *Neohotomus*, having a long low crown with a shearing cuspidate midridge slightly concave in lateral profile and with 10 serrations or cuspules. In the latter genus, however, there is, as far as known, only one additional cusp, which is large and high up on the slope of the third cusp of the main row. In *Prochetodon*, the crown is comparatively low and wide and the anterior root is considerably narrower than the crown. There are several external cusps, a large one opposite the second one of the main row, smaller ones opposite the third and the fifth, and three still more diminutive cuspules in this weak external marginal row. The posterior half of the external side of the tooth is slightly worn, and the external cuspules may have continued farther posteriorly, although this is doubtful.

In *Ectypodus* the crown of P⁴ is high, broad, and arched, and

the roots are close together, whereas P^4 of *Prochetodon* has a comparatively very low and narrow crown and widely separated roots, the posterior one having a very great anterior-posterior extent. In *Ectypodus* the anterior edge of P^4 extends well beyond the root, whereas there is only a slight projection in *Prochetodon*. These differences can be seen by comparing Figs. 3a and 7a, Pl. IV.

In summary, P^4 of *Prochetodon* narrows posteriorly as in *Philodus*, has a single main cusp now with additional antero-external cusps like *Ectypodus* and *Neoholomus*, but is very distinct from these three genera in many respects.

TABLE 14
MEASUREMENTS OF LOWER TEETH OF *Prochetodon cavius*

Specimens	I		P ₁		
	Max	Min	Serrations	L	W
Means					
13925	19	11			
13362			12	72	24
14436			12	74	25
14527			13		23

TABLE 15
MEASUREMENTS OF UPPER TEETH OF *Prochetodon cavius*

Specimens	P ¹			P ²			P ³			P ⁴		
	Cusps	L	W	Cusps	L	W	Cusps	L	W	Cusps	L	W
14034	3	22	16	3	25	18	44	37	20			
14434	3	22	16	3	25	17						
14336				3	25	18	44	37	21			
14433							44	37	22			
14435										3+	10	53 17

MIMETODON,¹⁶ new genus

Type.—*Mimetodon churchilli*, new species.

This genus also includes *Philodus trovessartianus* Cope, 1882, and *Philodus? douglassi* Simpson, 1935.

Distribution.—Middle to late Paleocene. Silver Coulee beds, Polecat Bench formation, Park County, Wyoming, Torrejon, San

¹⁶ Mimi-tooth.

Juan Basin, New Mexico, upper Lebo, Crazy Mountain field, Montana.

Diagnosis —Dental formula $\overline{1032}$ Lower incisor compressed laterally, plane internal side, crown covered with enamel P_4 of moderate height, front higher than rear, 1 ridge from first serration branches into 3 asymmetrically spaced ridges on anterior surface Cusp formula $M_1, 6.5$ M_1 comparatively long and narrow Lower jaw very deep below P_4 , posterior part long and shallow

Discussion —*Mimetodon* has been erected to receive the three species *trovessartianus*, *douglassi*, and *churchilli* after detailed comparisons among them and with other species *Phlodus trovessartianus* was founded by Cope (1882, p. 686) on three P_4 s (now A.M.N.H. no. 3025), "one of which stands on a part of the ramus .". The description was repeated and one of the three specimens was figured by Cope two years later (1884, p. 737, Pl. XXV, Fig. 19) The species was dedicated to Dr. F. L. Trouessart, but Cope and subsequent authors have spelled the name *trovessartianus* Later Cope (1885, p. 493) referred two additional lower jaws (now A.M.N.H. no. 3026) to the species, and Osborn in 1893 (p. 315, Fig. 1) figured one of these and labeled it the "type" of the species Gidley (1909, p. 614) designated one of the original three specimens (A.M.N.H. no. 3025) as the type specimen and called the other two paratypes, without reference to A.M.N.H. no. 3026. Granger and Simpson (1929, p. 631) followed Gidley in the type (no. 3025) and paratype (nos. 3025b and 3025c) references and also referred to the specimen which Osborn had figured, as the "neotype," a misuse of the term. In 1937 Matthew (p. 292) gave this designation as the type, "A. M. Cope Coll. no. 3025, three lower premolars, one on fragment of jaw, incisors, etc." Since a single specimen must be taken as the type or "holotype" of the species the designation by Gidley and Granger and Simpson of A.M.N.H. no. 3025 as the single type of the species, must stand, contrary to Matthew's later reference. This specimen is one of the three upon which Cope based the species

Granger and Simpson (1929, p. 631), and Simpson (1937a, p. 82) gave some dimensions and counts for *trovessartianus*, all of which differed from those cited by Cope (1882, 1885). The type material was therefore reexamined in this study and the conclusion of Granger and Simpson that the species is "certainly distinct from any other known" was fully corroborated. In calculating the

maximum· minimum diameters ratio of the incisor and in tabulating this and other observations, the distinctness of the species became even more clear, and when it was compared with other species, particularly those in *Phlodus*, it became obvious that *trovesarthianus*, *douglassi* and *churchilli* represent a genus apart from other *ptilodontines*. In comparison with the three species of *Phlodus*, the dimensions and counts of the species of *Mimetodon* are distinct, as shown in the Table 16. The cusp count of 6.5 on M_1 of the species of *Mimetodon* differs from the typical 6.4 of *Phlodus* and is obvious only upon unworn specimens. The diameters of the incisor of *Mimetodon douglassi* cannot be measured from the alveolus but it was obviously compressed laterally as it is in *churchilli* and *trovesarthianus*.

TABLE 16

COMPARISON OF LOWER TEETH OF SPECIES OF *Phlodus* AND *Mimetodon*

Species	I $\frac{\text{Max}}{\text{Min}}$	L P_4	L M_1	$\frac{L P_4}{L M_1}$	$\frac{L M_1}{W M_1}$	Cusps M_1	$\frac{L M_1}{L M_2}$
<i>Phlodus</i>							
<i>medius</i>		83	35	23	20	574	14
<i>montanus</i>	13	80	34	23	20	5841	14
<i>wyomingensis</i>	16	76	36	21	20	64	17
<i>Mimetodon</i>							
<i>trovesarthianus</i>	21	59	31	19	21*	65	18
<i>douglassi</i>		66	37	18	22	65	18
<i>churchilli</i>	19	51	32	16	23	65	

* The ratio of $L M_1 : W M_1$ of *trovesarthianus* is given as 1.9 by Simpson (1937a, Table 9, p. 82) but the tooth upon which this is based measures 3.1 by 1.5 mm.

Another characteristic which appears to be of generic significance is the comparatively great depth of the jaw below the middle of P_4 . This original measurement is impossible to calculate accurately for *Phlodus wyomingensis* and *Mimetodon douglassi*, due to breakage, but there is every indication that these two species conform to the generality that in species of *Mimetodon* the depth of the jaw below P_4 exceeds the length of P_4 , and in *Phlodus* the length of P_4 is the greater dimension.

P_4 of *Mimetodon* species resembles that of typical *Ectypodus*, in several respects, more than it does *Phlodus*. It is higher in front than in the rear, and the lateral ridges which converge to the first serration are not evenly spaced and parallel as they are in *Phlodus* but are asymmetrically disposed. The most prominent

ridge is directed from the first serration toward the labial side of the anterior basal concavity. One unworn specimen of *M. trovesarthianus* indicates that the front of P_4 in this species has a shallow vertical groove.

When better specimens of *trovesarthianus* and *douglassi*, from the Torrejon and the Lebo, respectively, are found, they may be separable generically from *churchilli*, the Silver Coulee species. Many structures of the three species herein assigned to *Mimetodon* are not directly comparable and therefore the opportunities for error are great.

MIMETODON CHURCHILLI,¹⁷ new species

(Pl II, Figs 6, 6a)



FIG 20 Lower jaw of *Mimetodon churchilli*, approximately natural size

Type —Princeton no 14525, left lower jaw with I, P_{2-4} , and M_1 .

Referred Specimen —Princeton no 14533, right P_4 .

Distribution —Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Specific Characters —Smaller than other species in genus. Lower incisor comparatively large. P_4 low and heavy. M_1 narrow and long. (Ratios, for comparison with other species, in Table 16.) Posteroexternal cusp on P_4 very prominent. Anteroexternal cusp of M_1 small.

Discussion.—The specimens of this species were, at the beginning of this study, confused with, and assigned to, *Ectypodus hazeni*, but *Mimetodon churchilli* is now seen to be very distinct. Most of the lower jaw peculiarities of the species and genus are apparently associated with the very large incisors. In both *Ptilodus* and *Ectypodus* a midpoint, between the superior border of the incisive alveolus and the posterior surface of the condyle, is in the vicinity of the middle of M_1 , whereas the midpoint of the lower jaw is posterior to M_1 in *Mimetodon churchilli*. This relative length of the posterior part of the jaw, or abbreviation of the front of it, is similar to the structure of the ramus of *Eucosmodon*, which likewise has a very compressed and large incisor. The mental foramen of *M.*

¹⁷ Named in honor of Mr. Fred Churchill of Powell, Wyoming.

churchilli, like that in *Eucosmodon*, is situated relatively farther above the floor of the pterygoid fossa than it is in *Ptilodus* or *Ectypodus*. The fossæ on the jaw are less deep laterally than they are in other ptilodontines. Although smaller than the huge condyle of *E. hazeni*, that of *M. churchilli* is large, wide and very gently curved.

P_2 is small and typically ptilodontine.

P_4 , with twelve serrations, is relatively straight along the posterior half of its edge.

The cusps in the external row on M_1 are sharp pointed when unworn, less rounded and blunt than on M_1 of *Ptilodus*. In most respects, the cusps of M_1 of *M. churchilli* appear similar to those of *Ptilodus*, but the proportions of length to width of M_1 in the two genera are distinct, as noted above. *M. churchilli* has a short blunt root between the two main ones of M_1 which are, themselves, comparatively slender.

The lower jaws and teeth of *Mimetodon churchilli* are so different from those of any species of *Ptilodus* or *Ectypodus* as to constitute strong evidence that the skull and upper teeth will be equally distinct or more so, when found.

TABLE 17
MEASUREMENTS OF LOWER TEETH OF *Mimetodon churchilli*

Specimen	I			P_4			M_1				$\frac{L P_1}{L M_1}$
	Max	Min	Max Min	Serra- tions	L	W	Cusps	L	W	$\frac{L}{W}$	
14525	2.8	1.5	1.9	12	5.1	1.9	6.5	3.2	1.4	2.3	1.6
14533				12	5.0	2.0					

Subfamily EUCOSMODONTINÆ

MICROCOSMODON Jepsen

Microcosmodon Jepsen, 1930b, pp. 508-510

Type.—*Microcosmodon conus*.

Distribution.—Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Diagnosis.—Generic characters unseparated from specific, described below.

MICROCOSMODON CONUS Jepsen

(Pl V, Figs. 3, 3a)

FIG 21 Lower jaw of *Microcosmodon conus*, approximately natural size

Microcosmodon conus Jepsen, 1930b, pp. 508-510, Pl. VII, Figs 3-5.

Type —Princeton no. 13331, left lower jaw with I and P_4 and alveoli of P_3 , M_{1-2} .

Referred Specimens.—Princeton nos. 14324, left lower jaw with I, P_4 , M_1 , alveoli of M_2 , and unerupted P_3 , 14430, left lower jaw with I, P_4 , M_1 , and alveoli of M_2 , 14322, right lower jaw with I, M_{1-2} , and alveoli of P_4 , 14427, left lower jaw with P_4 , M_1 , and alveoli of I, M_2 , 13401, right lower jaw fragment with P_4 - M_1 , 14549, right lower jaw with I, P_3 , P_4 , and molar alveoli.

Diagnosis —Dental formula $\overline{1023}$. Lower incisor very large, compressed laterally, enamel, posterior to tip, restricted to longitudinal band on antero-inferior surface. Maximum diameter of lower incisor greater than length of P_4 . P_4 shorter than M_1 . Five serrations on P_4 , lateral ridges weak or absent. Seven external, 5 internal cusps on M_1 , 4 2 on M_2 .

Discussion.—This is the sole known species of the genus. Some of its characters are compared with those of other species of eucosmodontines in Table 19. The diminutive group is noteworthy for the comparatively enormous size of the lower incisor, in relation to the size of the other teeth and the jaw bone. In no other ptlodontid is the length of P_4 less than the maximum diameter of the incisor. No unworn lower incisors have been found, but those representing the youngest individuals indicate that the tip of the unworn crown may have been invested with enamel. With age the enameled portion of the incisor grew out of the socket.

P_3 is highly variable both in size and attitude. On only one specimen (no. 14549) had it fully erupted, on another (no. 14324) it was revealed by dissection of the bone (see Pl. V, Fig 3), to be present but unerupted. From its position it appears to be buttressed against the anterior root of P_4 , into which it had, on other specimens, worn a small pit below the alveolar lip, perhaps an individual deformity. P_3 is not present on any other specimens, although some have a small alveolus for its accommodation. At any rate it is extremely minute, having a diameter of about 0.2

mm, seems to lack enamel, and may have been on the verge of disappearance in the group

P_4 is smaller than that of any other known Tertiary multituberculate and is comparatively simple in structure. The five little cuspules along the keel of P_4 are comparatively large for the tooth, but the lateral ridges from them are very short and indistinct. The anterior serration or cusp is lower than the other four and is well separated from them. A prominent posterolabial basal ledge supports a marginal crescentic ridge with its horns embedded in the base of the tooth. The crown slightly overhangs the anterior root, but the size of the anterior basal concavity is variable, perhaps conforming to the variability of development of P_3 . Two irregular vertical ridges modify the anterior face of the tooth. This species and *Pentacosmodon pronus*, described below, are the only Tertiary ptilodontids whose P_4 is shorter than M_1 , although in *Meniscoessus*, the Cretaceous genus, P_4 is shorter than either M_1 or M_2 .

M_1 has 7 external and 5 internal cusps, and the anterior one of each row is smaller than the others. The tooth is implanted by five roots, a large one at each end, one small root midway between them on the outer side, and two on the inner. The intercusp valleys are deep and of complicated shape. The posterior end of M_1 is much broader than the anterior

M_2 , present on only one specimen, has 4 outer and 2 inner cusps. It is wider than M_1 and has two roots. Its dimensions are listed below

The jaw is deep and heavy, as it must of necessity be to accommodate the large incisor. The species is distinguished also by its extremely deep masseteric and pterygoid fossæ

A right upper incisor, no. 14429 (Pl. V, Fig. 2), and part of a maxilla with two anterior premolars, no. 14431, may belong in this species or in *Pentacosmodon pronus*. The incisor has its two-tined crown coated with enamel which thins toward the root and divides on the lingual side and exposes a band of dentine. The tooth has a length of 5.8 mm. and its maximum and minimum diameters are 1.8 and 1.1 mm., respectively. The upper premolars are of such doubtful reference that their description seems futile. They appear to be too small, each measuring .8 mm. in length, to be associated with the upper incisor, but this unusual size relationship is also true of the lower incisor and P_4 .

TABLE 18
MEASUREMENTS OF LOWER TEETH OF *Microcosmodon conus*

Specimens	I			P ₁			M ₁			$\frac{L P_1}{L M_1}$	M ₂		
	Max	Min	Max Min	Serra- tions	L	W	Cusps	L	W		Cusps	L	W
13331 (Type)	21	10	21	5	16	07							
14430	22	10	22	5	17	07	7 4	2 5	12	0 68			
14324	21	10	21	5	17	07	7 4	2 5	12	0 68			
14322	21	10	21				7 4	2 5	12		4 2	1 5	1 3
14427				5	16	07	7 4	2 5	12	0 04			
13401				5	17	07	7 4	2 5	12	0 68			
14549	21	10	21	5	17	07							

TABLE 19
COMPARISON OF LOWER DENTITIONS OF SPECIES OF EUCOSMODONTINÆ *

	Dental Formula	I	P ₁		M ₁ Cusps	L $\frac{P_1}{M_1}$	$\frac{L P_1}{\text{Max } I}$
		Max Min	Serra- tions	L			
<i>Microcosmodon conus</i>	10222	21	5	17	7 4	67	8
<i>Pentacosmodon pronus</i>	1012	21	5	19	4 3	80	11
<i>Eucosmodon</i>	1012						
<i>gratus</i>		25	11	49	7 5	11	13
<i>americanus</i>		22	12?	106	5 4	19	18
<i>molestus</i>		22	15?	150			26
<i>jepseni</i>		27	11	43			11
<i>Neohotomus</i>	10222						
<i>conventus</i>		26		140†			22†
<i>ultimus</i>			14	114	6 4	16	16

* *Eucosmodon leithardi* is omitted because it is insufficiently known

† Approximate

PENTACOSMODON,¹⁸ new genus

Type — *Pentacosmodon pronus*, new species

Distribution — Silver Coulee beds, Polecat Bench formation, Park County, Wyoming.

Diagnosis — Generic and specific characters unseparated, discussed below.

¹⁸ Five cusps or serrations on P₁

PENTACOSMODON PRONUS,¹⁰ new species

(Pl V, Figs 1, 1a, 1b)

FIG 22 Lower jaw of *Pentacosmodon pronus*, approximately natural size

Type.—Princeton no. 14085, right lower jaw with P_4 , M_{1-2} , and broken I

Referred Specimen.—Princeton no 14426, left lower jaw with P_4 , M_{1-2} , and broken I

Diagnosis.—Dental formula $\overline{1012}$. Lower incisor large, compressed laterally, enamel apparently limited, in the typical eucosmodontine pattern, to the lower border P_4 shorter than M_1 . Five strong serrations and lateral ridges on P_4 . Few cusps on lower molars, 4 3 on M_1 , 3 2 on M_2 .

Discussion —As indicated above, this genus may be closely related to *Microcosmodon*. At the beginning of the analysis of its structure, all of the specimens of the two groups were referred to *Microcosmodon conus*, but their distinctness soon became obvious. Sexual dimorphism was the first conjectured explanation for the pairing of characters, despite the discrepancy in the recovered numbers of individuals representing the two groups (7 specimens of *Microcosmodon conus*, 2 of *Pentacosmodon pronus*) which itself might be a feature controlled by sex. However, as the contrasts increasingly outnumbered the similarities between the groups, the present conclusion, that they represent different genera, was reached. In fact, *Pentacosmodon pronus* differs more, structurally, from *Microcosmodon conus* than the genotypes of *Phlodus* and *Ectypodus* differ from each other, despite the size disparity of the latter two

The lower jaw of *Pentacosmodon pronus* is shallower and less robust than that of *Microcosmodon conus*. This difference is associated with the smaller incisor of the former. P_4 of *Pentacosmodon* is longer than the maximum diameter of the incisor. The ascending ramus arises opposite M_2 , rather than near the middle of M_1 , as it does in *Microcosmodon*, and is of comparatively light construction. The masseteric and temporal fossæ of *Pentacosmodon* are not unusually deep, but the pterygoid fossa is huge. From an in-

¹⁰ Refers to the backward deflection of the cusps on M_2

side view, the rear portion of the jaw looks like a human ear (Pl. V, Fig. 1b), the lobe being the inwardly inflected floor of the fossa. Below the large and gently curved condyle the bone is shallow

Apparently the lower incisor was enameled in the general eucosmodontine pattern, although this is not entirely shown on the specimens. The maximum minimum diameter ratios of the incisors of *Microcosmodon conus* and *Pentacosmodon pronus* average 2.1 in each species, but the incisor of the latter has a longitudinal groove along its inner face, as do all the ptilodontines except *Mimetodon*.

Like *Eucosmodon*, *Pentacosmodon* lacks P_3 .

P_4 is considerably larger, more robust, and more typically ptilodontine in *Pentacosmodon* than in *Microcosmodon*. In distinction from that of the latter it has prominent ridges, on each side of the blade, leading to all of the serrations except the first. This first serration is well separated from the remaining four which are evenly spaced. The front of P_4 slightly overhangs the anterior root but there is no basal concavity. The anterior root of P_4 is much heavier than the posterior, whereas the two are about equally developed in *Microcosmodon*. A small posterolabial basal cusp widens this region of P_4 to about the width of the tooth in the vicinity of the anterior root. In *Microcosmodon* the posterior external cusp makes the posterior part of P_4 much wider than the anterior. In both genera the highest part of P_4 is below the plane of the molar cusp tips. Although *Microcosmodon* has two weak and irregular ridges on the anterior surface of P_4 below the first serration, the front edge of this tooth in *Pentacosmodon* is smoothly rounded.

M_1 of *Pentacosmodon* has 4 external and 3 internal cusps. Relative to P_4 it is large and massive, and is of uniform width throughout. The intercusp groove pattern is simple, there being few or no anastomosing ridges upon the slopes of the cusps. The tips of the cusps have a distinct posterior lean. In general M_1 of *Pentacosmodon* is simpler than that of *Microcosmodon*. It has four roots, one on either side between the ones at each end.

M_2 has two distinct cusps on each of the two rows and, in the outer row, a posterior cuspule. The four large cusp tips cant posteriorly. The postero-lingual cusp leans backward at an angle of about 45 degrees, its tip being posterior to its base. M_2 has the same width as M_1 .

TABLE 20
MEASUREMENTS OF LOWER TEETH OF *Pentacosmodon pronus*

Specimens	I			P ₁			M ₁			$\frac{L P_1}{L M_1}$	M ₂		
	Max	Min	Max Min	Serra- tions	L	W	Cusps	L	W		Cusps	L	W
Means				5	1.9	0.95	4.3	2.4	1.35		3.2	1.45	1.35
14085 (Type)				5	1.9	0.9	4.3	2.3	1.3	0.83	3.2	1.4	1.3
14428	1.7	0.8	2.1	5	1.9	1.0	4.3	2.5	1.4	0.76	3.2	1.5	1.4

Clark Fork Multituberculates

Multituberculates are but meagerly represented among the specimens from the Clark Fork. None have been identified in the collections in the American Museum of Natural History, and there are only a few fragments, not good enough for generic identification, in the Princeton collection. One small piece of an incisor pertains to a eucosmodontine, and some other tooth fragments are most likely from ptilodontines. The absence of identifiable representatives of these subfamilies is probably due to the fact noted above, that no sites in the Clark Fork beds have been extensively quarried and therefore much of the microfauna is unknown.

The report (Jepsen, 1930b) of *Paractypodus* sp. from the Clark Fork was an error in locality identification. Princeton no. 13332, the indicated specimen, came from the Gray Bull and probably represents *Ectypodus simpsoni*.

SUMMARY AND STRATIGRAPHIC CONCLUSIONS

The Polecat Bench section contains equivalents of the major North American mammalian faunas of Paleocene time and may serve as a useful comparative or reference section, particularly because of its median geographic location. Its utility as such, however, depends upon the success with which its limits and elements are defined. Stratal terms previously applied in the region, such as "Fort Union," "Puerco," "Tullock," "Lebo," "Torrejon," and "Tongue River," imply lithic and faunal correlations which are inexact, largely unjustified, and misleading. In order to minimize future confusion the use of these names in the area should be discontinued, or suspended until the implied correlations are proved. Stratal and temporal terms should be clearly distinguished because the limits of Paleocene mammalian faunas rarely coincide with

lithic units Some or most of the techniques of stratigraphy founded upon marine strata are inapplicable in studying terrestrial sediments and faunas.

For both practical and theoretical uses, the Paleocene Epoch in America may be defined arbitrarily as the interval between the times when the Cretaceous Lance formation and the Eocene Gray Bull beds or their equivalents were deposited. Dinosaurs became extinct at the end of Lance time (by present definition) and the earliest known ancestors of several groups of modern mammals, such as the perissodactyls, have been found in the Gray Bull beds. In the Polecat Bench section there is no obvious loss of the sedimentary record between the Lance equivalent and the Gray Bull beds. The section is thus favorable for a decisive (and therefore highly arbitrary) delimitation and definition of the Paleocene Epoch in terms of rocks and faunas.

Polecat Bench rocks deposited during the Paleocene Epoch are about 3500 feet thick and are herein named the *Polecat Bench formation*. The lowest and earliest fauna occurs at the base of the formation in a 130-foot thick massive sandstone, the *Mantua lentil*. This Mantua fauna correlates approximately with those from the Puerco formation. Seventy feet above the Mantua lentil, the *Rock Bench quarry beds* yield a fauna which was nearly contemporary with the Lebo and the Torrejon assemblages. Beginning about 1000 feet higher and continuing through approximately 1800 feet of strata, the fauna of the *Silver Coulee beds* correlates with Tiffany, Bear Creek, and Melville faunas. Both the Rock Bench and the Silver Coulee faunas have limited vertical extents. Neither is coextensive with an easily recognized lithic unit or series. The Rock Bench quarry beds are "suspended" in the Polecat Bench formation. This conception and usage violates some arbitrary rules of stratigraphic nomenclature, which were largely based upon studies of marine beds, but is an attempt to present actual field conditions with as little ambiguity and false accuracy as possible. The uppermost 500 feet of the Polecat Bench formation contain a distinctive fauna and were named Clark Fork beds by Granger (1914, p. 204).

Structural features of multituberculate teeth are remarkably uniform within each well defined group. There is but slight variation among the individuals of each species, the species within a genus are in most cases clear cut. The pairing among certain

groups may be due to sexual dimorphism, although there can be no proof that the structures involved are sex linked. The order of succession of the teeth of multituberculates is unique among mammals and emphasizes the taxonomic isolation of the order.

The family Ptilodontidae is composed of at least two subfamilies, Ptilodontinae and Eucosmodontinae. Ptilodontinae includes the American Tertiary genera *Mesodma*, *Kimbetohia*, *Ptilodus*, *Ectypodus*, *Mimetodon*, *Anconodon*, and *Prochetodon*. Eucosmodontinae comprises *Eucosmodon*, *Microcosmodon*, *Pentacosmodon*, and *Neohotomus*.

In general the known distribution of the familial and generic groups is useful for stratigraphic correlations, and some categories are excellent index fossils. Both subfamilies range through the Paleocene into the Eocene. The Ptilodontinae are represented in the late Cretaceous by *Cimolomys* and, possibly, by *Meniscoessus*.

Four genera, *Mesodma*, *Kimbetohia*, *Microcosmodon*, and *Pentacosmodon*, are each known from only one site. *Mesodma* of the Mantua and *Kimbetohia*, a Puercan genus, cannot be directly compared, but both are possible ancestors of the later ptilodontines. *Mesodma ambigua* is perhaps a derivative of *Cimolomys* and may have been ancestral to either *Ptilodus* or *Ectypodus*, or both.

Ptilodus ranges from the upper Dragon to the Melville, through the Torrejon, Rock Bench, and Lebo, and one specimen from the Paskapoo is questionably assigned to the group. The acme of the genus occurred in Torrejonian time, and the three species, *mediævus* (Torrejon), *montanus* (Lebo), and *wyomingensis* (Rock Bench), are so closely allied as to constitute a good evidence of approximate contemporaneity of their horizons.

Ectypodus, represented by its numerous species, occurs from the Melville to the Gray Bull, and is particularly characteristic of the early Late Paleocene, Melville, Tiffany, and Silver Coulee, and part of the Paskapoo. The species *musculus*, *hunteri*, *cochranensis*, and *laytoni* from the Tiffany, Melville, Paskapoo, and Silver Coulee, respectively, support the correlation of the localities but, by themselves, are inconclusive. Species questionably assigned to *Ectypodus*?, *silberlingi*, *grangeri*, and *sinclairi*, are found in the Lebo, and *silberlingi* also comes from the Rock Bench. These species, however, may be removed from the genus when they are adequately represented. *Ectypodus simpsoni* and *tardus*, Gray Bull species, are the latest known representatives of the Allotheria, but are so rare they are almost useless as indices of correlation.

Anconodon has two species, *girdleyi* and *russelli* from the Lebo and the Rock Bench. *Ectypodus? silberlingi* has the same distribution. This indicates that the Lebo and Rock Bench are closely similar in age or that the species are persistent or incredibly convergent.

Mimetodon includes three species which are based solely upon lower jaws that share unique dimensions and structures. The distribution of the species, *douglassi* from the Lebo, *trovessartianus* from the Torrejon, and *churchilli* from the Silver Coulee, is unusual in that the genus ranges from mid to late Paleocene. *Phlodus* is the only other genus with a comparable zone-breaking range, as the distributions are now known. *M. churchilli* is better defined than the other two species of *Mimetodon*, *trovessartianus* and *douglassi*, which may be generically separable when better specimens are discovered. The genus and its species are at present useless in correlations.

Prochetodon cavus, a species with a unique incisor and P_4 , is known from the Silver Coulee zone, and one specimen referred to the genus has been found from the Gray Bull.

Eucosmodon occurs in the Mantua, Puerco, Torrejon, Lebo, and Rock Bench, but the species from each have little value in correlation. *E. americanus* from the Puerco is composed of larger individuals than *E. gratus* from the Mantua. The latter species is small, but larger than *E. jepseni* from the Lebo. The genus is one of the three which are found in both the Puerco and the Torrejon, a range corroborated by its presence in the Mantua and the Rock Bench. It has been reported also in the Paskapoo from a site which may be of Torrejon age (Russell, 1929, pp 165-166). *Eucosmodon* is thus characteristic of the Puerco and the Torrejon and their equivalents, an Early to Middle Paleocene genus.

Neohotomus has two Gray Bull species, *ultimus* and *conventus*, and the latter, the largest of the ptilodontines, also occurs in the Plateau Valley.

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PLATE I

FIG 1 *Eucosmodon gratus*, external side of right lower jaw, \times ca 4 Princeton Mus nos 14418 (incisor and ramus), 14419 (P_1), 14417 (M_1)

FIG 1a The same, crown view M_1 reversed from type, Princeton Mus no 13373

FIG 2 *Eucosmodon gratus*, section of lower incisor, \times ca 7 Princeton Mus no 13374

FIG 3 *Eucosmodon gratus*, external view of right upper incisor, \times ca 4 Princeton Mus no 14416

FIG 4 *Eucosmodon gratus*, crown view of left M^1 , \times ca 4 Princeton Mus no 14420

FIG 5 *Mesodma ambigua*, external side of left lower jaw, \times ca 4 Princeton Mus no 14414, type

FIG 5a The same, crown view

PLATE I

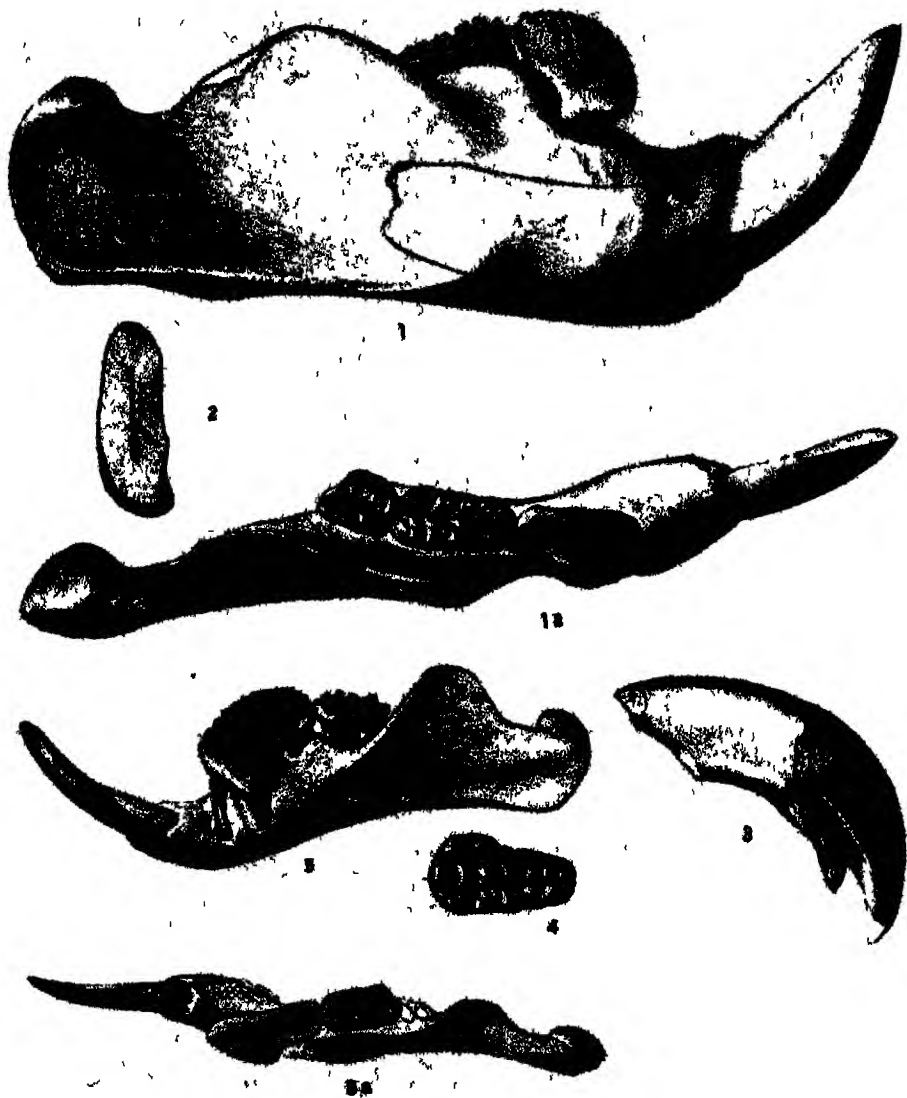


PLATE II

All figures \times ca 35

FIG 1 *Phlodus wyomingensis*, crown view of right lower cheek teeth P_4 from Princeton Mus no 14219 (type), M_1 reversed from Princeton Mus no 14500, and M_2 from Princeton Mus no 14469

FIG 1a *Phlodus wyomingensis*, external view of right lower jaw, type, Princeton Mus no 14419, with incisor no 14503

FIG 2 *Phlodus wyomingensis*, external view of right P^4 , Princeton Mus no 14468

FIG 2a The same, crown view

FIG 3 *Phlodus wyomingensis*, crown view of left P^{1-2} , Princeton Mus no 14428

FIG 4 *Anconodon guleys*, internal view of left P_4 , Princeton Mus no 14524

FIG 5 ?*Anconodon* sp, external view of left P^4 , Princeton Mus no 14470

FIG 5a The same, crown view

FIG 6 *Mimetodon churchilli*, external view of left lower jaw, type, Princeton Mus no 14525

FIG 6a The same, superior view

PLATE II



PLATE III

FIG 1 *Ectypodus powelli*, crown view of left lower cheek teeth, \times ca 8, type, Princeton Mus no 13979

FIG 1a The same specimen, left lower jaw, external view, \times ca 8

FIG 2 *Ectypodus laytons*, crown view of left upper cheek teeth, \times ca 8.5, type, Princeton Mus no 14464

FIG 2a The same, external view

FIG 3 The same specimen, left lower jaw, external view, \times ca 8 Rear part of jaw extended from Princeton Mus no 14338

FIG 3a The same, superior view

PLATE III



PLATE IV

All figures \times ca 4

- FIG 1 *Ectypodus hazeni*, external view right lower jaw, no 14323
FIG 1a The same, superior view
FIG 2 *Ectypodus hazeni*, crown view of right M_2 , reversed from specimen no 14422,
left
FIG 3 *Ectypodus hazeni*, crown view of right upper teeth, type, no 14432
FIG 3a The same, external view
FIG 4 *Prochetodon carus*, external view right lower jaw, type, no 13925 P_4 fore-
shortened in this orientation
FIG 5 *Prochetodon carus*, internal view right P_4 , no 14436, oriented to show maxi-
mum dimensions
FIG 6 *Prochetodon carus*, palatal view of right maxillary fragment containing
 P^{1+2} , no 14034
FIG 7 *Prochetodon carus*, crown view of right P^4 , reversed from specimen no 14435,
left
FIG 7a The same, external view

PLATE IV

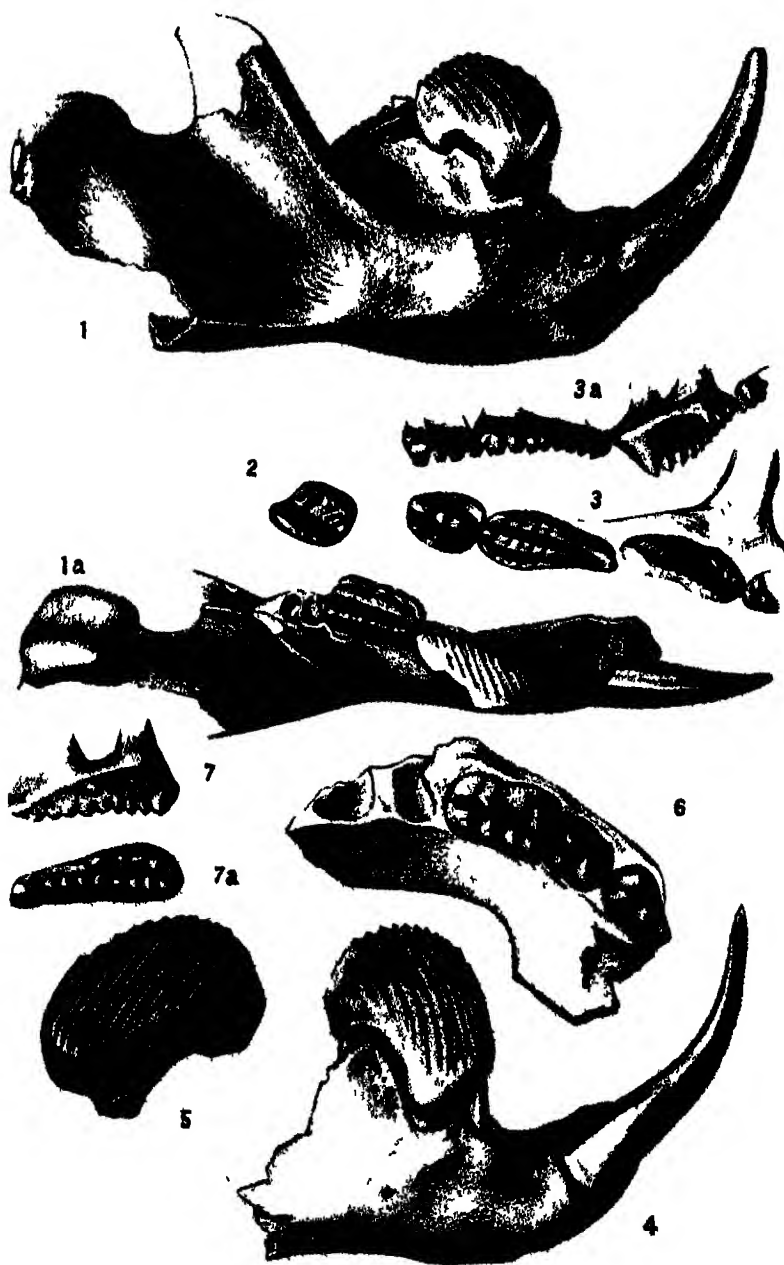


PLATE V

FIG 1 *Pentacosmodon pronus*, external view right lower jaw, type, Princeton Mus no 14085 Incisor root drawn from no 14426 \times ca 7

FIG 1b The same, crown view

FIG 1c The same, internal view

FIG 2 Right upper incisor, referred to *Pentacosmodon pronus* or to *Microcosmodon conus*, Princeton Mus no 14429, \times ca 6 6

FIG 3 *Microcosmodon conus*, external view left lower jaw, Princeton Mus no 14324 Posterior part of bone drawn from no 14430 \times ca 6

FIG 3a The same, superior view, with M_2 drawn, reversed, from no 14322

PLATE V



BENJAMIN FRANKLIN'S VIEWS ON THE PHOSPHORESCENCE OF THE SEA

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(Read November 17, 1939)

ABSTRACT

That the sea was "the grand source of lightning" and "its luminous appearance to be owing to electric fire produc'd by friction between the particles of water and those of salt" has been widely quoted as Franklin's explanation of phosphorescence of sea water. This idea was abandoned as the result of experiments in 1750, when Franklin found that sea water soon lost its power to luminesce by agitation in a bottle and that sea salt dissolved in water would not luminesce when shaken. In correspondence with John Bowdoin in 1753, he adopted the view that the light may be due to "an extremely small animalcule, too small to be visible even with the best glasses." Many theories to account for burning of the sea have been suggested—chemical ignition of sea salt, collision of salt particles giving sparks like flint, friction of atmosphere on earth's movement, frictional electricity, volcanic origin, flashing of small ice crystals when broken, presence of phosphorus, absorption of light by day which is given off at night, putrefaction of animal material and luminescence of small organisms. The last is correct and Franklin's views were abreast of the advancing knowledge of his time.

In September 1753, Benjamin Franklin, Esq¹ of Philadelphia, wrote to Peter Collinson, Esq, F R S London

Sir,

In my former paper on this subject, wrote first in 1747, enlarged and sent to England in 1749, I considered the sea as the grand source of lightning, imagining its luminous appearance to be owing to electric fire, produc'd by friction between the particles of water and those of salt. Living far from the sea, I had then no opportunity of making experiments on the sea water, and so embraced this opinion too hastily.

For in 1750 and 1751, being occasionally on the sea coast, I found, by experiments, that sea water in a bottle, tho' at first it would by agitation appear luminous, yet in a few hours it lost that virtue, *hence, and from this*, that I could not by agitating a solution of sea salt in water produce any light, I first began to doubt of my former hypothesis, and to suspect that the luminous appearance in sea water, must be owing to some other principles.

Although Franklin is generally quoted² as believing the

¹ Experiments and Observations on Electricity, made at Philadelphia in America to which are added Letters and Papers on Philosophical Subjects, London 1769, Letter VII

² It is unfortunate that in Ehrenberg's great monograph "Das Leuchten des Meeres," published in the *Abhandlungen der Koniglichen Akademie der Wissenschaften* in Berlin, for 1834, pp 411 to 576, Franklin's older idea of the phosphorescence of the sea is given. A comprehensive history of the light of animals is to be found in this volume as well as references to early literature.

luminous appearance of the sea to be due to electric discharges, the statement is perfectly definite that he gave up the idea because of a simple experiment that he performed Franklin must have pondered the matter further for on Nov 12, 1753, he received a letter from J.B (John Bowdoin)³ in Boston "concerning the light in sea-water." Extracts were read at the Royal Society, Dec 16, 1756

When I was at the Eastward, I had an opportunity of observing the luminous appearance of the sea when disturbed At the head and stern of the vessel, when under way, it appeared very bright The best opportunity I had to observe it, was in a boat, in company with several Gentlemen going from Portsmouth, about three miles, to our vessel lying at the mouth of Piscataqua River Soon after we set off (it being in the evening) we observed a luminous appearance, where the oars dashed the water Sometimes it was very bright, and afterwards as we rowed along, gradually lessened, till almost imperceptible, and then re-illuminated This we took notice of several times in the passage When I got on board the vessel, I ordered a pail to be dipped up, full of sea-water, in which, on the water's being moved, a sparkling light appeared I took a linnen cloth, and strained some of the water through it, and there was a like appearance on the cloth, which soon went off, but on rubbing the cloth with my finger, it was renewed I then carried the cloth to the light, but could not perceive any thing upon it which should cause that appearance

Several Gentlemen were of opinion, that the separated particles of putrid, animal, and other bodies, floating on the surface of the sea, might cause that appearance, for putrid fish, etc they said, will cause it And the sea-animals which have died, and other bodies putrified therein since the creation, might afford a sufficient quantity of these particles to cover a considerable portion of the surface of the sea, which particles being differently dispersed, might account for the different degrees of light in the appearance above-mentioned But this account seems liable to this obvious objection, That as putrid fish, etc make a luminous appearance without being moved or disturbed, it might be expected that the supposed putrid particles on the surface of the sea, should always appear luminous, where there is not a greater light; and, consequently, that the whole surface of the sea, covered with those particles, should always, in dark night, appear luminous, without being disturbed But this is not fact

Among the rest, I threw out my conjecture, That the said appearance might be caused by a great number of little animals, floating on the surface of the sea, which, on being disturbed, might, by expanding their fins, or other wise moving themselves, expose such a part of their bodies as exhibits a luminous appearance, somewhat in the manner of a glow-worm, or fire-fly That these animals may be more numerous in some

³ Franklin's *Letters and Papers on Philosophical Subjects*, 1769, p 273

places than others, and, therefore, that the appearance above-mentioned being fainter and stronger in different places, might be owing to that. That certain circumstances of weather, etc might invite them to the surface, on which, in a calm, they might sport themselves and glow, or in storms, being forced up, make the same appearance

To this Franklin ⁴ replied on Dec 13, 1753,

The observation you made of the sea water emitting more and less light in different tracts passed through by your boat is new, and your manner of accounting for it ingenious. It is indeed very possible, that an extremely small animalcule, too small to be visible even by the best glasses, may yet give a visible light. I remember to have taken notice, in a drop of kennel water, magnified by the solar microscope to the bigness of a cart-wheel, there were numbers of visible animalcules of various sizes swimming about, but I was sure there were likewise some which I could not see, even with that magnifier, for the wake they made in swimming to and fro was very visible, though the body that made it was not so. Now, if I could see the wake of an invisible animalcule, I imagine I might much more easily see its light if it were of the luminous kind. For how small is the extent of a ship's wake, compared with that of the light of her lantern.

Here we have Franklin's final and correct views on the burning of the sea. Further work merely added more evidence to the biological theory of its origin. The first observation of the well known flagellate, *Noctiluca*, cause of the light of the sea, is recorded by an early microscopist, Henry Baker. He received a letter from a friend, Joseph Sparshall, describing in sea water little globes with tiny tails which luminesced, but the accompanying figure came too late to be reproduced in the first edition of Baker's book, *Employment for the Microscope*, published in 1753.

Le Roi in 1754 probably also saw *Noctiluca* as he examined with a microscope sea water on a trip from Naples to France and noted luminous particles like the head of a pin, but thought them of an oily or bituminous nature, since they did not have the appearance of an animal.

In 1757, Baster recognized with his microscope a number of minute "infusoria" as the cause of the light and found, as did Bowdoin, that filtering the sea water through blotting paper would remove the organisms and the luminescence. Forskål confirmed this in 1762 and Rigault (1765) described *Noctiluca* so minutely that it could be definitely recognized, while Dicquemare ⁵

⁴ Smyth, A. H. *The Writings of Benjamin Franklin*, London, 1905. Vol. III, p. 192.

⁵ Dicquemare, Abbe. "Sur la lumière dont la mer brille souvent pendant la nuit." *Journ. de Physique*, 6, 319, 1775.

in 1775 and Slabber⁶ in 1778 both figured it. There can be no mistake in recognition, as can be seen from the accompanying Fig 1. Slabber called the animal "medusa marina."

Although phosphorescence of the sea came definitely to be connected with minute organisms in the 1750's, Le Gentil (1761) and Bajon (1774) still considered it electric because it was excited by friction, Martin (1761) and Canton (1769) thought it due to putrefaction such as causes luminescence of dead fish and flesh (luminous bacteria), Silberschlag (1770) thought it was phosphorus and J. Mayer (1785) "conjectured that the surface of the sea imbibed light which it afterwards discharged."

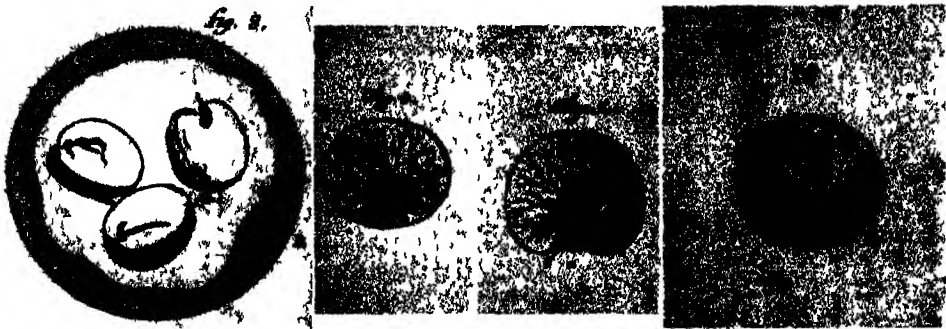


FIG 1 Photographs of the earliest drawings of *Noctiluca*. Fig 8, after Dictionnaire, 1775. Figs 4 and 5, after Slabber, 1778. 10, after Macartney, 1810.

In this way, Macartney, who also described *Noctiluca* as *Medusa scintillans*,⁷ speaking before the Royal Society of London in 1810, summed up the various older views and said, "I shall not trespass on the time of the Society to refute the above speculations, their authors have left them unsupported by either arguments or experiments, and they are inconsistent with all ascertained facts upon the subject. The remarkable property of emitting light during life is only met amongst animals of the four last classes of modern naturalists, viz., molusca, insects, worms and zoophytes."

It is interesting to trace the development of opinion on the phosphorescence of the sea before Franklin's time, for the display

⁶ Slabber, Martinus. *Naturkundige Verlustingen*. Haarlem 1778.

⁷ Macartney, J. "Observations on Luminous Animals." *Phil Trans*, 100, 258, 1910. The present name, *Noctiluca*, was given by Suriray, a physician at Havre, in a letter to Lamarck in 1816. He described the animal as *Noctiluca miharis*. Rules of zoological nomenclature seem to indicate (Kofoid) that the most famous of luminous organisms should be referred to as *Noctiluca scintillans*.

was a mysterious phenomenon to the older observers. Curiously enough there are few references in the writings of the ancients.

In more recent times light of the sea was noted by many famous voyagers, Amerigo Vespucci (1452-1512), João de Castro (1500-1548), John Davis (1550-1603), but no attempts to explain the origin of the light appeared until Francis Bacon in 1620. He listed many luminescences, including that of the sea, fire-flies, worms, sugar on crushing, the sweat of heated horses, and also jelly-fish, which he regarded as heated sea foam. He argued that just as transparent glass when heated becomes opaque and luminous, so air and water, although themselves transparent, become when combined as foam, opaque and self lighting.

This is the first of the purely physical explanations of sea light, which, in one form or another, usually regarded the phosphorescence as due to friction or heat. Thus, Papin (1647) speaks of the light as a chemical ignition of sea salt, Cartesius (1648) thought the salt molecules rubbed together giving sparks like flint, while Robert Boyle (1673) imagined some cosmical law must produce the effects. He wrote (Birch's first edition of Boyle's Works, Vol 3, p 91, 1744)

When I remember how many questions I have asked navigators about the luminousness of the sea, and how in some places the sea is wont to shine in the night as far as the eye can reach, at other times and places, only when the waves dash against the vessel, or the oars strike and cleave the water, how some seas shine often, and others have not been observed to shine, how in some places the sea has been taken notice of, to shine when such and such winds blow, whereas in other seas the observation holds not, and in the same tract of sea, within a narrow compass, one part of the water will be luminous, whilst the other shines not at all when, I say, I remember how many of these odd phaenomena, belonging to those great masses of liquor, I have been told of by very credible eyewitnesses, I am tempted to suspect, that some cosmical law or custom of the terrestrial globe, or, at least, of the planetary vortex, may have a considerable agency in the production of these effects.

Mornay (1667), Biornonius (1673) and Martius (1675) all recorded sea light but added nothing to our real knowledge of the phenomenon. Indeed the views of that time were highly fantastic and the simplest experiment would have disproved them. Rumph (1680) noted the phosphorescence of the sea at Amboina in the Dutch East Indies and thought it volcanic in origin while Tachard (1686) saw the southern sea near Siam luminescent and thought it was due to absorption of sunlight. Worms (1709)

also speaks of fiery ghosts like lightning from the heat of the sun's rays during the day

A definite correlation between the character of the sea water and its luminescence is given by Father Bourzes (1708) who noted that luminescent water was shiny or glutinous After describing a remarkable phosphorescence observed in the Indian Ocean on July 10, 1704, bright enough to read by, he wrote in the *Philosophical Transactions of the Royal Society*, 11, p. 599

If one takes some Water out of the Sea, and stirs it never so little with his Hand in the dark, he may see in it an infinite number of bright Particles Or if one dips a piece of Linnen in Sea Water, and twists or wrings it in a dark Place, he shall see the same thing, and if he does so, though it be half dry, yet it will produce abundance of bright Sparks When one of the Sparkles is once formed, it remains a long time; and if it fix upon any thing that is solid, as for instance, on the side or edge of a Vessel, it will continue shining for some Hours together It is not always that this Light appears, tho' the Sea be in great Motion, nor does it always happen when the Ship sails fastest Neither is it the simple beating of the Waves against one another that produces this Brightness, as far as I could perceive But I have observ'd that the beating of the Waves against the Shore, has sometimes produced it in great plenty, and on the Coast of Brazil the Shore was one Night so very bright, that it appeared as if it had been all on Fire

The Production of this Light depends very much on the Quality of the Water I have often observed, that when the Wake of the Ship was brightest, the Water was more fat and glutinous, and Linnen moisten'd with it produced a great deal of Light, if it were stir'd or mov'd briskly Besides, in sailing over some Places of the Sea, we find a Matter or Substance of different Colours, sometimes red, sometimes yellow, In looking at it, one would think it was Saw-dust Our Sailors say it is the Spawn or Seed of Whales What it is, is not certain, but when we draw up Water in passing over these Places, it is always viscous and glutinous Our Mariners also say, That there are a great many Heaps or Banks of this Spawn in the North; and that sometimes in the Night they appear all over of a bright Light, without being put in Motion by any Vessel or Fish passing by them But to confirm farther what I say, viz That the Water, the more glutinous it is, the more it is disposed to become luminous, I shall add one particular which I saw myself One Day we took in our Ship a Fish, which some thought was a *Boneta*. The inside of the Mouth of the Fish appeared in the Night like a burning Coal, so that without any other Light, I could read by it the same Characters that I read by the Light in the Wake of the Ship It's Mouth being full of a viscous Humour, we rubbed a piece of Wood with it, which immediately became all over luminous, but as soon as the Moisture was dried up, the Light was extinguish'd

I leave it to be examined whether all these particulars can be ex-

plained by the system of such as assert, that the principle of this light consists in the motion of a subtle matter, or globules, caused by a violent agitation of different kinds of salts

In 1713 Deslandes thought the light due to oily drops from decomposition of small "insects" and worms which burned. At this time the scientific world devoted considerable attention to the study of phosphors and the Academy at Bordeaux offered a prize for the best essay on the subject. This was won by Dartous de Mairan in 1717 for a thesis entitled, "Dissertation sur la cause de la lumière des phosphores et des Noctiluques". It has some-

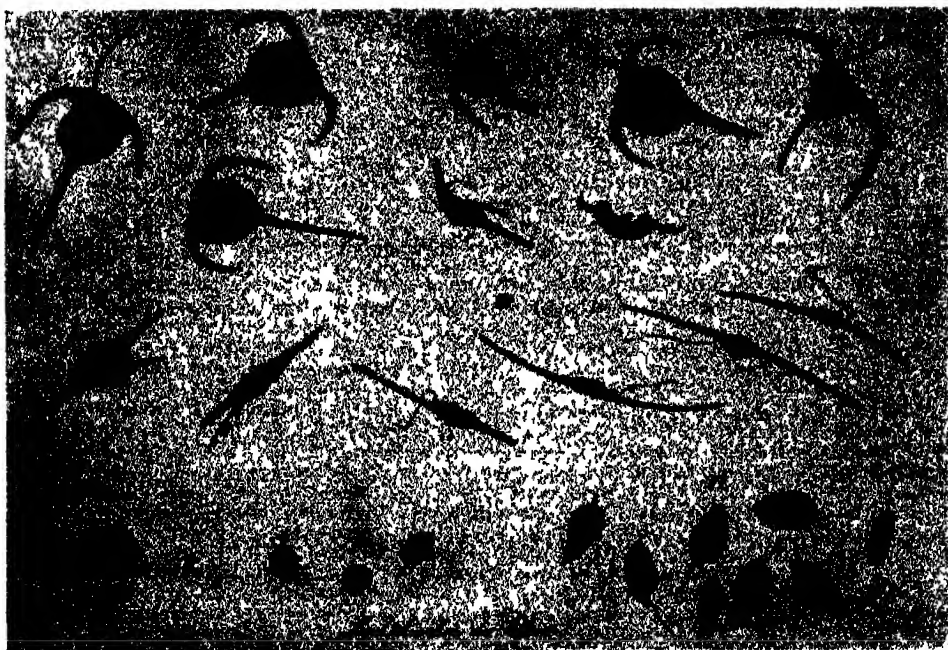


FIG 2 Photograph of one of Ehrenberg's (1834) plates, showing various kinds of dinoflagellates, greatly magnified

times been said that this essay refers to the animal, *Noctiluca*, but this is probably not true.⁸ The word noctiluca was then used for any kind of phosphorescence and Robert Boyle speaks of the Bolonian phosphor (impure barium sulphide) as a solid noctiluca. He also speaks of gummous, liquid and aerial noctilucas, meaning the element phosphorus as solid, dissolved, and in gaseous form.

⁸ I have been unable to obtain a copy of Dartous de Mairan's work

It was not until the middle of the 18th century that small luminous water "insects" were noted. Thus Anderson (1747) saw a crustacean (*Oniscus fulgens*) and Adler (1749) a worm (*Nereis phosphorans*). Vianelli (1749), Griselini (1750) and the Abbe Nolle (1750) also noted tiny luminous worms called *Scolopendra marina*, in the lagoons at Venice. Then came Franklin's animalcule hypothesis and Baker's description of *Noctiluca*, especially abundant along the coasts of northern Europe.

This creature is one of the largest of unicellular animals, sometimes one millimeter in diameter. Luminescence of its numerous relatives the dinoflagellates, was not definitely established until the researches of Michaelis (1830) and Ehrenberg (1834). One of Ehrenberg's plates is reproduced as Fig. 2. Dinoflagellates live in open seas and are truly microscopic, less than one tenth the diameter of *Noctiluca*. They give a yellow or reddish tint to sea water by day, shine like fire at night, and were undoubtedly responsible for the "sawdust" seen by Father Bourzes on his journey to India. Without a good microscope, it is not surprising that phosphorescence of the sea should have been such a mysterious phenomenon. Benjamin Franklin's views were definitely abreast of the advancing knowledge of his time.

STUDIES OF LIVING NERVES. VI. EFFECTS OF METRAZOL ON TISSUES OF FROG TADPOLES WITH SPECIAL REFERENCE TO THE INJURY AND RECOVERY OF INDIVIDUAL NERVE FIBERS¹

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ABSTRACT

In recent years many cases of human mental disorder have been cured by injections of metrazol, a treatment which causes profound shock. The purpose of the present investigation was to determine whether any structural changes could be detected in individual nerve fibers and other tissues by direct microscopic observations during and following metrazol treatment. Although such observations cannot be made in man, they can be made quite readily in living frog tadpoles.

The reactions of nerve fibers and their endings have been watched after mild, moderate, and severe metrazol treatments. Illustrative motion pictures of the microscopic changes have been made. Typical changes of irritation and injury take place. Such changes in the myelinated fibers include swelling, vacuolation, microfibrillation, myelin globule formation, and in extreme cases loss of some of the terminal myelin segments.

Changes in the nerve terminals include swelling, occasional retraction, and loss by degeneration of variable lengths of the endings. Sometimes in extreme cases a whole cluster of endings is lost.

Slightly injured nerve fibers quickly become normal again in appearance after a day or two of recovery. Severely injured fibers which have lost appreciable lengths of nerve substance by degeneration undergo the typical stages of regeneration. New endings grow out to establish connections which are different from those before the metrazol treatment. In other words, the metrazol treatment has brought about a "new deal" of nerve endings.

These observations strongly suggest that similar changes probably take place in nerve endings located within the brain at the synapses between nerve cells. On this basis the improvement in human mental conditions after metrazol injections is correlated with the breaking down of some of the nerve endings and synapses, and the establishment of new ones.

Muscle fibers, epithelial cells, and the general circulation are also markedly affected by metrazol treatment.

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¹ Aided by a grant from the Penrose fund of the American Philosophical Society. The cin6-photomicrographic outfit was purchased originally by a grant from the Grants-in-Aid Committee of the National Research Council.

INTRODUCTION

WITHIN recent years pharmacological shock therapy has been developed and used extensively in the treatment of schizophrenia and other human mental disorders. Many individuals suffering from schizophrenia have been cured, or benefitted greatly, by injections of metrazol or by injections of insulin. Both of these treatments are accompanied by profound shock. Von Meduna ('34 and '38) has perfected the treatment with metrazol (penta-methylenetetrazol, cardiazol). Sakel ('38) is chiefly responsible for the insulin shock treatment. Electric shock treatments are also now being employed for similar purposes (Cerletti and Bini, '38, and Kalinowsky, '39).

Exactly what happens to the nervous system during and following the treatments is not known. Ample evidence, however, has been presented to indicate that nerve cell damage may occur in some cases. This has been reported in both man and experimental animals (*e g*, Hassin, '39, Liebert and Weil, '39, von Meduna, '34, and Accornero, '38).

Purely as a working hypothesis Sakel ('38b) has presented an interesting conception of nerve cell changes with which he tries to account for the symptoms associated with schizophrenia and for the improvement that sometimes follows the insulin treatment. He thinks that there exist in nerve cells pathways higher and lower in the evolutionary scale. When nerve cells react normally the newer pathway patterns supersede the old. When nerve cells react abnormally, however, either obsolete pathways become reactivated or else pathways themselves become distorted and criss-crossed. In such cases a stimulus may activate pathways not ordinarily activated by such a stimulus, and reactions may be elicited which are not ordinarily elicited by such a stimulus. If cellular dysfunction continues and the same stimuli continue to invoke the same confused reactions involving the same confusion of pathways, the dissociated and false pathways become fixated and finally supersede the normal pathway patterns (psychotic deterioration). With insulin treatment Sakel thinks that a blockade may be set up preventing stimuli from reaching a nerve cell which has been reacting abnormally so that it has the time and opportunity to "polarize" the normal pathway. He thinks also that shock treatment which nearly causes destruction of the cell "serves to shatter or destroy every pathway which is not firmly

established so that every possible defensive resource of the cell is provoked and reactivated, in order to save the cell by reestablishing normal conditions in face of this attack "

It is, of course, impossible to watch living nerve cells within the brain of a vertebrate animal. Peripheral nerve fibers and their endings, however, can be observed in minute detail in living frog tadpoles. I have already reported many of the changes in nerve fibers during growth, regeneration, irritation, injury, and recovery (Speidel, '32, '33, '35, '36, and '39). The present investigation was undertaken to find out whether visible structural changes in nerve fibers could be detected in living frog tadpoles during metrazol treatment and during the ensuing recovery period. The effects of insulin and electric shock treatments on tadpoles are also now being investigated.

MATERIAL AND METHOD

Tadpoles of the swamp cricket frog (*Pseudacris feriarum*) and of the tree frog (*Hyla crucifer*) were used. These species are particularly favorable for microscopic observations with the use of an oil-immersion objective lens.

Metrazol was administered by merely placing the animals in an aqueous solution.² The grosser reactions of the tadpole's tissues were watched under a binocular microscope during the metrazol treatment. The finer details were watched under a compound microscope, using a method described in earlier papers of this series of studies. In some cases the use of chlorotone anesthesia was necessary, in others this was dispensed with. Successful case histories of microscopic changes in nerve fibers and other tissues during and following metrazol treatment were obtained in about one hundred tadpoles.

Illustrative ciné-photomicrographs were also made directly from the experimental animals. These reveal clearly the principal changes in the tissues during irritation, injury, and recovery. They record also the changes in position from day to day of the delicate nerve endings.

² Metrazol injected into the blood stream of the frog in suitable amounts causes convulsions (Hildebrandt, '37). Because of their small size tadpoles were unsuited for injection techniques. Tadpoles immersed in metrazol solutions of the concentrations usually employed did not exhibit convulsions ordinarily. It should be pointed out that metrazol invades the tissues of the tadpole relatively slowly by passing through the moist epithelial surface (possibly some by way of the gills and blood). This is in contrast with the more rapid invasion effected by the injection method, as used in man and experimental mammals, by which metrazol introduced into the blood stream passes quickly through the blood vessel walls to get to the tissues.

PRELIMINARY EXPERIMENTS AND OBSERVATIONS

Preliminary experiments with various dilutions of metrazol were first carried out. Aqueous solutions of metrazol varying from 5 per cent to 0.3 per cent were tried. Very favorable results were obtained with solutions of about 2 per cent. Tadpoles immersed in a solution of this strength for about 20 minutes exhibited marked reactions of irritation and injury. Obvious microscopic changes in the tissues were discernible. The tadpoles, however, readily recovered when they were replaced in pond water.

Metrazol solutions of greater concentration quickly caused severe injuries. Thus, 3 per cent metrazol acting upon an animal for 10 minutes was usually fatal. Even for a period of 5 minutes this concentration caused marked injury.

Metrazol solutions of less than 2 per cent induced irritations and injuries of milder nature. Thus, an animal immersed in 1 per cent metrazol for 20 minutes suffered irritation but not much injury. Even for a treatment lasting 40 minutes recovery readily followed, although the tissues had suffered obvious injury. Very weak metrazol solutions were tolerated for much longer periods. Thus, a tadpole immersed in 0.3 per cent metrazol showed only slight irritative effects after a period of 5 hours.

The degree of metrazol injury varied with the size of the animals treated. A small tadpole was injured much more severely than a large tadpole when both were placed in the same metrazol solution for the same length of time.

Furthermore, it was clear that the degree of injury varied in different parts of the tail, being greatest near the tip and least near the root. The tail of the frog tadpole is thicker and more mature near the root, and thinner and younger near the tip. It represents, therefore, a graded structure in anatomical differentiation and reacts as such in its response to metrazol treatment. Likewise it was seen that the thicker axial zone of the tail was less susceptible to injury than the thinner dorsal and ventral fin zones, especially near the fin edges.

The following general effects of metrazol treatment were noted: decrease in the speed of the circulating blood with stasis in capillaries in the thinner regions of the tail fins, decrease in the rate of heart beat, formation of epithelial vesicles or blisters, vacuolation of individual epithelial cells and sometimes destruction of some

areas, various grades of irritation and degeneration of striated muscle fibers, and various grades of irritation and degeneration of nerve fibers

Each animal under treatment was carefully watched and the progress of the injury noted. Valuable criteria of this progress, readily observable with low magnification, were the circulatory and epithelial surface reactions. As the circulation became slow, epithelial blisters made their appearance. They were developed first near the tail tip and the fin edges. Almost invariably a treatment with 2 per cent metrazol for 20 minutes induced a fair number (Fig. 1) Even after the tadpole was replaced in pond

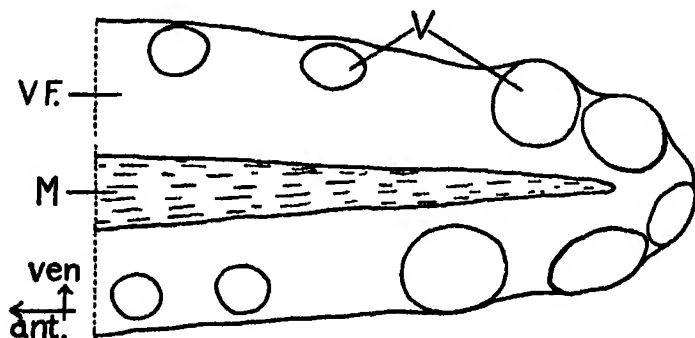


Fig 1 Diagram showing vesicle formation in the tail of a metrazol-treated frog tadpole. Small tadpole no 2217, immersed in 2 per cent metrazol on March 12th from 11 30 A M to 11 59 A M, tail tip sketched at 12 19 P M. The arrows indicate anterior (ant) and ventral (ven) directions. As metrazol entered the tissues vesicles (V) appeared in the thin regions of the tip and borders of the tail, though not in the thicker muscle zone (M). The illustration shows 4 vesicles in the ventral fin (V F), farther anteriorly in the ventral fin 12 more vesicles of smaller size were visible. (In this animal the blood circulation became slow and typical structural changes of irritation and injury could be discerned in nerve endings, in myelinated segments, and in some muscle fibers. The tadpole readily recovered from the treatment.)

water a few more vesicles often appeared. Within an hour or two, however, marked epithelial cell adjustments ensued. The vesicular fluid was eliminated. The epithelial cells in the vicinity moved enough to become readjusted to the changing stresses and tensions. Ciné-photomicrographs of the fast motion type vividly reveal the intense activity on the part of the epithelium a few hours after metrazol treatment. Within a day or two recovery is practically complete.

MYELINATED NERVE FIBERS DURING METRAZOL INJURY AND RECOVERY

In earlier studies I have reported the principal microscopic changes in nerve fibers during irritation, injury, and recovery (Speidel, '35 and '36) Entirely similar changes are exhibited by

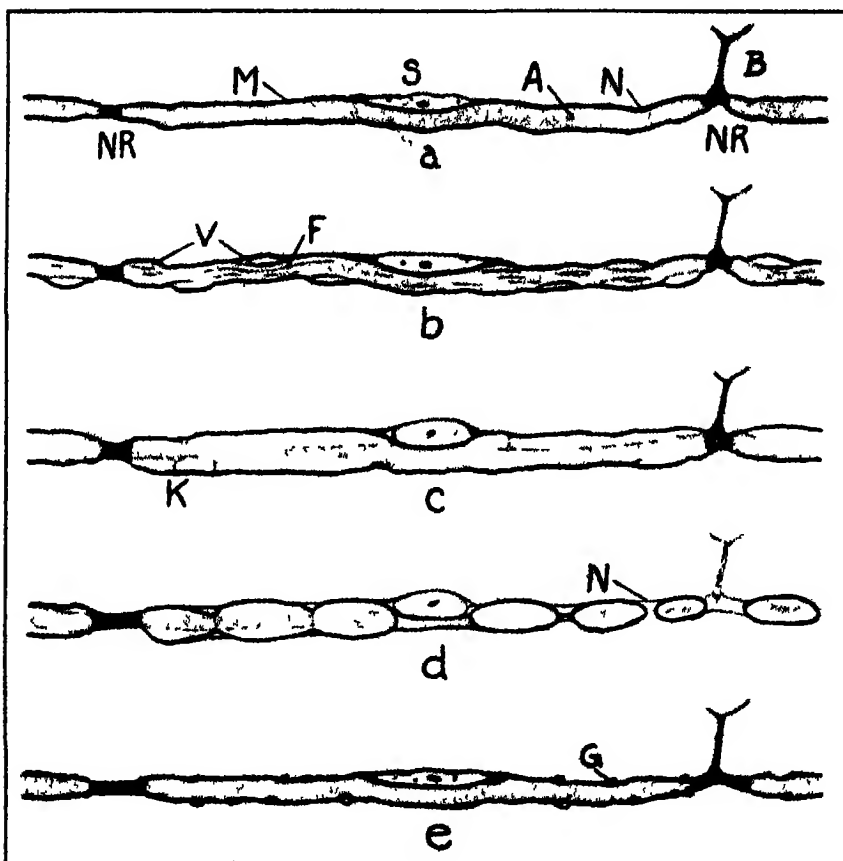


FIG 2 Diagram showing some typical irritative changes in myelinated nerve fibers *a*, normal fiber showing one complete internodal segment between the nodes of Ranvier (NR) with myelin sheath (M), axis cylinder (A), sheath cell (S), and neurilemma (N) At the node of Ranvier to the right is shown the base of a side branch (B) which divides and gives rise to a whole cluster of cutaneous nerve endings (not shown) *b*, moderately irritated fiber characterized by vacuoles (V) between the myelin sheath and axis cylinder Delicate fibrils (F) are vaguely visible within the axis cylinder *c*, markedly irritated fiber characterized by swelling and separation of myelin sheath and axis cylinder A few delicate strands connecting these are discernible, as at K *d*, severely injured fiber, characterized by fragmentation of the myelin sheath into ovoids Toward the right the axis cylinder has also suffered fragmentation In some places the neurilemma (N) is clearly visible *e*, recovering fiber one day after marked irritation This fiber is characterized by small globules and granules cut off from the myelin sheath and by slight loss of myelin in the vicinity of the nodes of Ranvier

nerve fibers in metrazol-treated tadpoles. Some of these are illustrated in diagrammatic form (Fig. 2). A fiber subjected to gradually increasing degrees of irritation exhibits swelling, the appearance of delicate fibril-like structures within the axis cylinder, slow undulating movements of the myelin sheath, the appearance of vacuoles between the axis cylinder and myelin sheath, and the gradual separation of the myelin sheath from the axis cylinder. If the irritation is not too great the fiber readily recovers and again resumes its normal diameter as swelling subsides. A few globules of the myelin sheath are usually cut off as a result of the irritation. These are often visible for a day or two after the injury (Fig. 10). During extreme injury the myelin sheath breaks up into myelin ovoids. Usually there is corresponding fragmentation of the axis cylinder. The large-sized myelin ovoids which are formed at first become broken up during succeeding days into the typical smaller-sized globules and granules of degeneration (Fig. 12). Occasionally, however, the axis cylinder survives even though the myelin sheath degenerates (Fig. 8).

Examples of varying degrees of myelin segment injury and recovery in metrazol-treated tadpoles are presented. A moderately irritated myelin segment (Fig. 10) readily recovers. A severely injured myelin segment (Fig. 18) may or may not recover. In this particular case the tadpole succumbed to the combined metrazol treatment and chlorotone anesthesia. In another case, however, the various myelin segments of a fiber were greatly swollen as a result of a metrazol treatment. Those most terminally located degenerated, those more proximally located recovered. The illustration (Fig. 12) shows the transition region on the day after the metrazol treatment. In the nerve fiber figured, axis cylinder degeneration was not as extensive as myelin degeneration. In this same tadpole it is interesting to note that the more deeply located myelin segments of a nearby fiber did not suffer degeneration.

Other instances of myelin degeneration are given. One shows myelin degeneration without accompanying axis cylinder degeneration (Fig. 8). Another shows both myelin degeneration and accompanying axis cylinder degeneration (Fig. 9).

TERMINAL AMEBOID GROWTH CONES OF REGENERATING FIBERS DURING METRAZOL IRRITATION AND RECOVERY

Actively growing tips of regenerating nerve fibers are particularly sensitive to irritative conditions. They exhibit in somewhat

exaggerated fashion the changes that also take place in the more stable resting nerve endings. I have already published an account of the changes that may be discerned in their reactions in tadpoles during alcoholic intoxication and recovery (Speidel, '36).

Metrazol treatment induces changes that are quite similar structurally to those induced by alcohol. In diagrammatic form some of the more usual shapes assumed by the growing nerve terminals (growth cones) are illustrated (Fig. 3). These include the spherical resting type, the swollen irritated type, the retracting type, and the actively advancing type.

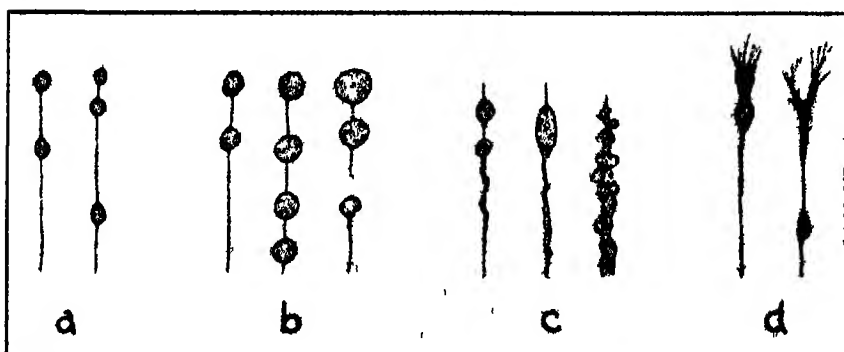


FIG 3 Diagram showing some varieties of nerve endings as seen in the tail of the living frog tadpole a, resting endings characterized by spherical or ovoid tips b, irritated endings characterized by swollen tips The fiber at the right exhibits marked swelling and separation of the terminal portion (nerve autotomy) c, retracting endings characterized by retraction clubs with a pointed filament at the tip The fiber at the right exhibits marked thickening with knob-like excrescences d, growing endings characterized by terminal enlargements (growth cones) with variable numbers of delicate filamentous extensions

Case histories from two animals illustrate clearly how the growth cones react to metrazol. The first of these (Fig 5) shows the rapid change in form from typical growing tips to irritated and retracting tips. The period immediately after the treatment reveals that the irritative effect continues for some minutes, the first signs of definite recovery being noted in one of the endings about 14 minutes after the end of the metrazol treatment. Further growth then readily ensues.

Another growth cone in the same tadpole (Fig 6) displayed similar irritation and retraction. This was followed, however, by growth during the next two hours along a new route. This in turn proved to be temporary and the ending again retracted and then proceeded again in the original direction.

In the other case history selected for illustration (Fig. 7) the reactions of several growing tips are recorded. Three metrazol treatments were given within a 24-hour period. In each instance the growth cones were transformed into irritated swollen endings and exhibited a certain amount of retraction. One short branch was permanently eliminated. Recovery and growth of the others quickly followed the restoration of normal conditions.

Many other direct observations on growth cones during and following metrazol treatment make it certain that the foregoing cases are typical.

CUTANEOUS NERVE ENDINGS DURING METRAZOL INJURY AND RECOVERY

In less pronounced fashion, resting cutaneous nerve endings exhibit changes that are essentially similar to those of the growth cones of regenerating fibers. One example which was recorded ciné-photomicrographically shows a slight but definite amount of retraction over a period of 30 minutes (Fig. 11). This nerve ending, located just beneath the epidermis, belonged to a side branch which emerged at a node of Ranvier of a myelinated fiber. On each side of the node the axis cylinder displayed microfibrillation during the treatment.

Another case is cited which brings out several other points about nerve reactions to metrazol treatment (Fig. 8). In this case a fairly severe treatment caused marked irritation of a myelinated fiber. On the following day it was noted that the myelin segment distally located had suffered degeneration though its axis cylinder survived intact, together with a side branch at the node of Ranvier. There was no degeneration of the other myelin segment figured, which was located proximally. Two of the endings of the side branch during the next four days exhibited definite growth. One of them also gave rise to a new branch. It was also noted that an ending grew into the zone of observation.

In still another case, a very severe metrazol treatment (that of May 5th) caused loss of the terminal segments of myelin sheath and of an entire cluster of nerve endings which belonged to a side branch at a node of Ranvier (Fig. 9). In this case the axis cylinder of the fiber also underwent degeneration in the region illustrated, though farther proximally it did not degenerate but merely suffered irritation. Quick regeneration ensued. The new fiber occupied

the old neurilemma. A new side branch arose from it at the former site. The sub-branches which developed and the pattern of endings, however, differed markedly from the original condition before treatment.

A final case (Fig 4) illustrates another feature of interest. In this a tadpole was subjected to metrazol treatment several times. Three nerve endings were kept under careful observation. They were branches of different myelinated fibers, but were, however, located in the same general region of the tail fin not far from one another. The metrazol treatments were not severe enough to

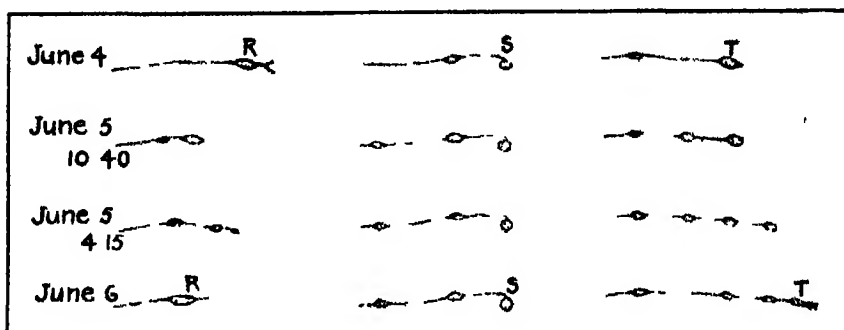


FIG 4. Variations in the reactions of three nerve endings in the same tadpole, following two successive metrazol treatments. Tadpole no. 2274, subjected to 2.5 per cent metrazol on June 4th and on June 5th from 10 57 to 11 20. On June 4th in a normal tadpole before treatment several nerve endings were kept under observation, three of which (*R*, *S* and *T*) are figured. Following a treatment with metrazol, little change took place in these over a five-hour period, though *R* and *T* seemed somewhat plastic. On June 5th at 10 40 A.M., however, *R* had retracted, *S* and *T* showed little change except that a new varicosity was present on each fiber. A second metrazol treatment was given leading to temporary irritation. Five hours later at 4 15 P.M. both *R* and *T* exhibited definite growth, *S* exhibited no change. On the following morning, June 6th, *R* had retracted, *T* had grown out farther, while *S* remained the same. (A third severe metrazol treatment was then given following a brief period of chloroform anesthesia. Extensive injury to both muscle and epithelium resulted, and the tadpole did not survive.)

induce myelin-sheath degeneration, though irritative changes were visible. The first ending exhibited retraction, growth, and again retraction, the net result being a definite amount of retraction. The second ending exhibited neither retraction nor growth, but retained its original position. The third ending exhibited no retraction, definite growth occurred. Thus, a moderate degree of irritation may affect different nerve endings in different ways. The result after recovery from the treatment may be a net advance, a net retreat, or no change in position.

EFFECTS OF METRAZOL ON UNMYELINATED NERVES

In the tadpole's tail are many unmyelinated nerves. The larger ones include multiple fibers and multiple neurilemmas. Every single fiber, however, is not necessarily ensheathed by a separate neurilemma of its own. Several fibers are sometimes included within a single neurilemma (*cf* Speidel, '35). Small-sized nerves are occasionally of this type. Near their tips at the epidermis the fibers are naked.

Metrazol treatment causes irritative changes in unmyelinated nerves similar to those already described for alcohol treatment (*cf* Speidel, '36). Vacuoles appear which often contain one or several dark refractive granules. These may persist for some time (Fig 13). Ready recovery usually ensues, provided the nerve substance is not killed.

Irritated sheath cells of Schwann (neurilemma cells) are characterized by swollen, rounded nuclei and by small vacuoles in their scanty cytoplasm. Their reaction is about the same whether they are located on myelinated or unmyelinated fibers. They usually recover readily.

EFFECTS OF METRAZOL ON OTHER TISSUES

The gross effects of metrazol on the cutaneous epithelium, muscle, and circulation have already been mentioned. A few words should be added concerning the details of microscopic changes in these and also in the connective tissue and endothelium.

Epithelium —Since the epithelium of the skin is directly bathed by the metrazol solution it is not surprising that the cells exhibit marked reactions. Vacuoles, one or several in number, usually appear in individual cells (Fig 15). These may grow in size. Spaces may also appear between cells. Often a cell in mitosis offers a locus that is especially favorable for attack. Normally as such a cell rounds up it becomes somewhat discrete from neighboring cells. Metrazol treatment emphasizes this process. In the illustration (Fig 15) a watery zone is conspicuous about the dividing cell. Several other dividing cells were observed in this same animal, each with a similar watery zone about it. I have previously also called attention to the induction by metrazol of epithelial vesicles, each of which involves many epithelial cells. Recovery readily takes place provided the metrazol injury is not

too great Single epithelial cells, or groups of epithelial cells, sometimes suffered death in animals which nevertheless recovered quickly.

Connective Tissue The most usual change displayed by an irritated connective tissue cell is vacuolation. Quick recovery ensues if the cell is not too badly injured. Occasionally, a fine example of process retraction may be seen (Fig 14) Ordinarily this is accompanied by the formation of knob-like excrescences along the retracting processes In a general way the appearance of a retracting connective tissue cell process is quite similar to that of a retracting ending of a nerve fiber or of a capillary sprout (*cf* Speidel, '33) Following a period of irritation as adjustments take place, the connective tissue cells may move and shift their positions markedly

Endothelium.—The endothelial cells of both blood and lymph capillaries sometimes exhibit vacuole formation as a result of strong metrazol treatment (Figs 16 and 17)

Muscle—In many of the metrazol-treated tadpoles the fibers of striated muscle in the tail exhibit conspicuous changes in structure. The changes caused by metrazol are entirely similar to those caused by other injurious agents. In other papers I have presented a detailed account of muscle fiber injury and recovery in frog tadpoles (*cf* Speidel '38 and '39). Metrazol-treated tadpoles which displayed marked nerve changes also usually displayed definite muscle fiber injuries A few of the more common types of injury are illustrated somewhat diagrammatically (Figs 19 to 22) The most common one was that characterized by a clot at both (or at one) of the muscle-tendon junctions (Fig 19) Fibers in this condition readily recover after a couple of days, replacing the clot with new cross striæ. The fiber of Fig. 20 represents a stage preliminary to the repair of a fiber which previously had clotted at each end. A fiber injured as badly as that of Fig 21 is still capable of regenerating within the old sarcolemma, after losing its cross-striated organization, provided that not all of the muscle nuclei have been killed; or a fiber of this type may fail to regenerate successfully and become lost entirely. A fiber injured as badly as that of Fig. 22 does not regenerate. The cross-striated substance has been transformed into waxy degenerative clots (Zenker's degeneration).

CINÉ-PHOTOMICROGRAPHS OF NERVES AND OTHER TISSUES OF METRAZOL-TREATED TADPOLES

Many ciné-photomicrographs of nerve fibers and other tissues of metrazol-treated tadpoles have been obtained.³ These are mostly of the fast-motion type, which reveal very slow movements. The pictures are taken directly from the living animal usually with the aid of an oil-immersion objective lens. They reveal the finer details of the changes in the cells and fibers. Some pictures are taken at the normal rate. These afford an excellent record of day-to-day changes in location and structure. These motion pictures reveal in vivid fashion many of the changes in tissues during metrazol injury and recovery which have been described in the preceding pages. The subjects of some of the pictures follow

- 1 Examples of mild, moderate, and severe metrazol injury of myelinated nerve fibers, and their recovery on following days

- 2 Successive metrazol injury and recovery of the same myelin segment over a period of several days

- 3 Retraction of a nerve ending of a myelinated fiber over a period of a half hour

- 4 Changes in shape of nerve endings in metrazol-treated tadpoles.

- 5 History of the same four nerve endings through three successive metrazol treatments and recovery

- 6 History of the degeneration and regeneration of a side branch at a node of Ranvier and of a nerve ending belonging to it

7. History of irritation of a side branch at a node of Ranvier, its recovery, growth, and the genesis of new branches following a metrazol treatment severe enough to induce myelin degeneration distal to the node of Ranvier under observation

- 8 Examples of the effects of metrazol treatment on growth cones of regenerating fibers during the 4th, 5th, and 6th days of regeneration. Recovery of irritated swollen growth cones and their resumption of growth outward toward the skin

- 9 Vacuolation of an irritated unmyelinated nerve fiber

- 10 Lymphocyte migration within the neurilemma of an unmyelinated fiber traveling past a nerve fork and sheath cell, with intermittent protrusion of pseudopods

11. Formation of cutaneous vesicles in the tail fin, followed after 35 minutes by rapid elimination of the vesicular fluid and movements of the related epithelial cells.

12. Rapid readjustment of the epithelial cells related to a former vesicle, 3½ hours after the termination of a metrazol treatment.

13. Epithelial cell mitoses in recovering epithelium, 1 to 4 days after metrazol treatment.

³ Some of these were exhibited at the meeting of the American Philosophical Society at Philadelphia on November 18, 1939

14 Lack of movement of epithelial cells on the day after injury, contrasted with rapid movement a few hours after injury

15 Movements of the sub-epithelial tissues during recovery from metrazol injury

16 Abnormal epithelial evaginations several days after the injury induced by metrazol treatment

17 Abnormal double tail axes six weeks after the injury induced by severe metrazol treatment

18 The induction of retraction caps on muscle fibers in metrazol-treated tadpoles and recovery of the same fibers on the following day

19 Degeneration and elimination of muscle fibers during and following severe metrazol injury

20 The condition of injured muscle fibers 2½ hours after metrazol treatment, and the recovery of the same fibers on the next day

21 A connective tissue cell recovering with extension of processes following a metrazol treatment severe enough to cause retraction of processes

22 Changes in the sub-epidermal vacuolated cells following metrazol treatment

23 Marked circulation reaction from metrazol treatment characterized by slow speed of blood flow in most of the tail and by almost complete cessation of blood flow near the outer fin edges

MECHANISM BY WHICH METRAZOL MAY EXERT ITS EFFECT ON HUMAN MENTAL PATTERNS

The foregoing observations reveal that metrazol treatment may induce various degrees of general tissue injury in frog tadpoles. The changes exhibited by most of the tissues need not be particularly discussed. The changes in the endings of nerve fibers, however, are of special significance. They suggest at least a part of the mechanism by which mental patterns in man may be affected by metrazol treatment.

Mental activities, as well as nervous activities in general, are mediated by chains of nerve cells. The axonic endings of one nerve cell touch the dendritic endings (or processes, or cell body) of the next cell, thus forming the synapse. The synapse is the most vulnerable link in the chain. My observations show that metrazol may cause retraction of peripheral nerve endings and in extreme cases loss of appreciable lengths of nerve fiber with whole clusters of nerve endings. It seems probable that synaptic nerve endings centrally located are similarly affected by metrazol treatment. (Unfortunately no synapses are available for direct microscopic observation in living tadpoles.) Retraction or loss of nerve

endings at some synapses in the brain might account for the shakeup and confusion of mental patterns in a patient immediately following a metrazol treatment. Reextension or regrowth of nerve terminals as recovery ensues might result in a new pattern of nerve endings and new synapses.⁴ Thus, an anatomical basis would be afforded for the establishment of new pathways and of new mental patterns. On this basis, therefore, the improvement or change in human mental conditions brought about by metrazol treatments is correlated with (1) the breaking down of some of the old synapses as nerve endings retract or degenerate, and (2) the establishment of new synapses as new nerve endings develop.

RELATED CONSIDERATIONS

A few additional comments may be made on the general subject of changes in nerve structure. In the first place, it is not surprising that the nerve ending should be readily affected by treatments causing irritation or injury. In any cell with a long process the part farthest away from the nucleus is most likely to suffer regressive change (*i. e.*, retraction or degeneration) when the immediate environment becomes unfavorable. In frog tadpoles I have observed this not only for nerve cells, but also for connective tissue cells, pigment cells, endothelial cells of both blood and lymph capillary sprouts, and special sub-epidermal cells with long vacuolated processes. The reactions of nerve endings in metrazol-treated tadpoles, therefore, resemble similar changes in other cells with long processes.

In the second place, there is nothing specific about the structural changes exhibited by nerve fibers in response to metrazol treatment. The same changes have been seen after other treatments that cause injury. In a previous paper (Speidel, '36) I have recorded in great detail the nerve changes that accompany alcoholic intoxication and recovery. Similar nerve changes may

⁴ I consider it very likely also that new collateral sprouts might be evoked from uninjured nerve cells and fibers adjacent to the injured territory. These, too, might contribute to the establishment of new synapses. I do not mean to exclude the possibility that some of the effects of the metrazol may be ascribable to irritative changes in other regions of the nerve cells, such as the myelinated fiber portion or the main cytoplasmic body. Nevertheless, in accounting for the changes in mental activities I believe the changes in nerve endings and synapse zones are of greatest significance.

In general, it is known that extensive nervous regeneration does not take place in the brain after injury. There is ample evidence (Cajal, '28), however, in support of my view for nerve ending recovery in the brain, as well as for the growth of collateral endings from neighboring cells and fibers.

be induced with the electric current, heat, and various chemical agents. Furthermore, during normal growth and development there is a continual adjustment of nerve endings. I have watched individual cutaneous clusters of endings in tadpoles for more than a month while the animals increased greatly in size. Day-to-day records revealed clearly that extension, retraction, and autotomy occurred in individual endings, correlated with the changing stresses, strains, and other features of the environment (Speidel, '39). It seems probable to me that a certain amount of nerve ending readjustment may occur at synapses in the brain under normal conditions, even without a strong irritation such as is induced by metrazol treatment.

In the third place, synapse zones vary greatly in structure and probably in susceptibility to injury. From the anatomical standpoint some would suffer disruption much less readily than others. For example, in the cerebellum some synaptic zones are characterized by complex intertwining of processes of the two nerve cells concerned. Obviously such a synapse would resist disjunction more than one formed between cells whose endings merely touch one another.

From the physiological standpoint I should also expect that synapses concerned in the proper functioning of vital bodily activities, as for example in the regulation of the heart beat, would be more resistant to destruction than synapses concerned with the higher mental activities. This point of view is perhaps in line with some observations reported by Sakel ('38b). He points out that in insulin-treated patients, as hypoglycemia deepens into coma, reflexes are abolished according to their rank in the evolutionary scale. Thus, the cremaster reflex is abolished early, then the Babinski sign, and last the corneal reflex. In like manner the temperature regulating center, a late acquisition, is early attacked, the respiratory center is last to be attacked. With the rise of blood sugar during recovery of the patient, the reflexes appear again in the reverse order. Sakel is probably correct in his theory that the most recently acquired pathways are the most susceptible to injury.

A final comment may be added concerning multiple metrazol treatments in relation to the differential susceptibility to injury of nerve endings at rest and nerve endings in process of regeneration. In the frog tadpole a regenerating nerve fiber with its growth cone tip seems more susceptible to irritation and injury than does

a fiber at rest. It follows that if an early metrazol treatment is sufficient to induce injury and subsequent regeneration, a second treatment within a few days could then act on the nerve endings in their more susceptible regenerating stage to bring about further retraction or injury

SUMMARY

1 Immersion of living frog tadpoles in dilute solutions of metrazol causes tissue irritation and injury. Nerves, muscles, and epithelial cells display pronounced histopathological changes.

2 Practically any degree of neuritis may be induced, if the length and strength of metrazol treatment is suitably controlled. The structural changes in single nerve fibers have been directly observed in living animals in minute detail during both irritation and recovery

3 Moderately injured myelinated fibers exhibit typical irritative changes. These include vacuolation, microfibrillation, swelling, myelin globule formation, and separation of the myelin sheath and axis cylinder. Severely injured fibers exhibit fragmentation of both myelin sheath and axis cylinder. Less severely injured fibers sometimes exhibit fragmentation of the myelin sheath without accompanying destruction of the axis cylinder. Distal myelin segments are more susceptible to injury than proximal ones

4. Terminal ameboid growth cones at the tips of regenerating fibers react quickly and conspicuously to metrazol treatment. Growth ceases, the growth cone becomes transformed into a retraction club, and a variable amount of retraction may take place. Quick recovery follows replacement of the animal in pond water. A new growth cone develops at the tip and extension follows, either along the former pathway or along a new one. Second, third, and later growth cones along a regenerating nerve behave similarly.

5. Irritated cutaneous nerve endings may exhibit swelling, retraction, and occasionally autotomy. Recovery follows quickly after normal conditions are restored. The processes of retraction and recovery are essentially like those of irritated growth cones though they are less conspicuous.

6. Severe metrazol treatments sometimes cause loss of the distal portion of a nerve fiber which may include some myelin segments and some clusters of cutaneous nerve endings. Following

regeneration of the fiber new endings may develop which are quite different in pattern from the original ones.

7. Exact duplication in nerve fiber regeneration corresponds only to the extent of the neurilemma which enclosed the degenerating fiber. Nerve endings are naked without either neurilemma or myelin sheath. No pathway, therefore, is left after their degeneration to guide regenerating endings. New patterns result.

8. Metrazol treatments of moderate severity may cause myelin sheath degeneration on each side of a side branch, even though the side branch with its cluster of cutaneous endings survives.

9. The observed changes in cutaneous nerve endings during metrazol injury and recovery make it seem probable that similar changes take place in synaptic nerve endings within the central nervous system. This further suggests an anatomical basis for metrazol-induced changes in the mental outlook of human patients suffering from various mental disorders. The improvement in human mental conditions sometimes brought about by metrazol treatments would be correlated with the breaking down of some synapses in the brain as nerve endings suffer injury, followed by the establishment of new and different synapses as new nerve endings develop.

10. Muscle fibers are strongly stimulated by metrazol treatment. They may exhibit various grades of injury, such as retraction-cap formation at the muscle-tendon junction, full retraction with tearing at the muscle-tendon junction, and complete disorganization with multiple discrete clotting throughout the fiber (Zenker's degeneration). Severely injured fibers suffer complete degeneration and removal, moderately injured fibers may recover and repair themselves within one to several days.

11. Large epithelial vesicles often characterize tadpoles injured by immersion in metrazol solution. Elimination of the vesicular fluid and rapid readjustment of the related epithelial cells follow restoration of normal conditions. Vacuolation of individual cells is also common.

12. Irritative changes are sometimes visible in the connective tissue and endothelial cells.

13. The general circulation is markedly affected by metrazol treatment. The rate of heart beat decreases. The speed of blood flow decreases and in the peripheral portions of the tail fin it may cease entirely. Recovery follows restoration of normal conditions.

14. Illustrative ciné-photomicrographs of both the normal and fast-motion types have been obtained from metrazol-treated tadpoles. These pictures record the principal changes in individual nerve fibers and other tissues during the periods of irritation, injury and recovery.

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EXPLANATION OF PLATES

PLATE I

FIGS 5 and 6 Retraction and recovery of regenerating nerve endings following metrazol treatment Tadpole no 2223, regenerating zone 4 days after tail section, subjected to 3 per cent metrazol from 12:02 P M to 12:10 P M.

FIG 5 At 12:00 M two growth cones were observed actively advancing At 12:10 after metrazol treatment the growing tips assumed a spherical contour A varicosity appeared on the upper fiber. At 12:17 the fibers became thicker, varicosities appeared, and slight retraction took place The tip of the lower fiber was characterized by a typical retraction club At 12:24 the upper fiber showed distinct signs of recovery with the tip about to resume its growth At 2:30 it had grown and divided as shown The lower fiber also exhibited some growth At 4:30 the fibers exhibited further extension with typical active growth cones at their tips

FIG 6 At 12:01 P M an active growth cone was noted advancing toward the skin (This nerve fiber tip was located farther distally but nearer the central axis of the tail, in relation to the fiber tips illustrated in Fig 5) At 12:18 following metrazol treatment the tip became rounded and varicosities appeared At 12:22 the fiber ended in a pointed filament as retraction took place At 12:25 the fiber exhibited slight further retraction, but within a few more minutes it began to grow out again At 2:35 a new branch was present reaching out almost at right angles to the former path This proved to be temporary At 4:35 the fiber tip was again visible extending along the former pathway (A second treatment of 3 per cent metrazol was given to this tadpole from 4:36 P M. to 4:45 P M Some retraction again took place in this ending and in the endings of Fig 5 The animal, however, did not survive)

PLATE I

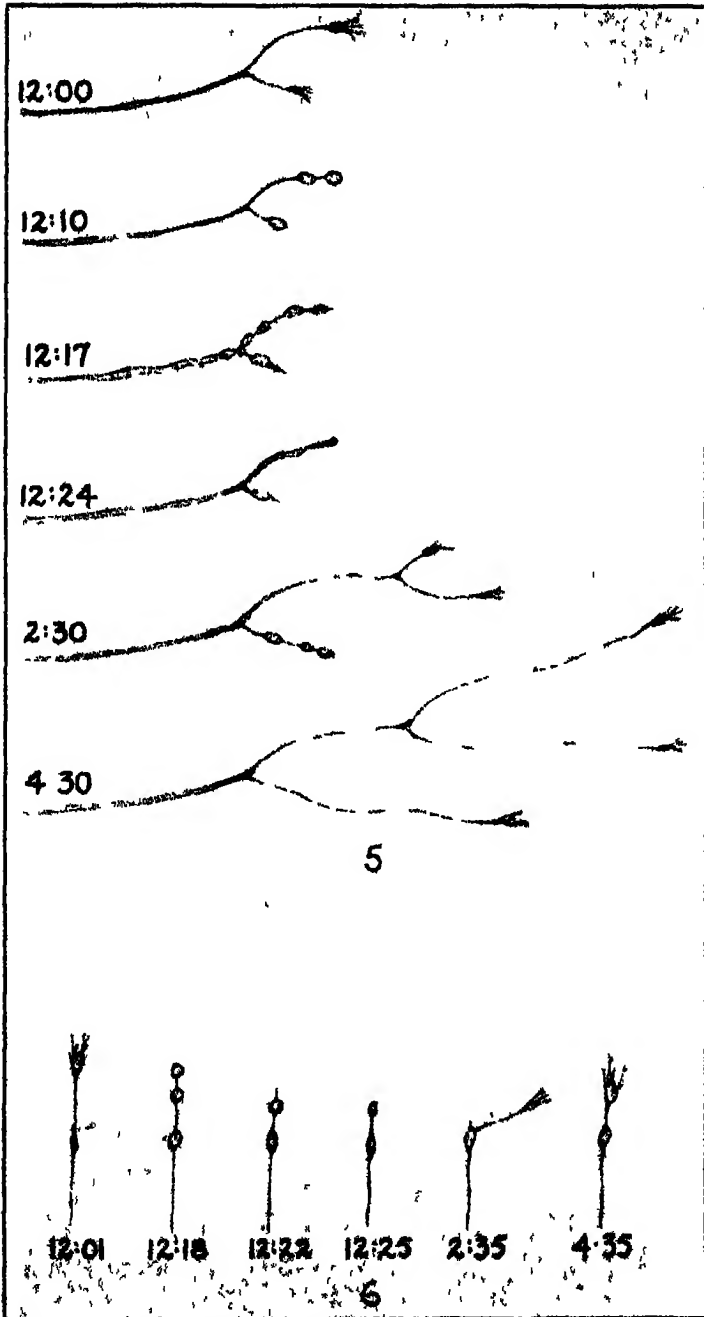


PLATE II

FIG 7 Effects of three successive metrazol treatments on the first and later growth cones of regenerating nerves. Tadpole no 2224, regenerating zone 5 days old, subjected to 2 per cent metrazol on March 15th from 10 13 A M to 10 33 A M, also from 3 05 P M, to 3 25 P M, and on March 16th from 9 12 A M to 9 32 A M. At 10 10 A M on March 15th four active growth cones (*P*, *Q*, *R*, and *S*) were visible along a small nerve. At 10 40 following the first metrazol treatment, the short branch ending in *R* retracted, the other growth cones became rounded. At 11 25 after the tadpole had been in pond water for a period, growth cones were again visible. At 3 00 P M *P*, *Q*, and *S* were active, the latter two having definitely advanced in position. At 3 30 following the second treatment, *P*, *Q*, and *S* again rounded up. At 5 30 all exhibited growth. At 9 05 A M on March 16th *P*, *Q*, and *S* had all grown to the right beyond the field illustrated, as indicated by the arrow. Growth cone *S* had joined the line laid down by *P* and *Q*. Two new growth cones (*T* and *U*) had advanced from the left into the field illustrated. A sheath cell of Schwann (*M*) also slowly advanced along the nerve. (At 9 35 following a third metrazol treatment, the growing tips again rounded up. A connective tissue cell located at *K* exhibited retraction of its processes with accompanying vigorous formation of knob-like excrescences. This cell is illustrated in Fig 14.)

PLATE II

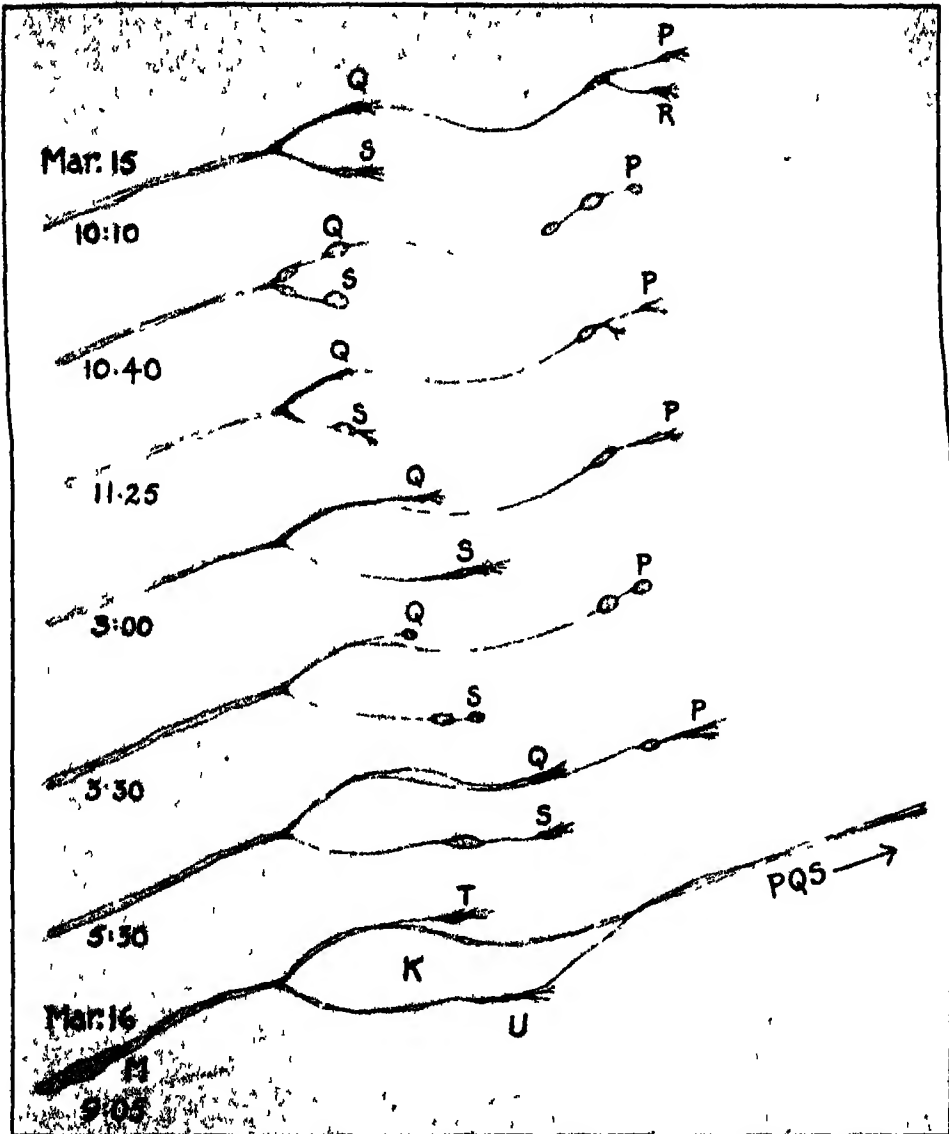


PLATE III

FIG 8 Growth and branching of resting cutaneous nerve endings following a metrazol treatment severe enough to cause myelin sheath degeneration in some segments of the nerve fiber from which the endings arose Tadpole no 2257, subjected to 2 per cent metrazol on May 5th from 11:23 A M to 11:48 A M A motion picture record of this case was obtained On May 5th at 11:20 a normal myelinated fiber was visible with a side branch at a node of Ranvier At 12:03 following metrazol treatment the fiber exhibited acute irritation with vacuoles developing beneath the myelin sheath On May 6th the myelin sheath above (proximal to) the side branch was still present, that below had disappeared Two cutaneous endings (*P* and *Q*) were in the positions shown On May 8th both *P* and *Q* had grown and a new ending (*R*) had advanced to a point between them On May 10th another new ending (*S*) appeared as a result of branching of the fiber ending in *P* The ending, *Q*, also grew out farther

PLATE III

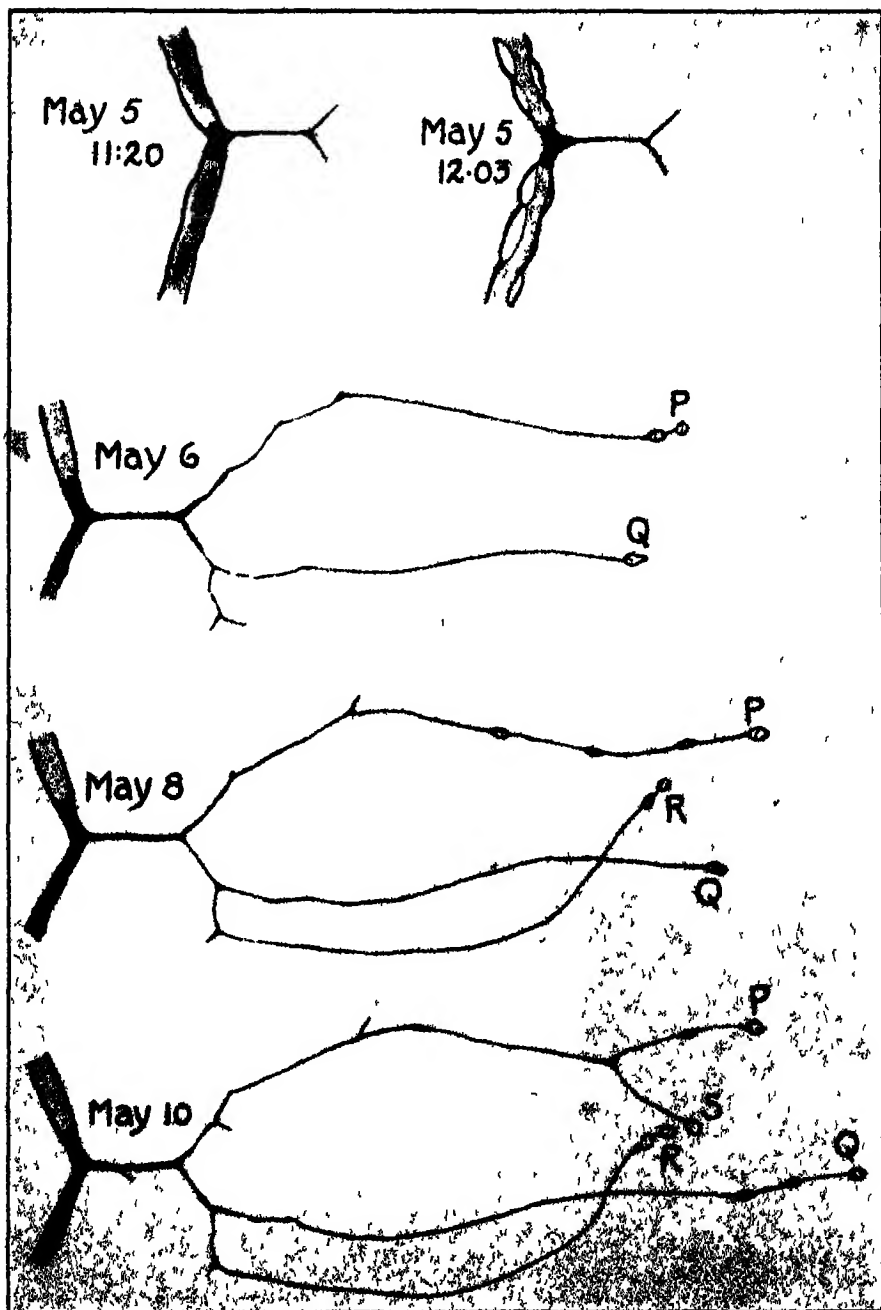


PLATE IV

FIG 9 Metrazol-induced degeneration of a cluster of cutaneous nerve endings and a part of the myelinated fiber from which they arose, followed by regeneration leading to a new pattern of nerve endings. Tadpole no. 2254, subjected to 2 per cent metrazol on May 2nd from 4:47 P.M. to 5:02 P.M. and to 2.5 per cent metrazol on May 5th from 10:58 A.M. to 11:08 A.M. On May 3rd a side branch emerging from a node of Ranvier terminated in a cluster of endings, one of which is shown. On May 4th this ending had advanced. On May 5th at 10:13 it had retracted approximately to its former position. Later on May 5th at 11:15 following a second severe metrazol treatment it rapidly began to exhibit the changes of acute injury, becoming swollen at certain points and tenuous at others. Partial or total degeneration of many muscle fibers was also apparent at this time. On May 6th the nerve fiber had undergone degeneration to a point several myelin segments away in a proximal direction. No trace of the cluster of endings could be seen. However, at the former node of Ranvier a short stump was still visible. This represented a funnel-shaped extension of the neurilemma with enclosed granules, the remains of the former side branch. Globules and granules of degeneration replaced the former myelin segments. On May 7th the regenerating fiber grew out at the exact locus of the former side branch. The pattern of branching, however, was quite different from the original pattern. The three nerve tips ended in different positions. There was no ending corresponding to the one of the preceding sketches.

PLATE IV

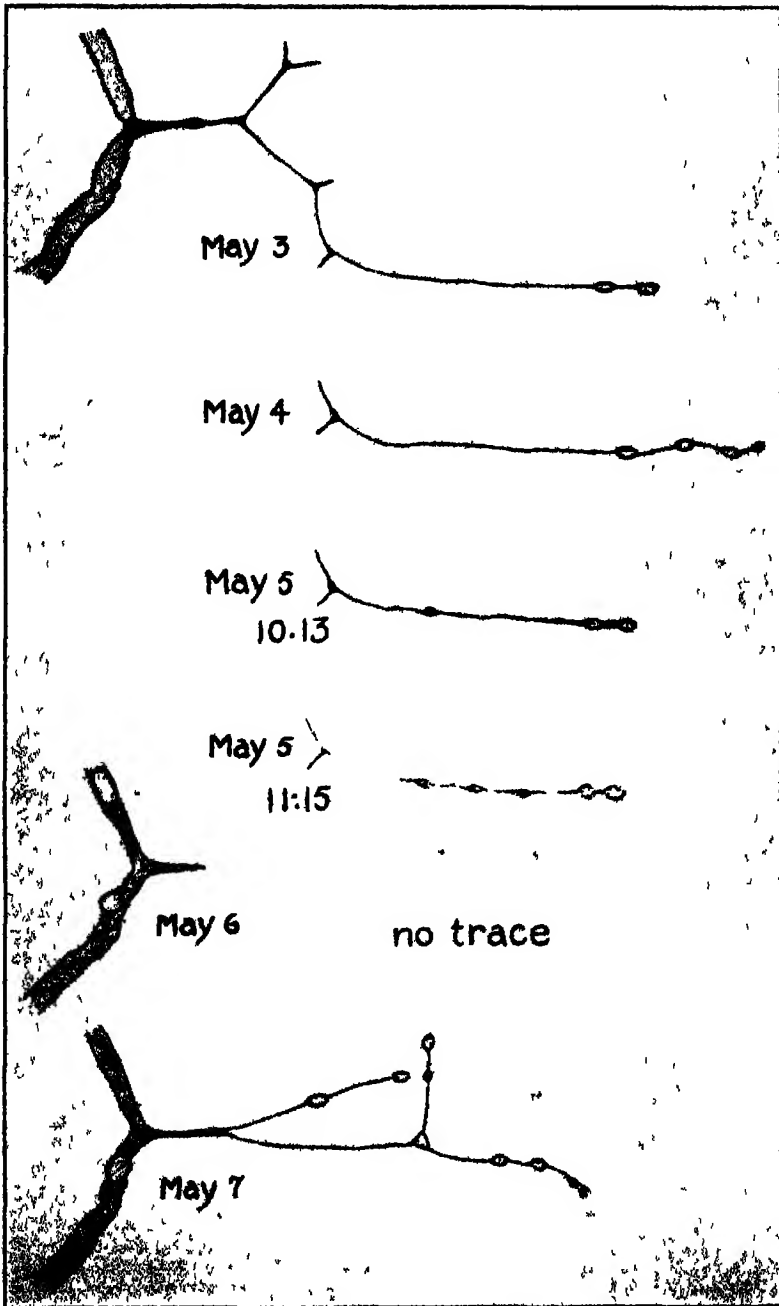


PLATE V

FIG 10 Metrazol-induced irritation and recovery of a myelinated nerve fiber Tadpole no 2254, subjected to 2 per cent metrazol on May 2nd from 4 47 P M to 5 02 P M. The sketches are drawn exactly to scale from motion picture records. On May 2nd at 5 30 P M the myelinated fiber exhibited vacuoles, located chiefly between the myelin sheath and the axis cylinder. On the following morning, May 3rd, the vacuoles were gone and the fiber was essentially normal again except for a few globules and granules of myelin along the fiber. (This same myelin segment exhibited similar irritation from a second metrazol treatment on May 5th, and a similar recovery on May 6th.)

FIG 11 Changes in a retracting nerve ending after metrazol treatment. Tadpole no 2233, subjected to 2 per cent metrazol for 20 minutes. The sketches are drawn exactly to scale from a motion picture record. At 12 10 P M, a few minutes after the termination of the metrazol treatment, a cutaneous nerve ending was watched slowly retracting, its tip already in the form of a retraction club. At 12 40 it had retracted a slight but definite amount. A few varicosities also appeared along the fiber.

FIG 12 Grades of injury to myelin segments correlated with their proximal or distal location and with their superficial or deep position. Tadpole no 2219, subjected to 2.5 per cent metrazol on March 13th. On March 14th as the tadpole recovered, a myelinated fiber was seen in which the proximal segments were recovering while the distal segments were degenerating. Segments *P* and *Q* and all others on this fiber to the left of *P* (not shown in the illustration) recovered in spite of the strong irritation. Segments *R*, *S*, and one other to the right (not illustrated) suffered myelin sheath degeneration. Segment *Q* also lost about one-fourth of its myelin sheath, that part nearest segment *R*. On an adjacent nerve fiber which, however, was more deeply located, the segments *K*, *L*, and *M* survived the metrazol treatment, though small globules were cut off from the myelin sheath as a result of the strong irritation.

FIG 13 Irritative changes in a small unmyelinated nerve after metrazol treatment. Tadpole no 2219 (the same animal and the same treatment described for Fig 12). Small vacuoles and granules were discernible in the nerve as a result of severe metrazol treatment on the preceding day.

FIG 14 Retraction and recovery of processes of a young connective tissue cell following metrazol treatment. Tadpole no 2224, regenerating some 6 days old, subjected to 2 per cent metrazol on March 16th from 9 12 A M to 9 32 A M. At 9 05 a fairly normal young connective tissue cell was noted (at region *K* in Fig 7). At 9 37 after metrazol treatment some of the processes of this cell were undergoing retraction. Rounded excrescences were formed on these as retraction proceeded. Within 8 more minutes, however, at 9 45 the retraction ceased, and the processes began again to grow out. At the same time the cell began to move slowly distally in the direction indicated by the arrow. (Nerve sprout reactions in this same tadpole are described in Fig 7.)

FIG 15 Effects of metrazol on resting and dividing epithelial cells of the skin. Tadpole no 2226, subjected to 2 per cent metrazol from 10 31 A M to 11 04 A M. Following the treatment at 11 25 A M an epithelial cell in mitosis was noted almost completely separated by a fluid space from the neighboring resting cells. Almost without exception the resting cells in the vicinity exhibited one or more vacuoles in their cytoplasm as a result of the metrazol irritation. (The vacuolated capillary sprout of Fig 17 was present in this tadpole.)

FIG 16 Effects of metrazol on the endothelium of a blood capillary. Very small tadpole no 2202, subjected to 1 per cent metrazol for 5 minutes. One hour after treatment several vacuoles were discernible in the endothelium of a blood capillary near an endothelial cell nucleus. Three nerve endings each with a clear vacuole were also visible near this capillary (though not exactly in the relative positions shown in the illustration).

FIG 17 Vacuolation of a solid endothelial sprout following metrazol treatment. Tadpole no 2226, subjected to 2 per cent metrazol from 10 31 A M to 11 04 A M. In a long capillary sprout which had not yet acquired a lumen many small vacuoles of irritation were discernible 25 minutes after metrazol treatment.

PLATE V

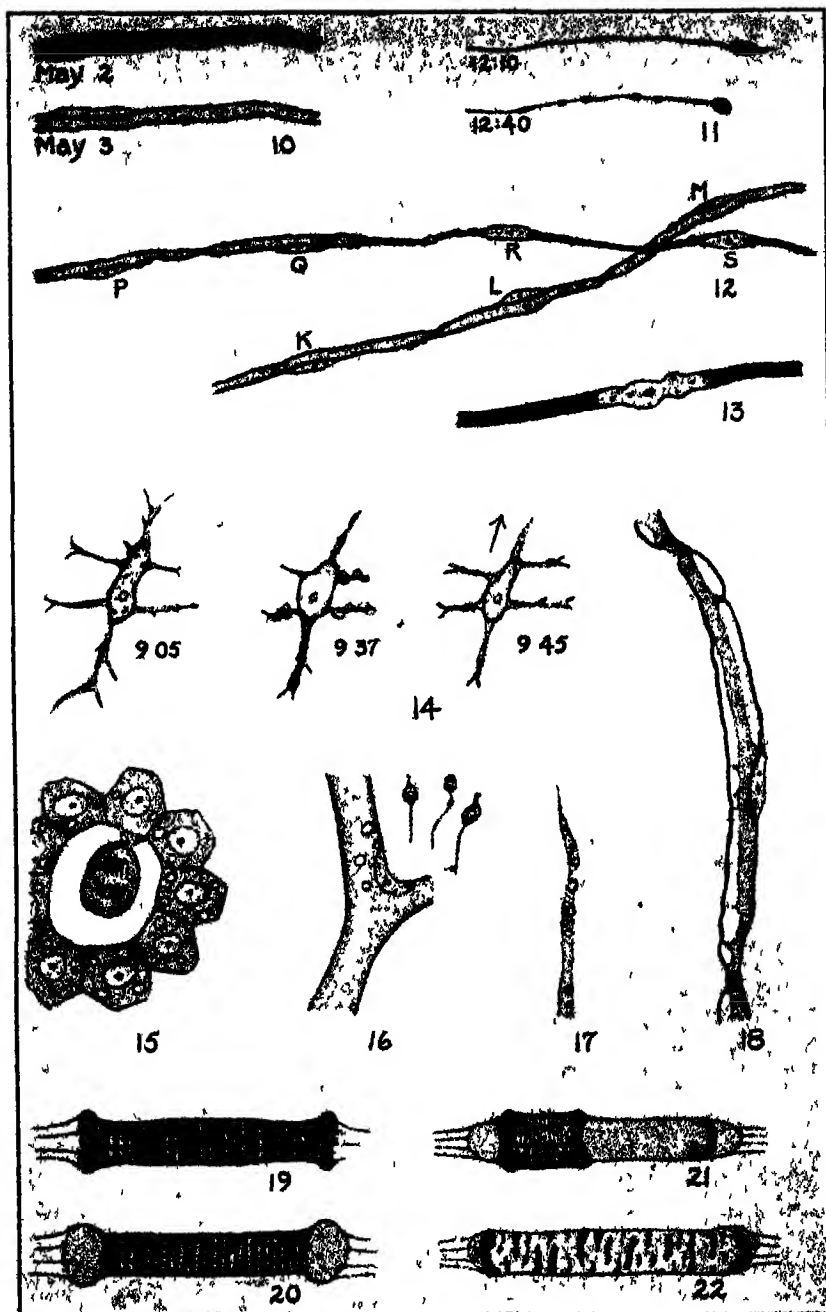


FIG 18 Severe irritation of an internodal segment of a myelinated nerve fiber following metrazol treatment Tadpole no 2237, subjected to 3 per cent metrazol from 12:05 P M to 12:10 P M The sketch drawn exactly to scale from a motion picture record shows the condition of a greatly swollen fiber at 12:40 P M The diameter is nearly double that of the normal fiber Fluid has accumulated in the space between the axis cylinder and myelin sheath

FIGS 19 to 22 Diagrams showing some injury effects of metrazol on fibers of striated muscle in the tail of the tadpole

FIG 19 A short muscle fiber which has suffered a slight injury with transformation of a few cross striæ at each end (i e, the muscle-tendon junctions) into a hard clot ("retraction cap")

FIG 20 A muscle fiber in which a former retraction cap at each end has become dissolved and replaced by fluid This condition precedes the invasion of the end zones by undifferentiated myoplasm from the uninjured part of the fiber. New cross striæ will then be formed as the fiber repairs itself

FIG 21 A muscle fiber which has suffered complete retraction The resting cross striæ have been transformed into contraction bands Three retraction caps are discernible, one of which is isolated Fluid fills much of the space within the sarcolemma

FIG 22 A muscle fiber which has suffered complete disorganization with fragmentation into multiple discrete clots as the result of severe injury

ON THE NEUROHUMORS OF THE COLOR CHANGES IN CATFISHES AND ON FATS AND OILS AS PROTECTIVE AGENTS FOR SUCH SUBSTANCES

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(Read April 18, 1940)

ABSTRACT

The common catfish ranges in its color changes from a coal black to a pale greenish yellow. The time interval for these changes is from one to two days. The receptors concerned are the skin and the eyes. When light falls upon the skin, certain photoreceptors are stimulated and thereby the pituitary gland is excited to discharge intermedin. This is carried by the blood to the melanophores which then disperse their pigment and the fish darkens. When light from a dark surface enters the fish's eye, autonomic dispersing nerve fibers become active and liberate at their terminals a neurohumor which also causes the dispersion of the melanophore pigment and the consequent darkening of the fish. When light from a white surface enters the eye, autonomic concentrating nerve fibers are activated and their terminals discharge a neurohumor which induces a concentration of melanophore pigment whereby the fish becomes pale. The illumination of the eye from a white surface indirectly inhibits the discharge of intermedin from the pituitary gland. The color changes of the catfish are therefore dependent upon three substances,—intermedin, a dispersing neurohumor, and a concentrating neurohumor. The chemical nature of intermedin is not known. On investigation, the dispersing neurohumor proves to be acetylcholine and the dispersing nerve fibers may therefore be classed as cholinergic. The concentrating neurohumor appears to be adrenaline, but this determination is not so certain as that of the acetylcholine. If the concentrating substance is adrenaline, the nerve fibers from the terminals of which it comes may be classed as adrenergic. Thus, the melanophores of the catfish would have a double innervation like that of the heart muscle in the higher vertebrates and this innervation would agree in essentials with the typical autonomic innervation of a number of other tissues.

1. INTRODUCTION

THE chief organs concerned directly with the color changes of the common catfish, *Ameiurus nebulosus*, are three, the pituitary gland and two sets of autonomic nerves one dispersing and the other concentrating (Parker, 1934a). These have been admitted in part or in whole by all recent workers who have studied with any degree of fulness the color changes in this fish (Abramowitz, 1935, 1936; Odiorne, 1937; Veil, 1937, 1938; Osborn, 1938a, 1938b; Wykes, 1938). In addition to the activating agents from these three sources an adrenalin-like excitant has been suspected (Abramowitz, 1936) as well as others all of which

will be discussed in this communication. These additional agents, however, appear to play very subordinate rôles in catfishes as compared with the three chief agents already noted and certainly exert at most only a minor influence on the color changes in this fish

As is well known these changes in the catfish range from a coal-black to a translucent, pale, greenish yellow. All the chromatophores in this fish are melanophores, micromelanophores in the epidermis and macromelanophores immediately under the derma. The color changes depend chiefly upon the macromelanophores the pigment of which, when concentrated, has the form in each color cell of a sphere some 40 to 45 microns in diameter. In the dispersed state this pigment spreads through the branched processes of the cell which extend over what is usually a broadly oval area with a major axis of 140 to 145 microns. Micromelanophores with concentrated pigment resemble under the microscope black dots the diameters of which are about 12 to 15 microns. In the dispersed state some of the pigment of the micromelanophores passes out into their relatively few, very narrow, elongate processes which cannot be said to cover any very definite area but which often form with the processes from adjacent micromelanophores what appears to be an extended network. As indicators of melanophore activity the micromelanophores are less satisfactory than the macromelanophores.

It is now known (Parker, 1935a) that the skins of dark catfishes contain a substance which when extracted with olive oil and injected under the skins of pale catfishes produces a dark spot resulting from the dispersion of melanophore pigment. This substance is believed to be a neurohumor from the terminals of the dispersing nerve-fibers and to be the means whereby these fibers induce the dispersion of the melanophore pigment. There is indirect evidence that an analogous material is produced by the terminals of the concentrating nerve-fibers whereby the melanophore pigment is made to withdraw into the body of the color cell itself. Thus beside the pituitary secretion, intermedin, there may be two other neurohumors involved in the color changes of the catfish one from the dispersing and the other from the concentrating nerve-fibers. These two neurohumors show evidence direct and indirect of solubility in oils or

fats and have therefore been called lipohumors (Parker, 1935a) in contrast with such neurohumors as intermedin and the like which because of their solubility in water have been called hydrohumors. It is the object of this paper to discuss the evidence as to the actual occurrence of these substances in *Ameiurus* and to ascertain, if possible, something of their nature.

2. THE PITUITARY NEUROHUMOR (INTERMEDIN)

Odiorne's suspicion (1933) that the normal darkening of *Ameiurus* involves a pituitary hormone was justified by tests on hypophysectomized catfishes (Parker, 1934a). After the removal of the pituitary glands by a relatively severe procedure I found that the caudal bands in these fishes failed to darken as they did in normal *Ameiurus* thus giving evidence of a blood-borne, darkening neurohumor which disappeared with the loss of the pituitary gland. In consequence of the severity of the operation these hypophysectomized fishes lived at most only a little over two days and therefore the full effect of the loss of their pituitary glands was not seen by me. The operation of hypophysectomy in *Ameiurus* was greatly simplified and improved by Abramowitz (1936, 1937) and as a result it became possible to keep operated fishes actively alive for months after the loss of the glands. The same seems to be true of *Parasilurus* (Enami, 1939).

After the removal of the pituitary gland from *Ameiurus* the intermedin in its blood does not at once disappear. Osborn found traces of this substance in catfishes some 70 hours following the operation. In my experience these traces have fully disappeared from the blood of this fish in from 5 to 7 days after the loss of the gland. From that time on one can be reasonably certain that no physiologically significant amount of intermedin remains in the fish.

Such fishes are found to be much limited in the extent of their color changes (Osborn, 1938a, 1938b; Wykes, 1938; Enami, 1939). In specimens tested by me their macromelanophores can expand at most to a diameter of about 100 microns which is somewhat greater than 70 microns, the limit given by Osborn. Both these estimates, however, fall well below the maximum for normal fishes, about 140 to 145 microns. Macromelanophores in hypophysectomized catfishes are also incapable of full pig-

ment concentration. In my tests the pigment masses in the color cells of pale, hypophysectomized fishes from white backgrounds had a diameter of about 55 microns or some 10 microns less than full concentration, a condition which resulted in the incomplete blanching of such fishes. This whole phenomenon seems to have escaped the attention of other investigators except Veil (1938) and Enami (1939). It will be discussed in a subsequent part of this paper.

When the almost colorless defibrinated blood of a dark catfish is injected under the skin of a pale fish a temporary dark spot is produced, but no color reaction is elicited when such blood is injected under the skin of a dark individual (Parker, 1934a). Conversely, when the blood of a fully pale fish that has been kept pale for some ten days is injected into either another pale fish or into a dark one no color change can be seen in either recipient

Aqueous extracts of the pituitary gland of the catfish when injected into pale catfishes cause darkening but they have no color effect on dark fishes (Odiorne, 1933, Parker, 1934a). These tests have been extended by Osborn (1938b) to hypophysectomized catfishes kept so long after the operation as to have lost all traces of pituitary materials and with results confirmatory of the earlier observations. Veil (1938) has demonstrated that *Ameiurus* will darken when there is introduced into its body one-thousandth of a milligram of beef intermedin per 100 grams of fish. All these observations collectively show that a neuro-humor from the pituitary gland of the catfish and of other animals is an extremely effective agent in darkening these fishes in that it induces an excessive dispersion of their melanophore pigment. That it brings about this change in fully denervated caudal bands the nerve elements of which have been allowed to degenerate completely shows further that this substance must act directly on the melanophores themselves and not through surviving nerve fragments or nerve terminals. The nature of this substance, commonly designated as intermedin, is in the main unknown. That it is freely soluble in water, acts effectively in extreme dilutions, is heat-stable, and is resistant enough to remain in the blood of a catfish for some days after its source, the pituitary gland, has been removed may be concluded from what has been stated.

3. THE DISPERSING NEUROHUMOR

The strongest evidence in favor of dispersing nerve-fibers in *Ameiurus* is seen in the reactivation by recutting of faded caudal bands in this fish. After a caudal band is allowed to fade in a pale catfish a new cut made in the faded band and at a point a short way distal to the original cut and within the limits of the band will induce the formation of a new band extending from the new cut approximately to the edge of the tail (Parker, 1934a). This new band shows that the originally severed nerve-fibers can still be reactivated and that their function is a dispersing one. Although these tests were made on fishes that had not lost their pituitary glands, it seemed impossible since the fishes were pale and the original band was equally so, that the new band could be formed by intermedin carried in the blood. Apparently the only other way in which the new band could arise was through the reactivation of dispersing nerve-fibers. Hence such reactivation was taken as evidence of the presence of this type of fiber.

This conclusion has been called in question by Wykes (1938) and particularly by Osborn (1938b) who was unable to confirm the observations on which it rests. Osborn states that although he has tried recutting faded bands many times he has never been able to evoke a second darkening. I, therefore, repeated such tests on normal catfishes. My procedure was to cut two caudal bands in the tail of each of several normal catfishes and to blanch the fishes with their bands by keeping them some five or six days in a white-walled, illuminated aquarium. When both fishes and bands were blanched, I recut one of the bands in the way already described and cut a new third band in an unoperated part of the tail. I thus had a fish the tail of which showed three kinds of bands, an original fully faded one, an original band with a re-viving cut in it, and a wholly new band.

The appearance of such a tail is shown in Fig. 1. Here of the two original faded bands the one that remained undisturbed is of the same tint as the rest of the tail except at its tip where it remains dark (left in Fig. 1). The original band newly recut (center of figure) is darker than the faded band, and the band last cut is darkest of all (right in figure). The states of the melanophores in these several situations are shown in the accompanying photomicrographs. In the fully faded band the

pigment of the melanophores is much concentrated (Fig. 2) and quite like that in the color cells of the tail as a whole (Fig. 3). In the melanophores of the revived band (Fig. 4) the pigment is more dispersed than in the fully faded band, but not so much so as in the newly cut one (Fig. 5). It is evident that the tints of the bands as seen by the unaided eye are in essential agreement with the states of the melanophores as viewed under the microscope. And it is also evident that the recut band shows unquestionable revival as compared with the fully faded control band. This revival, however, is not so considerable as the darkening to be seen in the newly cut band. This difference is doubtless due to the first steps in the degeneration of the chromatic nerves which has probably already begun in the two bands first cut. I have made many such preparations, probably at least a score, and in all instances the recut band has shown clear evidence of revival. I therefore reiterate my former statement that in *Ameiurus* faded caudal bands may be revived by recutting. These results differ from those of Osborn. So far as I can judge this difference is probably due to the different temperatures at which the two pieces of work were done. Osborn kept his fishes in water at 12° C. Mine were in aquaria the water of which was at about 20° C. Wykes (1938) remarks that at 6° C. the color activities of *Ameiurus* are at almost full abeyance. Probably 12° C. is too low for good revival responses.

Although there is no reason to suppose that intermedin has anything to do with the revived caudal bands just discussed, the fact that this neurohumor plays a part in the conditions of the bands in *Fundulus* even when it does not influence the melanophore system of this fish as a whole (Kleinholz, 1935) led me to try the kind of tests discussed in the preceding paragraphs on hypophysectomized catfishes. Three fishes from which the pituitary glands had been removed were kept in a white-walled, illuminated aquarium for a little over a month. By the end of this period they were as nearly pale as could be. Two bands were then cut in the tails of each fish. These bands darkened only moderately as might have been expected in fishes without pituitary glands. After they had blanched considerably, one band in each fish was recut and within an hour this band had become evident by its noticeable darkness (central band in Fig. 6). The states of the melanophores in the tails of these fishes

are shown in Figs. 7, 8 and 9. The pigment in the melanophores of the undisturbed portion of the tail is much concentrated (Fig. 7), that in the unaltered band is somewhat dispersed (Fig. 8), and that in the recut band is still more dispersed (Fig. 9). Here again it is evident that recutting revives a band and in the absence of all possible pituitary influence. This revival occurred in all three fishes and thus supports the view that dispersing nerve-fibers are present in *Ameiurus*. That fibers of this kind occur in this catfish has been shown in quite a different way by Abramowitz (1935) in his work on the regeneration of melanophore nerve-fibers in *Ameiurus*. The presence of this type of nerve-fiber seems to be taken for granted by Chang, Hsieh and Lu (1939).

It is from the terminals of these dispersing nerve-fibers that in my opinion the darkening neurohumor extracted by oil from the skins of dark catfishes is obtained. This substance, whatever it may be, cannot be extracted satisfactorily from catfish skins by water, cold ether, nor by ether used in a Soxhlet apparatus with pulverized, dried catfish skins that had been heated to 100° C. It can be dissolved from the pulped skins of these fishes by means of olive oil. In consequence of its solubility in oil I have designated it and other like substances lipohumors (Parker, 1935a). What the dispersing substance in the catfish is can only be surmised, but considering the trend of modern neurophysiology acetylcholine may well be suspected (Bacq, 1937).

In pursuance of this lead, tests were made to ascertain whether acetylcholine would darken the skins of pale catfishes,¹ and whether this substance could be identified in the skins of fishes that had normally assumed the dark phase. Because of the ease with which acetylcholine is destroyed by choline esterase catfishes were prepared for these tests by a preliminary injection of eserine the protective action of which on acetylcholine is well known. It is customary to use eserine in such

¹ Much difference of opinion exists concerning the efficacy of acetylcholine as an activator of chromatophores. Barbour and Spaeth (1917), Wunder (1931), and Smith and Smith (1934) reported negative results from this reagent. Parker (1931) suspected it of producing a slight melanophore expansion, but on modifying his technique by the preliminary use of eserine he noted (1934c) a slight contraction, a view subsequently espoused by Beauvallet (1938) and by Bogdanovitch (1938). Expansion has been reported by Chin (1939) and by Chang, Hsieh and Lu (1939). These several results were obtained from a variety of fishes none of which was the catfish.

amounts that its concentration in the experimental animal is one to 50,000. But at this concentration eserine itself will induce darkening in a catfish. This is not the case when the concentration is 1 part in 100,000 to 1 part in 500,000. I therefore injected eserine into catfishes in such amounts that they contained one part of the drug to 200,000 parts of fish with the belief that this greater dilution would still be a sufficient protection for the acetylcholine. Into such a fish in the pale phase 0.1 cc. of a one per cent solution of acetylcholine in Ringer's fluid was injected. Ten minutes after the injection no change of color was to be observed in the fish, but twenty minutes thereafter the region about the place of injection was decidedly darkened and the whole fish had deepened in tone to a point midway between pale and intermediate. Half an hour after the injection the fish had become still darker, a tint which it retained for over an hour after which it gradually blanched. As many as a dozen tests of this kind were made in all of which the fishes darkened to acetylcholine. Dark fishes, treated as these pale ones had been, showed no change of color. These responses were so invariable that one is led to conclude that acetylcholine when protected by eserine will darken catfishes.

To ascertain whether acetylcholine is present in the integument of normally dark catfishes extracts from the skins of these fishes were tested with the usual leech-muscle preparation. Two dark catfish skins weighing when wet 3.84 gms. with enough eserine in 0.1 cc. Ringer's solution without bicarbonate to give a concentration to the whole mixture of one part eserine to 50,000 parts of the other ingredients were ground with sand in a mortar to a fine pulp. The whole mixture was then made up to 30 cc. by adding Ringer's solution. This was then boiled, filtered, and the resulting clear liquid (22.5 cc.) used for tests. 1 cc. of this filtrate contained active material from 0.128 gms. of wet skin. 5 cc. of this extract was then diluted with an equal amount of Ringer's solution and tested on uneserinized leech muscle with the result that only a small response was recorded (Fig. 10, *A*). The muscle was then washed and treated with Ringer's solution containing eserine 1:50,000. On applying the extract to the eserinizd muscle a large response was recorded (Fig. 10, *B*). The muscle was then washed, re-eserinized and tested with acetylcholine 10^{-9} in eserinizd Ringer's solution with a small

response as a result (Fig 10, *C*) After appropriate washings and re-esterinization, tests were carried out with acetylcholine 10^{-6} in eserinizied Ringer's solution (Fig. 10, *D*, very large response), skin extract (Fig. 11, *E*, large response), acetylcholine 5×10^{-6} (*F*, slightly greater response than *E*), skin extract (*G*, slightly less than *E*), and lastly acetylcholine 2.5×10^{-6} (*H*). It was evident from these records that the skin extract is closely matched in concentration by acetylcholine 5×10^{-6} or 0.005 gamma acetylcholine per cc. Computed from this basis it was determined that one gram of wet skin contained on the average 0.078 gamma of acetylcholine, or that acetylcholine was present in the wet skin in the proportion of about 1 : 13,000,000. I am under obligations to Dr John H. Welsh, Jr, for very material help in these determinations.

This result leads to two important conclusions. The first is that since the leech-muscle test as here carried out is a specific one for acetylcholine, it can be concluded that the dispersing lipohumor in *Ameiurus* is that substance, and second, that this substance acts at a prodigiously low concentration, about 1 : 13,000,000 as stated. It is an interesting fact that while this work was in progress Chin (1939) published the statement that when an extract from the caudal fins of *Macropodus* containing the equivalent of 0.009 gamma of acetylcholine was injected into the fish its caudal fin darkened, and Chang, Hsieh and Lu (1939) announced a determination on the snake fish, *Ophiocephalus*, similar to the one given in this paper, in which the amount of acetylcholine was stated to be 0.077 gamma per gram. Although the very close agreement of our determination with that just noted must be more or less accidental, the two determinations point very clearly to the order of magnitude of the amount of acetylcholine involved.

My early inability (Parker, 1935a) to extract the dispersing neurohumor, which I now believe to be acetylcholine, from the dark skins of *Ameiurus* with water or ether was probably due to the destruction of this neurohumor by the choline esterase contained in the crude mixture, a destruction which did not take place so freely when olive oil was present. In this oil the acetylcholine probably quickly dissolved and was thus protected from the further action of the aqueous ingredients of the general mixture. Thus carried in the oil it could be transferred as a crude

emulsion to the subcutaneous spaces in the fish whence it probably made its way slowly out of the oil and to the neighboring melanophores which were thereby induced to disperse their pigment. Such being the case, it ought to be possible to imitate this operation by making a solution of acetylcholine in oil and injecting it under the skin of a catfish to ascertain if the skin might not thereby be darkened

Acetylcholine is freely soluble in olive oil and when a 0.5 per cent solution of it in this oil is agitated in Ringer's solution and as such injected under the skin of a pale catfish, in about two hours dark spots begin to appear immediately over the region of injection. These spots are due to the dispersion of pigment in the melanophores concerned as can be seen by inspection under the microscope. Such spots remain clearly visible for some four or five hours after which they blanch. They are indistinguishable in all respects from the spots obtained by injecting an oil extract of dark catfish skin into a pale fish (Parker, 1935a) and are a perfect imitation of such spots.

In my opinion these artificial spots are due to the escape of the acetylcholine from the protecting oil drops in which this substance is carried. The acetylcholine then presumably makes its way in aqueous solution to the adjacent melanophores which are thus induced to expand. Here acetylcholine would be open to the action of choline esterase by which it would probably be quickly destroyed

Under such circumstances it ought to be possible to protect the acetylcholine and prolong and extend its action by the use of eserine. When the appropriate concentration of eserine is injected into a pale catfish on which there is a group of dark spots produced by oil globules containing acetylcholine, the dark spots in about ten minutes begin to take on a deeper hue and finally spread and may coalesce. These changes pass off in an hour or more after which the spots return to their original forms and tint. Such changes are entirely consistent with the idea that acetylcholine is stored protectively in the oil, and makes its way out slowly into the adjacent tissue. All in all this evidence supports the idea that the dispersing lipohumor in the catfish is really acetylcholine.

The protective nature of olive oil for acetylcholine in a tissue environment was further tested in the responses of catfish me-

lanophores. In devising tests to this end, horse serum kindly supplied me by Dr. T. L. McMeekin, was used as a source of choline esterase, the natural control agent for acetylcholine in living tissues. Two injection fluids were prepared. In the first acetylcholine was mixed with an appropriate amount of warm olive oil to make a solution of 0.1 per cent strength. After this mixture had been agitated for an hour to dissolve as much of the acetylcholine in the oil as possible, an equal volume of horse serum was added to it. Thus the acetylcholine in the oil was subjected to what might be regarded as a destruction environment against which the protective action of the oil could exert itself. In the second fluid which was in the nature of a control, 0.1 per cent of acetylcholine was dissolved in Ringer's solution to which was afterwards added an equal volume of horse serum. This second fluid should soon lose all traces of acetylcholine and the same would be true of the first fluid excepting in so far as the olive oil would protect the acetylcholine.

The two fluids thus prepared were then injected in the following way into pale hypophysectomized catfishes. Such fishes, as already mentioned, are only moderately pale. For the tests here contemplated three groups of three fishes each were selected. The members of each group were carefully chosen so that they were as nearly of the same tint as possible. Into the flank of one fish in each group was injected 0.2 cc. of the agitated oil mixture and into that of another fish the same amount of the aqueous solution. The third fish in each group was held as a control. In all three fishes that received the oil mixture dark spots appeared in about an hour over the region of injection and remained there for three or four hours after which they slowly disappeared. In those that received an injection of the aqueous solution no color change at all was seen. As compared with the controls neither set of fishes showed any general color change. Hence it was only in those fishes into which the oil mixture had been injected that there was a color alteration and this alteration was of a kind such as acetylcholine might produce. It therefore seems probable that where acetylcholine is carried in olive oil it is thereby protected for a considerable time against the destructive action of such agents as choline esterase and may continue to exert its darkening influence on melanophores for some hours after it otherwise would have been entirely destroyed. These

tests support the view that acetylcholine is efficiently protected when dissolved in olive oil.

From the evidence presented in this section I conclude that the dispersing lipohumor for the melanophores of *Ameiurus* is acetylcholine and that, therefore, the autonomic nerve-fibers concerned with the discharge of this substance may be classed as cholinergic fibers. In this respect my results agree with those of Chin (1939) and of Chang, Hsieh and Lu (1939).

4 THE CONCENTRATING NEUROHUMOR

The presence of concentrating nerve-fibers in the chromatophoral systems of teleosts has been admitted from very early times. The best evidence for this type of fiber is seen in the blanching of fishes when their chromatic nerves or nerve tracts are stimulated electrically. In *Ameiurus* this test was unsuccessful when first tried (Parker, 1934a), but it was finally accomplished by Abramowitz (1936), Osborn (1938b) and particularly by Wykes (1938). Evidence for an oil-soluble neurohumor, like the dispersing lipohumor just discussed, was pointed out some six years ago (Parker, 1934a). When a pale catfish with a newly cut caudal band is allowed to remain in a white-walled, illuminated vessel for some days, the dark band gradually blanches beginning on its edges and proceeding toward its axis. This well known method of fading might be due to a loss from the edge of the band into the surrounding pale field of the dispersing lipohumor by which the band had been formed or it might result from an invasion of the band by a concentrating humor from the neighboring pale field. That the latter is the true explanation has been shown in an experiment by Matsushita (1938). If in the tail of a pale catfish with a faded caudal band a new band is cut directly next one of the sides of the faded band and allowed to blanch, it will be found that this new band will fade on the side next the innervated pale field and not next that of the denervated, faded old band. It is therefore altogether probable that the new band blanches not because of a loss of its dispersing humor which could occur on both its edges, but because of the positive concentrating action of the blanching humor from the normally innervated field. Hence the outcome of this test leads to the conclusion that in the skin of *Ameiurus* a concentrating neurohumor must be present.

Steps to extract this neurohumor are by no means simple, for the removal of a pale skin from the body of a fish necessitates the severance of all its nerves which quickly induces the dark phase. Hence though the experimenter may begin to remove a pale skin, by the time it is off it is well darkened. Nor does the decapitation of a pale fish and the subsequent and immediate dipping of the headless body into water at 60° C yield satisfactory results. The best procedure seems to be to remove the pale skin, allowing it to darken, and then on grinding it to mix with it an appropriate amount of horse serum the choline esterase of which will quickly destroy the momentarily produced acetylcholine. Thus it may be assumed with fair certainty that the only neurohumor left in the skin is the concentrating one. Extracts from such mixtures ought to yield the concentrating humor.

Such extracts, however, when injected into catfishes of intermediate tint were never found to produce a change in the general tone of the fish nor to induce the formation of a colored spot either pale or dark. When to the skins of five pale catfishes horse serum was added and they were then ground to a pulp with sand, mixed with Ringer's solution to a total volume of 30 cc. and dialyzed, the resulting dialyzate, 30 cc in volume, failed to change the color of catfishes into which it was injected. It has also been known for sometime that the defibrinated blood from pale catfishes when injected into other catfishes has no certain effect upon the tint of the recipients (Parker, 1934a, Abramowitz, 1936). From these tests it is concluded that if there is a concentrating neurohumor in the catfish, it is not present in the blood or in the skin in sufficient amount to make it easily identifiable. Naturally the substance to be expected under these circumstances is adrenaline. Steps were therefore taken to ascertain whether this material was a concentrating agent for the melanophores of *Ameiurus* and whether there was any evidence direct or indirect for its presence in the skins of pale fishes.

It has long been known that the injection of adrenaline will induce blanching in catfishes. This has been recorded by Bray (1918) and by Abramowitz (1936). As the following table shows catfishes will become pale from injections of 0.2 cc. of adrenaline up to one part in 10,000,000 Ringer's solution but not to further dilutions.

TABLE 1

EFFECTS OF DIFFERENT CONCENTRATIONS OF ADRENALINE IN RINGER'S SOLUTION ON THE MELANOPHORES OF NORMAL *Ameiurus*

Each fish used in the tests weighed about 50 grams and was injected subcutaneously with 0.2 cc of the given solution. The concentrations were made up from the Parke, Davis & Co preparation of adrenaline chloride 1:1000

Strength of Adrenaline	Melanophore Response
1:5,000	Quick, general blanching
1:50,000	General blanching in 10 min
1:100,000	General blanching in 15 min
1:1,000,000	General blanching in 25 min
1:5,000,000	Marked pale area at injection
1:10,000,000	Slight pale area at injection
1:20,000,000	No blanching at all
1:40,000,000	No blanching at all

The determinations given in Table 1 are fairly close to those recorded by Bray (1918) who states that the minimum effective dilution of adrenaline for blanching a catfish is 1:5,000,000 and that a further dilution of 1:10,000,000 caused merely a local contraction of the melanophores. As shown in Table 1 0.2 cc. of adrenaline 1 part in 1,000,000 was just sufficient to blanch a fish.

It does not necessarily follow, as argued by Chang, Hsieh and Lu (1939), that since adrenaline will blanch a fish, therefore, this substance is the natural agent in such a change of color. As recent studies in the physiology of the autonomic nervous system have shown, however, adrenaline is a very likely agent in such a response (Cannon and Lissák, 1939). To ascertain whether or not adrenaline is thus concerned, extracts of the skins of catfishes were tested by being applied to Straub preparations of the frog's heart. For aid in these tests I am under obligations to Mr. Ralph I. Smith.

Unless otherwise stated two pale catfish skins weighing together about 4 grams were removed and ground to a pulp with sand and 1 cc. of horse serum to remove acetylcholine. The pulp was then diluted with Ringer's solution without bicarbonate till the whole mixture had a volume of 30 cc. This was then dialyzed in a cellophane bag against 30 cc. of Ringer's solution. The resulting dialyzate was then treated in a variety of ways and used on frog-heart preparations.

When the simple dialyzate such as that just described was applied to a beating Straub frog-heart which had been in the apparatus several hours and washed numerous times with Ring-

er's solution, the beat of the heart changed as indicated by the increased length of the needle stroke, a feature which gradually disappeared as the heart was washed again with Ringer's solution. After thus washing the heart for some ten minutes a renewed application of the dialyzate gave a similar response which could be repeated as a rule many times. When pharmaceutical adrenaline 1:2,000,000 was dialyzed as described for the skin extract and the dialyzate (1 4,000,000) was applied to the heart much the same type of response ensued as did from the skin extract.

Adrenaline is known to be destroyed readily at the temperature of boiling water. A dilution of pharmaceutical adrenaline was prepared in bicarbonate-free Ringer's solution 1:4,000,000 and divided into two parts one of which was heated in a water-bath at 100° C. for an hour and the other kept at room temperature. On applying these two solutions alternately to the heart preparation no response was obtained from the boiled one, but a typical lengthening of the stroke was evoked by the unboiled one.

This test was then repeated with dialyzed skin extract. The dialyzate from two skins, 30 cc. in volume, was divided into two portions one of which was heated to 100° C. for an hour and the other kept at room temperature. The response to the application of the unboiled extract is shown in Fig. 12 at *A* and of the boiled extract at *B*. It is clear from these records that heating removed much of the active agent from the dialyzed extract, but that something still remained after the heating that continued to influence the heart. This residual substance is very likely inorganic, possibly calcium or potassium, but I am not in a position to state with any certainty what it really is. Judging from the test with heated adrenaline solution it is safe to conclude that it is not this substance. The considerable loss in response to the extract after it had been heated for an hour indicates that much of its activity must have been due to adrenaline.

A specific test for adrenaline is found in the change of response of the frog heart to this substance after treatment with ergotoxin. A frog's heart which had been beating regularly for about a day during which interval it was often washed with Ringer's solution was subjected to dialyzed extract of catfish

skin unboiled (Fig. 13, *A*) and boiled (Fig 13, *B*) to which it responded with characteristic strokes. On washing the heart with Ringer's solution the usual stroke returned. Ten minutes later during which the preparation had been washed five times, it was treated for two minutes with ergotoxin in Ringer's solution 1:5,000 which was then immediately replaced by the unboiled skin extract (Fig. 13, *C*). As can be seen by the record the beat strengthened somewhat in response to the ergotoxin and then again very slightly to the unboiled skin extract but by no means as it had done originally to this extract. On washing with Ringer's solution the response quickly subsided. A second application of ergotoxin and skin extract was followed by a similar response. As this type of reaction after ergotoxin is specific for adrenaline I conclude that this substance is present in the pale catfish skin.

How much adrenaline occurs in a normally pale catfish skin can only roughly be estimated from the records thus far obtained. A comparison of the effect on the frog's heart of unboiled, dialyzed extract from two catfish skins weighing in the wet together 4.121 grams with that of different concentrations of pharmaceutical adrenaline showed that the extract agreed approximately with an adrenaline concentration of 1 part in 5,000,000. Taking into account the dilutions employed in making the skin extract the concentrations of adrenaline in the original skin has been estimated to be one part in about 350,000. This determination was made by such steps that the final result may be compared with that for acetylcholine given in the earlier part of this paper. The concentration of acetylcholine in the ordinary dark skin of the catfish was there shown to be one part in about 13,000,000, which may be contrasted with one part in 350,000 for adrenaline. This comparison of the concentrations of the two neurohumors supports the commonly accepted view that though adrenaline is a very effective activator, it falls behind acetylcholine in this respect the efficiency of which in this instance is over 35 times that of adrenaline.

The conclusion to be drawn from the investigation recorded in this section is that the concentrating neurohumor in the catfish is adrenaline which is present in the skin to the extent of one part in about 350,000.

Adrenaline like acetylcholine is soluble in olive oil and when

it is injected thus dissolved into a dark catfish, a pale spot results which may last a day or two. No natural reactions of catfishes are known whereby pale spots are produced in the same way that dark spots and bands are. The nearest approach to such responses is seen in the electrical stimulation of chromatic nerves which thereupon induce the formation of temporary pale areas. These, however, are relatively slight and short-lived as compared with the dark areas produced by nerve severance. One obvious activity of adrenaline in the normal fish is seen in its slow creeping from pale areas into adjacent ones, an operation which is believed to be accomplished by the diffusion of the adrenaline through the lipid components of the tissues. Such conditions must involve a kind of competition between the two lipohumors in which the abundant on-coming adrenaline gradually overcomes the diminishing acetylcholine. Attention has already been called to such types of competition in the pale bands of *Mustelus* (Parker, 1937).

DISCUSSION

In addition to intermedin and acetylcholine as dispersing neurohumors and to adrenaline as the concentrating one in *Ameiurus* there may be other agents concerned with the color changes in this fish. Abramowitz (1936) pointed out that when an *Ameiurus* of intermediate tint is brought into a state of great excitement, as when repeated attempts have been made to catch it, the fish will often blanch and retain a pale tint for some time. After considerable experimentation with such animals Abramowitz was led to conclude that this blanching was comparable to what has been termed excitement pallor in other chromatic vertebrates and that it was due to an adrenaline-like material in the blood. The occurrence of this phenomenon in *Ameiurus* has also been noted by Osborn (1938b) who, however, passes it over without extended comment. I, too, have seen this type of blanching in *Ameiurus* and can confirm the evidence brought forward by Abramowitz that it is dependent upon a substance carried in the blood.

Thus *Ameiurus* appears to have two ways of blanching, a general one affecting the whole body of the fish and due to a blood-borne substance of the nature of adrenaline and a local one nervous in origin and attributed in this paper to adrenaline.

Is it possible that general blanching included that of denervated areas, and local well circumscribed, nervous blanching can both depend upon one such neurohumor as adrenaline or are we to assume that there must be at least two humors involved? That the second alternative is not necessary is shown by the way in which adrenaline itself acts. This substance when introduced into catfishes behaves very differently depending upon how it is dissolved. If it is injected in aqueous solution into a fish it immediately enters the blood, permeates the whole body of the animal and brings about a general blanching. Dissolved in oil and injected in discrete globules it acts in a strikingly local manner and produces pale spots in the skin that may persist a day or more. It is therefore entirely possible that the normal general blanching of a catfish may be due to adrenaline carried in the blood and that the more local type of response such as is associated with stimulated nerves may depend upon adrenalin in the tissue lipoids. Such an assumption implies that there is likely to be in *Ameiurus* two general sources for adrenalin. One of these we believe to be the system of terminals of the concentrating nerve-fibers which would deliver adrenaline to the lipoids of the cells. The other source can not be pointed out with certainty. It will be remembered, however, that hypophysectomized catfish are incapable of full blanching and it may be that the second source, that for the supply of adrenaline to the blood, may be in the pituitary gland. Hogben and Slome (1931, 1936) in their study of the color changes of *Rana fascicula* and especially of *Xenopus laevis* believed that they had good evidence in these amphibians for a blanching agent given out by the pars tuberalis of the amphibian pituitary gland. This agent was designated by them as W-substance. It would be interesting if this substance should prove to be the same as that concerned with the general blanching of the catfish which may also originate as already indicated in the pituitary complex of that animal.

The discussion in the preceding paragraphs leads to the conclusion that what appear to be the several blanching agents in the catfish may in reality be only one, adrenaline. Such a unification, however, is highly improbable for the darkening agents intermedin and acetylcholine. Although very little is known about the chemical nature of intermedin such as has been learned

sets this substance in strong contrast with acetylcholine. Intermedin is a relatively stable material in the blood of vertebrates. Acetylcholine on the other hand disappears very rapidly in this fluid. Even at this stage of our understanding of these neurohumors intermedin and acetylcholine appears to be definitely distinct.

So far as I am aware there are no color reactions in the catfishes that call for other darkening agents than the two just mentioned. However, catfishes do darken in a way which seems to have escaped attention and which deserves a word of comment. If a pale catfish is gently lifted out of the water of a white-walled, illuminated aquarium and as gently returned to it, a procedure that may be accomplished without a struggle on the part of the fish, the creature will darken perceptibly and remain so for some minutes. After this it will return rather quickly to its original pale tint. How is this temporary darkening accomplished? Is it through a momentary discharge of acetylcholine from the dispersing nerves, through a slight gush of intermedin, or by some other means?

If a blanched catfish with fully blanched caudal bands is handled as has just been described, it will darken over its whole body except for the caudal bands. These will remain pale, a tint to which the rest of the fish soon returns. Such a test indicates that the darkening of the fish on handling is due to nerve action; hence without doubt to acetylcholine. If the same test is carried out on hypophysectomized fishes the same results are obtained though not so strikingly. This test clearly demonstrates that the intermedin of the pituitary gland is not involved in the reaction and leads again to the conclusion that the handling response is nervous. So far as I know this is the only exceptional darkening reaction in *Ameiurus* and this may be set down as due to acetylcholine. There is therefore at present no reason to suppose that the normal blanching or darkening of this fish calls for more than the three neurohumors already discussed. In short the color changes in *Ameiurus* with its single system of melanophores is on what may be called a trihumoral basis.

In consequence of the striking darkness of newly cut caudal bands in pale catfishes I was led to assert that the dispersing nerves in this fish were strong in action as compared with the

pituitary chromatic secretion and I attributed to these nerves the chief rôle in darkening (Parker, 1934a). This assertion has been questioned by Veil (1937) and by Osborn (1938a; 1938b). Both these workers were much more successful in hypophysectomy than I had been and were thereby enabled to study active catfishes for weeks after they had been deprived of their pituitary glands. Under these conditions they believed that there was good evidence to show that the pituitary chromatic secretion, intermedin, was much more efficient as a darkening agent than the nerves. Veil asserted that it was as important for darkening in the catfish as it had been shown to be in selachians. As intermedin is the only known darkening agent in selachians this implies that it must be regarded as all important. Osborn (1938b) declared that intermedin is the most significant factor in producing the dark phase. Osborn's statement is based on the observation that in hypophysectomized catfishes the melanophores can disperse their pigment to only about 70 microns whereas in normal fishes the maximum limit is some 140 microns. As Osborn's hypophysectomized fishes had been kept till all intermedin had disappeared from their bodies, the dark response under these circumstances must have been purely nervous. As this was about one-third of the normal darkening Osborn concluded that some two-thirds of it must have been due to intermedin.

My own observations on normal and on hypophysectomized catfishes agree very closely with those of Osborn. In fact they are at variance with these in only one particular. I was able to get a dispersion of melanophore pigment in some seasoned, hypophysectomized fishes of as much as 100 microns instead of only 70, but this difference is of minor importance for 100 microns is well below the maximum of 145 microns that I have observed. I am therefore free to admit that my original estimation of the significance of the dispersing nerves in comparison with intermedin as a darkening agent was a mistaken one and that of the two agents intermedin is the more efficient. However, I do not even now rate it as high as Veil does nor do I regard the dispersing nerves as impotent as Osborn does. The dispersing nerves in my opinion show about half the capacity of intermedin. In estimating the value of the experimental evidence in this comparison it must be remembered that a hypophysectomized catfish

five or six days after the operation is far from a normal animal. The loss of the pituitary gland has a profound effect upon the general constitution of the fish. Although it may live several months from the time of the operation, it is never thereafter as active or as vigorous as a normal individual. In my opinion the reduced capacity of the dispersing nerves is in part due to this general debility and must be taken into account in any comparison between such nerves and the pituitary gland.

Another feature of interest in the present investigation is the relation of the two nerve neurohumors, acetylcholine and adrenaline, to their solvents. Each of these substances is soluble both in water and in oil, at least olive oil. Each acts very differently depending upon what it is dissolved in. In consequence of their solubility in water both pass freely into the blood and act generally on all the melanophores, adrenaline inducing them to concentrate their pigment and acetylcholine to disperse it. Both disappear in the blood, adrenaline rather quickly and acetylcholine no less than suddenly due to the destructive action of choline esterase. When dissolved in olive oil and injected as droplets under the skin of the catfish both agents remain active for hours or even days. Both appear to be protected from destruction by the oil and to make their way slowly from the oil droplets into the fluid about the melanophores which are then stimulated to respond. Thus these two neurohumors when dissolved in oil are stored and protected, and thereby enabled to act continuously over relatively long periods of time. The dark caudal bands and other dark patches which are produced in catfishes by nerve cutting and which I originally interpreted as the result of long continued nerve action are quite surely in part due to acetylcholine discharged by dispersing nerves and stored in the lipid components of the tissues. Such stores appear to remain effective for days and though temporarily overcome they may later reassert themselves. Thus a dark caudal band can be blanched by an injection of aqueous adrenaline and yet it will return as a dark band after the adrenaline has been destroyed in aqueous solution. Dissolved in oil neurohumors thus outlast those in watery solution, even when, as in the example just given, the neurohumor contained in the oil is much more sensitive to destructive agents than that in the surrounding lymph.

This interplay between the neurohumors and the lipoids of animal tissues points to a function which these fatty and oily constituents have in the animal body which has not been so fully appreciated as it deserves. These fatty materials undoubtedly serve as reservoirs for various agents the periods of activity of which are thus prolonged far beyond what they would have been had the agents been in purely aqueous solution. The fatty deposits in the bodies of animals and the lipid constituents of their cells must therefore be looked upon as constituting not only secure repositories for highly important substances but as an organic framework through which these substances may diffuse in ways quite impossible in the watery paths. Diffusion through such oily courses is known to be slow, in the vertebrate melanophore system a millimeter or so a day, but it is a protected and safe means of dispersion and enables substances that are highly important for the animal economy to spread throughout the organism. This method of dispersion of activating agents and other such materials is without doubt as significant for the egg and the embryo as it is for the adult.

6 SUMMARY

1. The three organs immediately concerned with the color changes of the catfish, *Ameiurus nebulosus*, are the pituitary gland, the dispersing nerves, and the concentrating nerves. At present there appears to be no reason to assume more than these three

2 The chromatic secretion of the pituitary gland, intermedin, is transported by the blood and is a highly efficient darkening agent for the catfish. After hypophysectomy catfishes can darken to only about half normal. They also fail to blanch fully

3. Dispersing nerve-fibers are present in catfishes as shown by the revival of blanched caudal bands when the chromatic nerves are recut either in normal or in hypophysectomized fishes

4. The neurohumor from dispersing nerve-fibers is acetylcholine as tested on leech-muscle preparations. Acetylcholine is present in dark skins to the extent of 0.078 gamma per gram of wet skin or 1 part in about 13,000,000 by weight.

5. Concentrating nerve-fibers are present in catfishes as

shown by the blanching of fishes on the electric stimulation of their chromatic nerves.

6 The neurohumor from concentrating nerve-fibers is adrenaline as tested on Straub frog-heart preparations with ergotoxin. Adrenalin is contained in the skins of pale catfishes to the extent of 1 part in about 350,000

7. The excitement pallor of *Ameiurus* is probably due to adrenaline. The darkening of this fish on handling is produced by dispersing nerves (acetylcholine).

8 As a darkening agent for *Ameiurus* the pituitary secretion is about twice as efficient as the dispersing nerve-fibers

9. In solution in the blood adrenaline disappears rapidly, acetylcholine still more rapidly. In solution in oil in a catfish both neurohumors persist a long time thus protected and continue to exert their influences on the melanophores for hours or even days. The fats, oils, and lipid components of tissues may serve as protective repositories for agents such as these neurohumors and thus greatly extend their time of operation. This protective feature of lipoids and the like probably plays a significant rôle in animals from their egg stages to the adult

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PLATE I

FIG 1 Tail of a pale normal catfish on which are three caudal bands the positions of which are marked by the three groups of darkened rays on the edge of the fin To the left in the figure is a band originally cut and fully blanched except for its outer end In the middle is a band cut at the same time as the left hand band and then recut after it had blanched To the right a new band cut at the same time that the middle band was recut

FIG. 2 Melanophores from the fully faded band in Fig 1, left They, like those for Figs 3, 4 and 5, were located in the space between the primary branches of the fin ray in the axis of the band

FIG 3 Melanophores from an uncut ray

FIG 4 Melanophores from the recut and thereby revived band, Fig 1, middle

FIG 5 Melanophores from the newly cut band, Fig 1, right

PLATE I

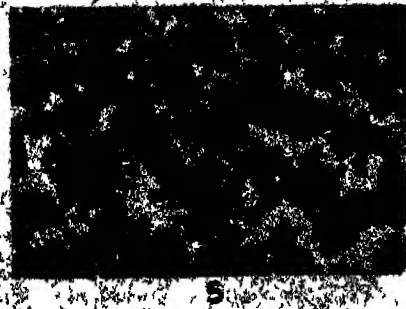
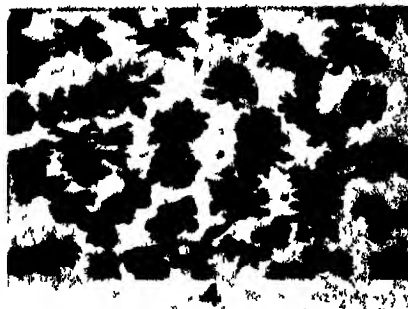
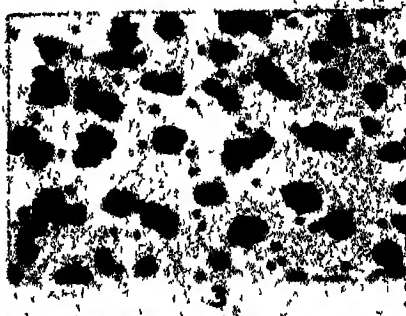
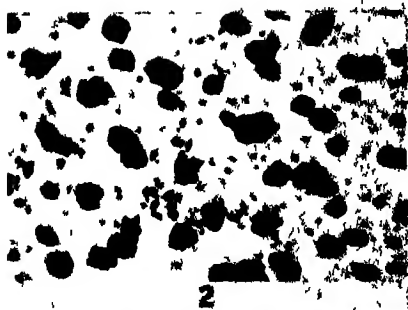
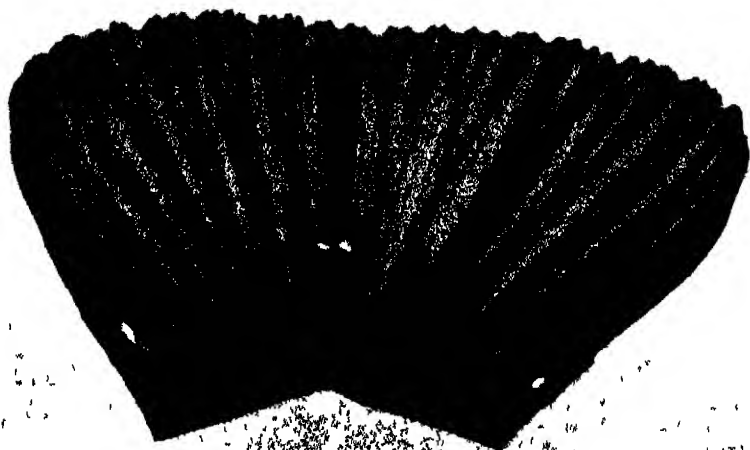


PLATE II

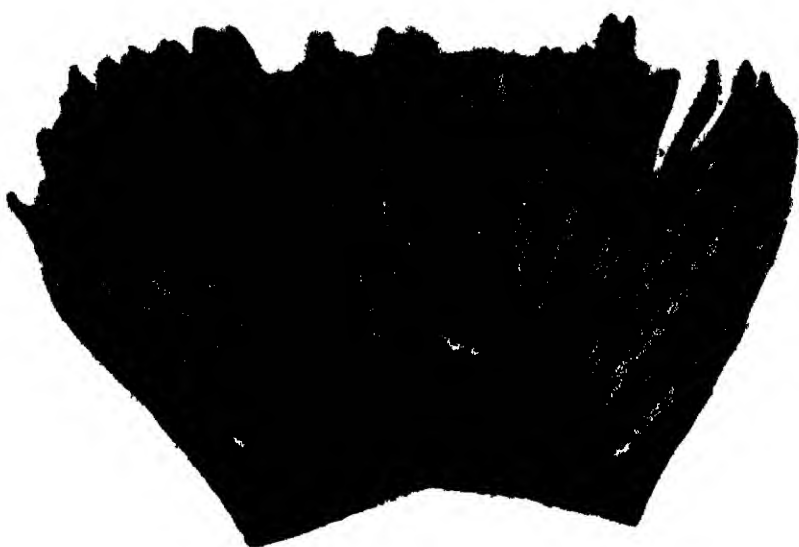
FIG. 6 Tail of a pale hypophysectomized catfish on which are two caudal bands. The melanophores in such fishes are incapable of full pigment dispersion or concentration. The band to the left in the figure was cut and then allowed to blanch somewhat. That to the right was cut, allowed to blanch as the left-hand band had been and was then recut.

FIG. 7 Melanophores from an uncut ray in the tail shown in Fig. 6. The color cells exhibit incomplete pigment concentration.

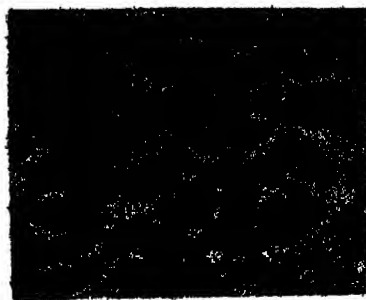
FIG. 8 Melanophores from the left-hand band in Fig. 6. The pigment in these color cells is in process of concentration.

FIG. 9 Melanophores from the right-hand band in Fig. 6. This band was initiated by a cut at the same time that the left-hand band was. After the original dispersion of pigment, concentration began and when it had reached the stage shown in Fig. 8, the band was recut whereupon its pigment began to redisperse.

PLATE II



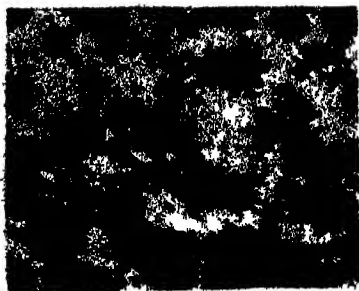
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7



8



9

PLATE III

FIG 10 Responses of a leech-muscle preparation to various solutions *A*, extract of dark catfish skin on uneserinized leech muscle, *B*, skin extract on eserinizied muscle, *C*, acetylcholine 10^{-8} on eserinizied muscle, *D*, acetylcholine 10^{-8} .

FIG 11 Continuation of the record of a leech muscle preparation shown in Fig 10, *E*, skin extract, *F*, acetylcholine 5×10^{-8} , *G*, skin extract, *H*, acetylcholine 2.5×10^{-8} .

FIG 12 Responses of a Straub frog-heart preparation to an unboiled extract of catfish skin (*A*), and to a boiled extract of the same (*B*). At each application of unboiled or boiled extract the extract was applied at the time indicated by the lettered vertical line. It was allowed to act on the heart about one minute and then washed out with Ringer's solution. This washing was repeated about five times between one application and the next. The interval between applications was some ten minutes. An interval of five minutes is indicated at the right hand end of the record.

FIG 13 Continuation of the record of the Straub frog heart shown in Fig 12, unboiled extract of catfish skin (*A*), boiled extract of the same (*B*), and applications of ergotoxine in Ringer's solution $1:50,000$ (2 minutes) followed for one minute by unboiled skin extract (*C*) and by washings with Ringer's solution. In this part of the record the heart responded to each application, including that of the Ringer's solution, by a momentary slight response as seen on the upper edge of the record.

PLATE III



B C D
10



E F G H
11



A B C A B
12



A B A C C
13

ENERGY DISTRIBUTION OF INCOMING COSMIC-RAY PARTICLES

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ABSTRACT

By sending recording cosmic-ray electrosopes essentially to the top of the atmosphere in a series of six different latitudes we have found the total amount of cosmic-ray energy that reaches the earth in each of these latitudes. Also, by taking the successive differences between these measured energies we have been able to obtain the distribution of energies among the electrons which continuously shoot through space. These energies are found to have a definitely banded structure such that at least 60 per cent of all cosmic-ray energy is carried by electrons of energy between 2 billion e-volts and 17 billion e-volts. Also this band has a strong maximum in the region between 6 billion and 10 billion electron volts. There are also indications of a band at 2 billion e-volts.

There should be bands in just these locations if the four most abundant elements except hydrogen that Bowen and Wise find in interstellar space have the capability, when they become free from frequent atomic collisions, of occasionally transforming their total rest-mass energy into cosmic rays. Such a transformation in the case of the carbon, nitrogen, and oxygen atoms would yield energies of 6 billion, 7 billion and 8 billion electron-volts, respectively. In the case of helium the corresponding energy would be 2 billion electron volts. These results need repetition and checking with increased accuracy.

1 REVIEW OF THE RESULT OF THE LATITUDE WORK OF THE SUMMER AND FALL OF 1936

EVER since 1931 Bowen and ourselves have been engaged in a program of trying to measure the total energy carried by cosmic rays as a function of latitude between the equator and the pole. Why? Because the earth is a great magnet having a magnetic field or magnetic "lines of force" which stretch out into space ten or twenty thousand miles and more, while the earth's atmosphere is in comparison a thin skin, say 100 miles thick, hugging closely the surface of the earth.

From the known strength of the earth's magnetic field we can compute what kind of blocking effects this field will exert on cosmic-ray electrons that are now known to be shooting into the earth continuously, and with essentially equal intensities, from all parts of the sky. The energy of each one of these shots is enormous, a

thousand times greater than that of the most energetic particle-rays found on earth. The density of the shooting, *i.e.*, the number of shots per given area per second is so small, however, that the total energy—the product of the energy of each shot by the number of shots—is only about the same as that of star light, so that there is no possibility of our ever being able to utilize cosmic rays for power purposes.

Some of these shots are probably photons of the same nature as X-rays or ordinary light, and we know that these are not influenced by the earth's magnetic field at all. These photon rays must then come in uniformly over the surface of the earth, thus constituting a part, at least, of the uniform background of cosmic rays that is the same at the equator as at the poles. This will not be true at all of the incoming cosmic-ray electrons (or charged particles of any kind) which we know constitute the larger part of the incoming cosmic rays. These will indeed get through the earth's magnetic field without difficulty *near the pole*, where their motion into the earth must be to a considerable extent *along* the earth's lines of magnetic force, but this will not be so as we go from the pole toward the magnetic equator. For at the equator or anywhere near it the motion of the incoming electrons will have to be in the main at right angles to these magnetic lines. Since the moving electron is the equivalent of an electric current, this motion across the earth's magnetic lines tends to turn the electrons around the direction of the lines and thus to force them out again in parabolic-like orbits into space. This, however, will not happen to those electrons that have a sufficiently high energy to break through this field resistance and thus get down to earth.

Further, the theorists have worked out that while it takes 17 billion (thousand million) electron-volts for either an electron or a proton (nucleus of the hydrogen atom) to break through the earth's field vertically near the equator, as for example at Madras, India, it takes but 6.7 billion e-volts to thus break through at San Antonio, Texas, 4.6 billion e-volts at Oklahoma City, 2.9 billion at Omaha, 1.2 billion at Bismark, and 1.4 billion at Saskatoon.

If, then, we can measure, as we actually succeeded in doing first in 1936, the total energy of all the cosmic-rays of all kinds that come in at Madras, and then repeat the same kind of measurements at San Antonio, the difference between these energies represents the total energy brought in by all the *charged particles*, electrons or protons, that can break through at San Antonio but

have not enough energy to break through at Madras—in other words, electrons of a mean energy of about 10 or 12 billion e-volts.

But how can we find the total energy brought in at a certain latitude, say that of Madras? By sending recording electroscopes up to the top of the atmosphere, plotting the curve of ionization—or rate of discharge of the electroscope—as a function of altitude clear up to the top, and measuring on the graph thus obtained the area underneath this curve. This gives directly the total number of ions produced in the electroscope, say per second, during the time of its travel to the top, but, since we already know that it requires 32 electron-volts to form each ion in air we can at once reduce the total incoming cosmic-ray energy at this latitude to electron-volts or any other convenient energy unit.

After many trials and some partial successes in airplane and manned-balloon flights between 1931 and 1936 we succeeded first in the summer and fall of the last mentioned year in getting so near the top of the atmosphere at both San Antonio and Madras—our best flights take our instruments 98.8 per cent of the way to the top—that nothing could be gained by going higher. These San Antonio and Madras curves are reproduced here in Figs. 1 and 2. The difference between these two curves, as shown in Fig. 3, told us three important things about these entering particles.

First—They are all very rapidly absorbable, as shown in curve A, Fig. 3, not one five-hundredth part of the energy brought in at the top by these particles getting down to sea level. That means that the incoming charged particles must be practically all electrons and not penetrating particles of any kind, such as protons or mesotrons. For it is to be remembered that charged particles in the range of energies above a billion e-volts experience practically the same deflection in a magnetic field whether they are electrons, protons, or mesotrons, but that when it comes to *penetrating the atmosphere* the coefficient of absorption varies inversely as the square of the mass of the moving particle so that electrons are enormously more absorbable or less penetrating than protons or mesotrons, the former of which have a mass 2000 times that of the electron, the latter 200 times.

Second.—These experiments bring forward the unambiguous proof that since the particles that get through the earth's magnetic field at latitudes between those of San Antonio and Madras have energies up to 17 billion e-volts, up to those huge limits electrons do not change their properties and become more penetrating than

they are known to be at lower energies. That they did just this was the hypothesis that many of us originally thought we had to make to explain the existence, so sharply demonstrated by cloud chamber experiments made at sea-level, of the tracks of very pene-

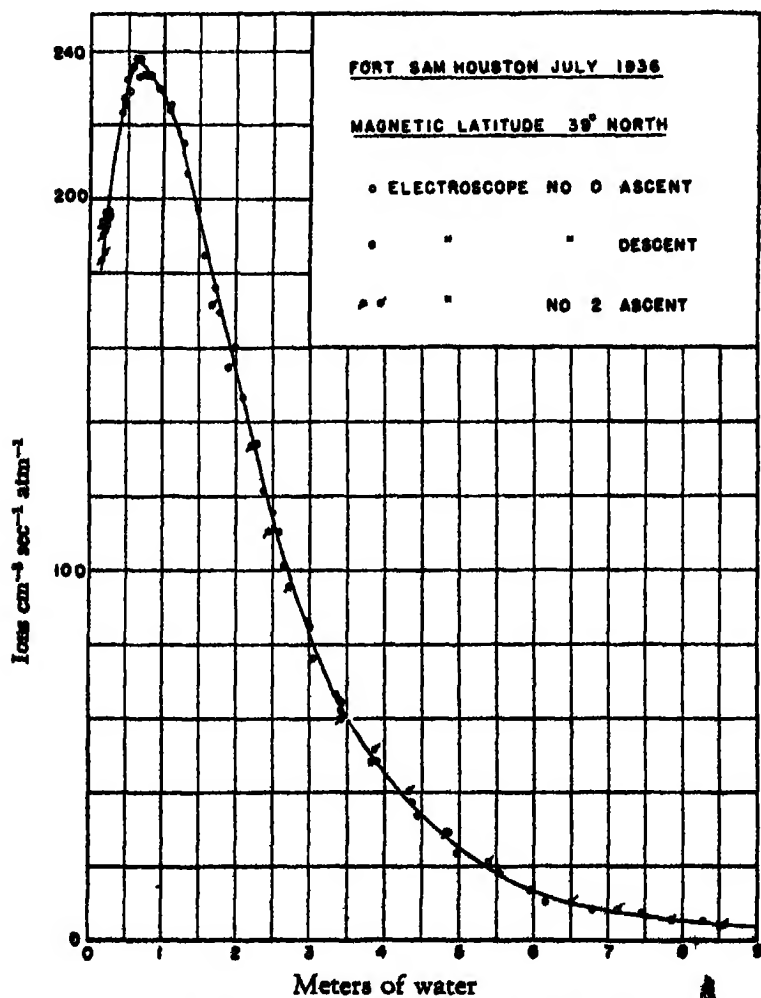


FIG 1 Altitude-ionization curve for both flights, reduced to ions per cc per sec at atmospheric pressure. The pressures are in meters of water below the top of the atmosphere (10 m = 1 atm)

trating particles which every one at first thought were electron tracks. The present experiments demonstrated, for the first time, the incorrectness of this hypothesis of a change in the penetrating power of electrons at very high energies. It showed that the pene-

trating particles (now distinguished from electrons by the term mesotrons), found so abundantly in the lower part of the atmosphere, must be *secondaries* formed directly or indirectly by the impact of high energy electrons on the nuclei of atoms contained in the atmosphere.

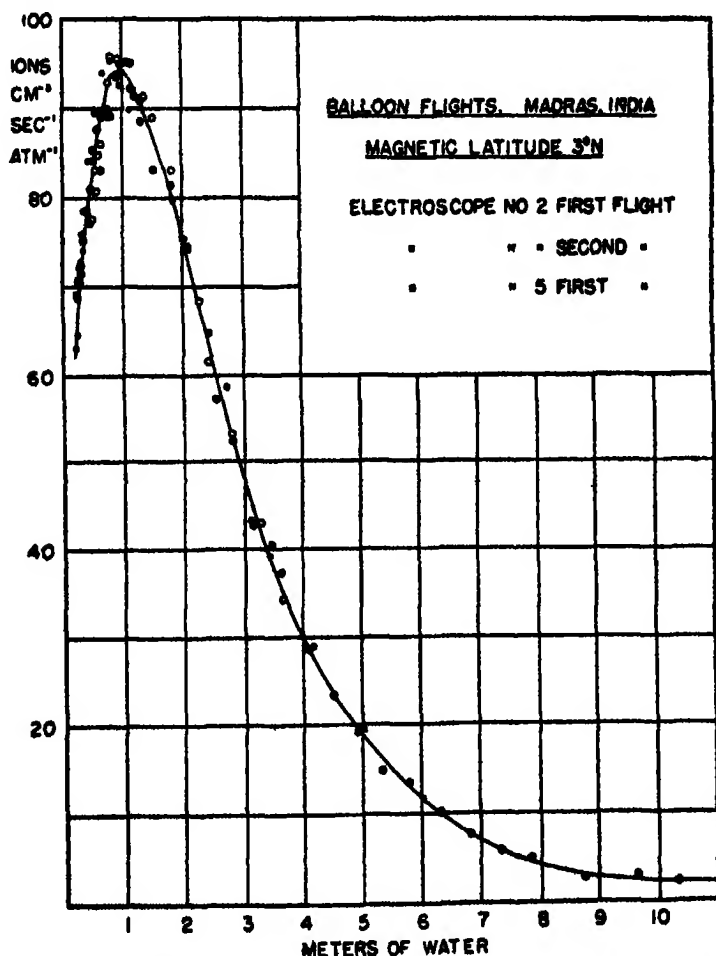


FIG. 2. Ionization as a function of depth, in equivalent meters of water, below the top of the atmosphere at Madras, India, mag lat 3° N

Thrd.—The total energy brought in by the band of electrons of energies from about 6.7 to 17 billion e-volts is about the same as the sum of the energies brought in by all photons plus that brought in by all electrons of higher energies than those contained

in this band, that is of higher energies than 17 billion e-volts. This assertion finds its justification in the fact that the area underneath curve A, Fig. 3 is nearly the same as that underneath curve B, Fig. 3

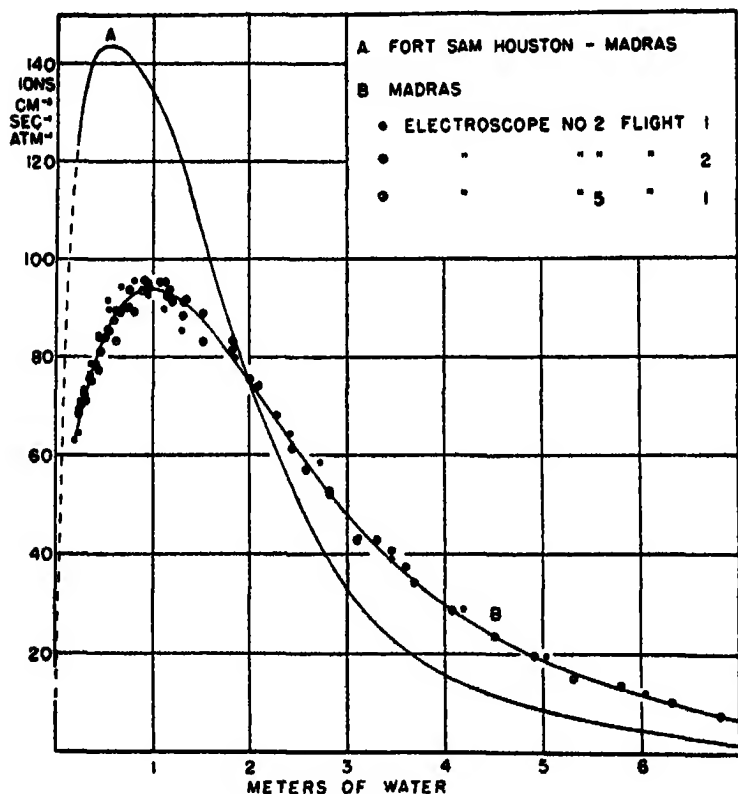


FIG. 3 Curve A shows the ionization at all depths down to 7 m of water due to the electrons entering the atmosphere of energies between 6.7 billion e-volts and 17 billion e-volts. This curve is the difference between the curves of Fig. 1 and Fig. 2. Curve B is the same as that shown in Fig. 2 but drawn to the same scale as curve A. The areas (or incoming energies) under the two curves are nearly the same.

2 RESULTS OF OUR LATITUDE MEASUREMENT IN THE SUMMER OF 1937

In the summer of 1937 we made at Omaha, Nebraska, and at Saskatoon, Canada, the same sort of measurements as we had made the preceding summer and fall at San Antonio, Texas and Madras, India, and then put all the results of the work of the two summers together in Figs. 4 and 5.

Figure 4 gives all four of the latitude curves, drawn to the same scale, from close to the top of the atmosphere down to a depth of 7 meters of water beneath the top, and the rectangular blocks 1, 2, 3, 4 in Fig. 5 give the succession of areas (incoming electron

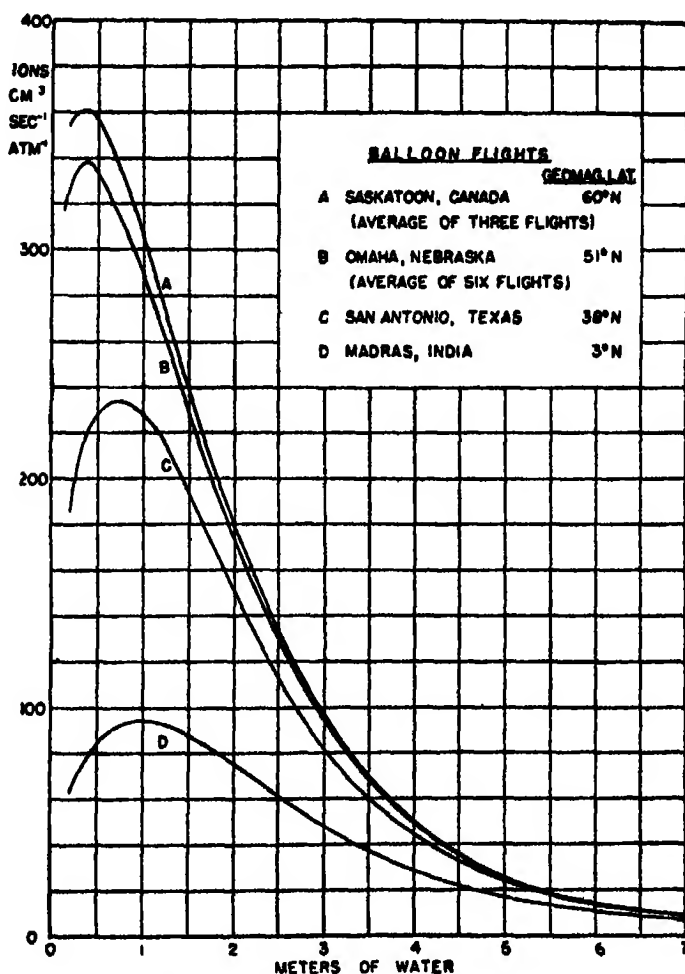


FIG 4 Results of balloon flights at four different latitudes

energies) between curves A and B, B and C, C and D, and underneath D, respectively.

The areas 1, 2 and 3 of Fig. 5 of course represent ionizations due to *incoming electrons* of energies between the limits shown on the V axis in the figure, but area 4, on the other hand, represents the total measured ionizing effect of all the rays that enter the

equatorial belt, no matter what their nature may be. In so far as these rays are photons, we have no knowledge as to what energies are associated with them. We merely include them with the *electron rays* of energy above 17 billion e-volts (the part of area 4 underneath the dotted line) because they are found with them in the equatorial belt.

Having thus built up from the directly observed ionizations the rectangular areas 1, 2 and 3, we proceed without in any way chang-

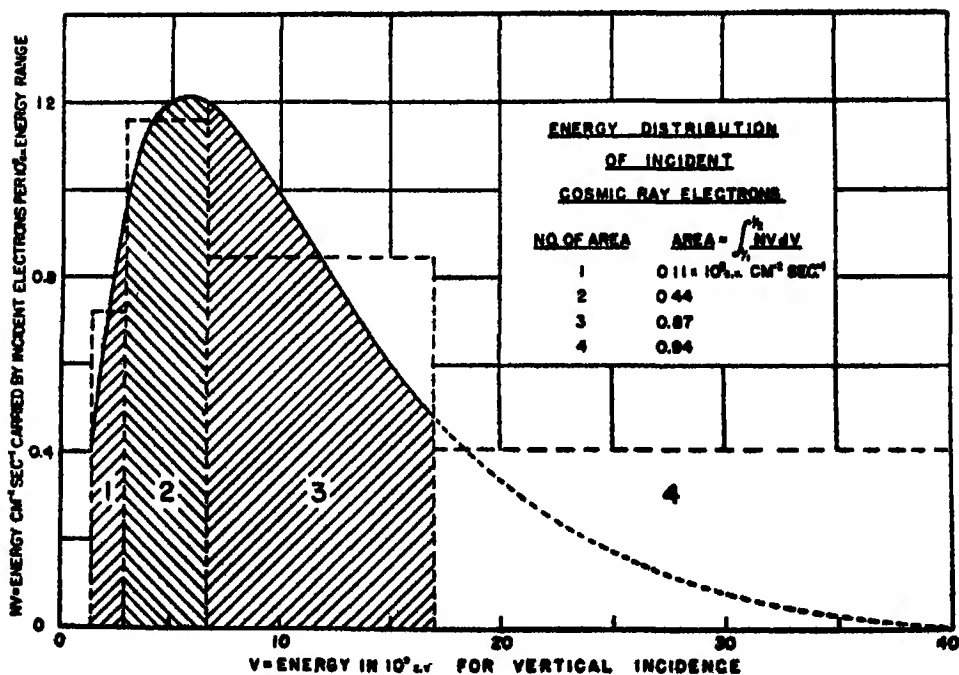


FIG. 5 The areas under A, B, and C of Fig. 5 are plotted between the corresponding values of individual electron energy, V , for vertical incidence. The ordinate thus gives the energy carried to the earth by electrons having energies lying between V and $V + dV$.

ing these areas to readjust their shapes at the tops in the manner that is dictated by the single hypothesis that there is some *continuous* distribution of energies of the incoming electrons as their energies vary from 1.4 billion to 17 billion e-volts. This imposed condition leads to the final shapes of the areas 1, 2 and 3 as shown in Fig. 5, and it is notable how little flexibility in the shape of this curve up to the energy value 17 billion e-volts is left when the one condition of "no sudden breaks in the curve" is imposed. This

curve then undoubtedly represents a first approximation to the actual distribution of incoming *electron* energies up to 17 billion e-volts.

The extrapolation of this electron-energy curve beyond 17 billion e-volts (see dotted lines passing through area 4) has a reasonable chance of having a rough correspondence to reality, but obviously no certainty. As we have extrapolated it in Fig 5, it takes care of about half of the integrated ionization which the electroscope actually experiences in the equatorial belt. The remainder of the observed ionization at Madras we have here represented by the remainder of the rectangle 4, which has been quite arbitrarily made to extend about as far to the right as the electron-energy curve extends before getting close to the *V* axis. This is more or less natural because of the rough interconvertibility of electrons and photons through nuclear impacts. Nevertheless, it is to be emphasized that rectangle 4 is inserted merely to have on the chart the representation of *the total ionization due to all the cosmic rays, no matter what their nature may be, that enter the equatorial belt*, and not to assert that the hypothetical photon part of area 4 lies between the energy limits between which it is found in the figure. Where these supposed photons lie in the energy spectrum is of no particular importance for the present considerations.

There are certain definite conclusions that can be drawn from Fig. 5, as follows

1. The first is that *the cosmic rays as they enter the atmosphere unquestionably have a definite banded structure*. This has been pointed out repeatedly before, but never until now as the result of direct, indubitable energy measurements.

2. The second conclusion is that *the maximum of the cosmic-ray energy brought into the atmosphere by electrons in the northern hemisphere, where our measurements are made, lies at about 6 or 7 billion e-volts*, and that the energy distribution curve of the incoming electrons falls off rapidly on both sides of this point.

3. The total cosmic-ray energy brought in by electrons of energy above 17 billion e-volts plus all that brought in by photons of all energies is about the same as the energy brought in by electrons alone of energies between 6.7 and 17 billion e-volts, and this energy is fully twice that brought in by all entering electrons of energies less than 6 billion e-volts. In other words, *by far the greater part of the whole cosmic-ray energy comes in as a relatively sharply limited band*.

4. The smallness of the amount of energy brought in by photons, namely, only a fraction (probably not more than a half) of that represented by the area of 4, means definitely that *the entering electrons have not at all got into equilibrium with their secondaries before entering the atmosphere*, for in equilibrium Carlson and Oppenheimer have shown that "at any energy and thickness $t > 1$ ($t = 0.4$ m of water) there are always more γ -rays than electrons",¹ while in Fig 5 the area assigned to photons is scarcely more than a sixth that assigned to electrons. This last conclusion does not rest solely upon the accuracy of the Carlson-Oppenheimer computations, for, as shown by the turnover points of the curves in Fig 2, entering electrons even of a mean energy of 10 billion e-volts do actually get into equilibrium with their secondaries before they have penetrated even a tenth of the way through the atmosphere, so that after it has become established that the entering particle rays are electrons² the smallness in the number of accompanying photons shows, from nothing more than a qualitative point of view, that *these rays cannot ever have come through an appreciable amount of matter in comparison with an atmosphere before entering the solar system.*

5 The conclusion drawn in 4 above means that *the cosmic rays cannot have originated within the stars or in any portions of the universe in which matter is present in appreciable abundance*. This conclusion also appears to be indicated by the mere fact that the curve of Fig 5 goes through a definite maximum of about 6 or 7 billion e-volts, unless the improbable assumption be made that the observed maximum is wholly due to the action on the incoming rays of the sun's magnetic field. For when an electron of given energy, say 10 billion e-volts, passes through matter, since the main mechanism of its absorption is first the formation of an impulse-radiation photon, then of an electron pair, then of two impulse-radiation photons, then of four electron pairs, etc., it follows that the energy corresponding to each value of V (Fig. 5) should remain a constant for all values of V lower than the original value of the incident electron energy. This permits of no such maximum as appears in Fig. 5, so that if this maximum is inherent in the character of the rays as they enter the solar system, then no such process of degradation of energy through the "bremsstrahlung"-pair-formation process can have taken place. Further, if the original electrons had

¹ Carlson and Oppenheimer, *Phys. Rev.*, 51, 225 (1937)

² This was proved more fully in *Phys. Rev.*, 53, 217 (1938).

energies of many different values, some low, some intermediate, and some high, then the energy-distribution curve resulting from the passage of these electrons through a small amount of matter would be *one rising continuously with decreasing values of V* . The evidence drawn from the existence of this maximum appears, then, to be, in agreement with that drawn from the smallness of the photon component, that the incoming cosmic-ray electrons have not passed through an appreciable amount of matter on their way from their point of origin to the earth

We have given attention to the question raised above as to whether the influence of the sun's magnetic field could have been responsible for the appearance of the strong maximum at about 6 or 7 billion e-volts, as shown in Fig 5, and have thought this unlikely from the consideration of the fact that if the blocking effect of the sun's field is not sufficient completely to prevent say 2 billion volt electrons from getting through to the earth (and we certainly find some of them getting through between 1.4 and 2.9 billion e-volts as Fig 5 shows), then 5 and 6 billion volt electrons would probably get through to the earth, *i.e.*, they could not be blocked off in appreciable amount by any sun's magnetic field which would let through even a small number of say 2 billion e-volts. But 5 and 6 billion volt electrons are both beyond the point of inflection that begins near the top of Fig 5, and which then indicates a maximum, or *a banded structure of the incoming rays before they reach the sun's magnetic field at all*. This conclusion is at least not contradicted by the more rigorous computations of Dr Epstein,³ who made a careful quantitative study of the effect of the sun's field on electrons coming into our solar system, and found it too small to cut off electrons of higher energy than about 2 billion e-volts.

3. SPECULATIONS AS TO THE MODE OF ORIGIN OF THE COSMIC RAYS

It was suggested more than ten years ago, in view of the two most surprising properties of the cosmic rays, namely (1) their enormous energies, and (2) their uniform distribution over the celestial dome, that it was difficult to conceive of any mode of origin that was at all capable of yielding such energies and such directions save the one that had been so successfully invoked since

³ Epstein, *Phys. Rev.*, **53**, 862 (1938). See also Janossy, *Zett. f. Phys.*, **104**, 430 (1937).

about 1905 in accounting for the otherwise inexplicably large evolution of energy by the sun and stars. Thus for at least thirty-five years astronomers have recognized the fact that there is no way of accounting for the known evolution of heat by the sun save through the assumption that it is actually somehow radiating away its mass. Einstein in the development in 1905 of his equation $mc^2 = E$ gave quantitative expression to this hypothesis. The successes of this equation have not only now made it a universally accepted basis for astronomical calculations but the equation has not yet failed in any of the nuclear transformation phenomena, like those involved in radioactive change, in which it has been found possible to subject it to careful test. *The transformation of rest-mass into other forms of energy is now the corner stone of practically all work in the field of nuclear physics*

In 1928 Millikan and Cameron⁴ used the Einstein equation, as applied to the partial annihilation of mass in the atom-building process, for the interpretation of cosmic-ray energies. This was before those energies had become definitely known through direct measurement, and when the indirectly estimated values of these energies seemed to make the loss of mass through the building up of the heavier elements out of the lighter elements yield adequate energies from the known values of so-called "packing fractions". This possibility has now been eliminated for two reasons. First, the largest energy obtainable from packing fractions in the case of any element of appreciable abundance, namely, iron was 0.48 g. per g.-atom, or about half a billion electron-volts, but beginning in 1931 Anderson and Millikan measured *directly* the energies of cosmic rays and found them running up above 6 billion e-volts, much higher than could be accounted for by any possible packing fractions. This alone was definite and final. Second, precisely these same atom-building processes involving these same "packing fractions" seem now to be successfully accounting for the sun's heat. If this be so we must now assume that those very processes, of *partial* annihilation through atom building, which we assumed going on *outside* the stars to account for cosmic rays are instead going on *inside* the sun to account for its evolution of heat. But since the cosmic rays do not come from the sun or the stars we are now debarred also from using these same processes to account for them. In other words, there is abundant reason for retaining the principle, beautifully illustrated by Bowen's interpretation of the nebularium

⁴ *Proc Nat Acad Sci.*, 14, 637 (1928), and *Phys Rev*, 32, 533 (1928)

lines, that the conditions existing in interstellar space make possible at least some sorts of energy transformations that are forbidden within the stars

Both of the foregoing difficulties disappear if we simply reverse our former suggestions and assume that the sun's heat is due to atom building going on in its interior while cosmic rays are due to the complete annihilation process going on in interstellar space, or better to the *complete*, instead of the partial, transformation of the rest-mass of the atoms into cosmic-ray energy. The question which we then face is, how well does this last suggestion work quantitatively?

We have called attention to two definite quantitative facts, namely, (1) Fig 5 shows a maximum of incoming electronic energy at about 6 or 7 billion e-volts, and (2) certainly not less than 60 per cent of the total incoming cosmic-ray energy is carried by electrons, for this is the ratio of the sum of the areas of the blocks 1, 2, 3 to that of 1, 2, 3, 4 (see table in Fig 5). But to carry through the desired computation we must know what are the most common elements in interstellar space where the cosmic rays seem to originate. Bowen and Wise⁵ have just been making determinations of such relative abundance of the atoms in the spaces between the stars by spectroscopic observations on the ring nebulae. Since these are a light year or more from the exciting star we can scarcely fail to take their figures as applying to interstellar space. Here are their results. They find that hydrogen atoms and helium atoms stand first and second in abundance, hydrogen being ten times as abundant as helium. They find, further, four other atoms all having about the same abundance, which, however, in each case is only about one-tenth the abundance of helium. No other atoms have more than a tenth of the abundance of any of these four. These four most abundant ones are *carbon, nitrogen, oxygen, silicon*.

Now the rest-mass energy of the hydrogen atom comes out by Einstein's equation just a little less than one billion e-volts. If, then, this rest-mass is capable of being transformed completely into cosmic rays, since the momentum principle must in any case be satisfied, the only way these two conditions can be met is through the appearance of a pair of electrons (or less frequently in view of the second of the above mentioned facts, of photons) starting out from the point of annihilation of the rest-mass of H_1 in opposite directions each of energy of 500,000 e-volts. We have already seen that an elec-

⁵ *Bull. Lick Observatory*, 19, 1 (1939)

tron endowed with this energy could not get through the sun's magnetic field and reach the earth so there is no use expecting to find it here. Again, if the rest-mass of a hydrogen molecule, instead of a hydrogen atom underwent such a complete transformation, it would produce two oppositely directed one billion e-volt electrons. This radiation also could not be detected here for the reason just given even if we make the very unlikely assumption that hydrogen molecules had sufficient abundance to produce such rays in detectable amounts; but the case of a molecule is here considered since, if the possibility of the complete transformation in interstellar space of rest mass into cosmic ray energy exists at all, there is no more reason for excluding molecules or even larger aggregates from the operation of the principle than in the case of the Brownian movements.

By the foregoing procedure we find that helium atoms of atomic weight 4 would produce 2 billion volt cosmic rays. If such rays got through the magnetic field of the sun, as, in all probability, according to Epstein, some of them might do, they could get through the earth's magnetic field everywhere north of about the latitude of Bismarck, North Dakota (N mag 56) but nowhere south of there. Altogether similarly carbon rays of energy 6 billion e-volts would begin to get through the earth's field at a latitude a little north of San Antonio, for at San Antonio the voltage required to get electrons down to earth vertically is 6.7 billion e-volts. This is also very close to the latitude at which nitrogen rays of energy 7 billion e-volts would first be found as the observer moves northward. A few degrees still farther south the southern edge of the oxygen cosmic ray plateau corresponding to an energy of 8 billion electron volts would be found. Another southern edge of a cosmic ray plateau corresponding to the entrance of silicon rays would be expected to appear where the energy required to get through the earth's magnetic field would occur at a magnetic latitude at which the energy required to break through the earth's magnetic field vertically is 14 billion e-volts and from that point down to the magnetic equator there should be no further change in the intensity of the incoming cosmic rays. The edges of this succession of overlapping cosmic-ray plateaus correspond to these energies 6, 7, 8 and 14 billion e-volts would not be expected to be very sharp since the energies required to get in vertically have been used in this discussion while the electroscope technique which we have been using takes in rays from all directions.

4. THE BEGINNING OF AN ATTEMPT TO SUBJECT THE FOREGOING PREDICTIONS TO EXPERIMENTAL TEST

In the first publication⁶ on the energy-distribution of entering cosmic-ray electrons it was pointed out that further checks on the suggested possible origin of the rays might be obtained by getting as accurate measurements as possible of the total cosmic-ray energy entering at a larger series of more closely spaced latitudes, thus breaking up the three entering electron energy blocks shown in Fig. 5 into smaller blocks so as to get higher resolution into that energy distribution curve. In the last week in June and the first two weeks in July, 1938, we carried out a part of this program by obtaining four successful flights, some of them essentially to the top of the atmosphere, in Bismarck, North Dakota (mag lat 56° N) and three such flights in Oklahoma City (mag lat 45° N). The mean values at each altitude obtained from the four flights at Bismarck and the three flights at Oklahoma City are plotted in Fig. 6.

When the mean readings at Bismarck are compared with those obtained at Saskatoon between August 13 and 17, 1937, it is found that in the upper fifth of the atmosphere, i. e., above 2 m. of water, the readings at Bismarck are consistently about 4 or 5 per cent *higher* than those at Saskatoon. For the lower four-fifths of the atmosphere the readings at Bismarck are identical with those at Saskatoon within the limits of uncertainty of the readings. To understand how the reading near the top of the atmosphere at Bismarck can be *higher* than those at Saskatoon we may postulate differences at different times in the strengths of the earth's or the sun's magnetic fields as causes. In any case, since it is the Bismarck readings that are the higher, the conclusion seems fairly well justified that there are no electrons entering the atmosphere of energies lower than those that can just get through at the latitude of Bismarck, where according to LeMaitre and Vallarta the energy required to get in vertically is 2 billion e-volts.

We shall then take the plateau of uniform cosmic-ray intensities at all altitudes as setting in very close to the latitude of Bismarck.⁷ The depth-ionization curves at Saskatoon, Omaha, Oklahoma City, and San Antonio were all taken at nearly the same season, namely,

⁶ Bowen, Millikan and Neher, *Phys. Rev.*, **53**, 855 (1938).

⁷ Indeed, in 1932 we called attention to this plateau when in airplane flights up to 22,000 feet we got no differences in readings taken at a given height at Spokane (mag lat 54°) and at Cormorant Lake (mag lat 63°). See *Phys. Rev.*, **43**, 661-9 (1933) and *Phys. Rev.*, **44**, 246 (1933).

late July, August, and very early September, but those at San Antonio were taken in '36, those at Omaha and Saskatoon in '37, and those at Bismarck and Oklahoma City in '38.

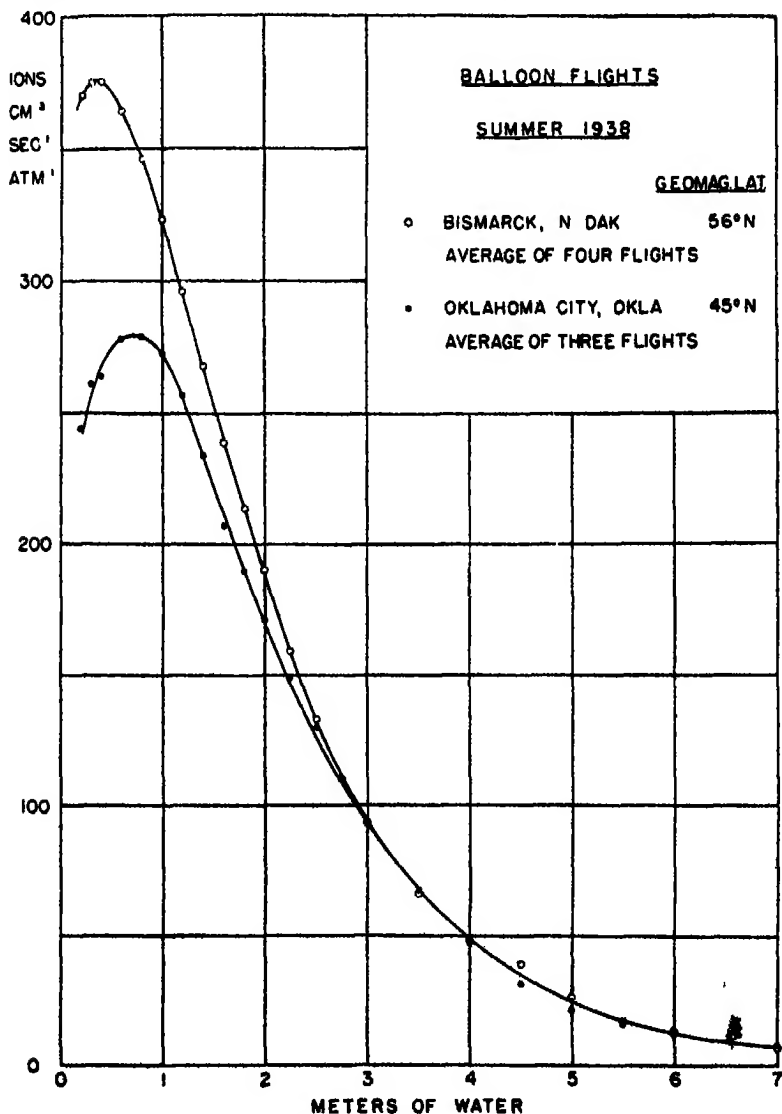


FIG 6

Since Saskatoon is north of the edge of the plateau, or polar cap, of uniform intensities for 2 billion e-volt rays, and since according to the foregoing theory there can be no entering energy of less than 2 billion e-volts, also since in our airplane flights of 1932 we

got no change at all between Spokane (mag. lat. 54°) and Cormorant Lake (mag. lat. 63°) we shall take as the best value of cosmic ray energies on the plateau beginning at Bismarck, to compare with the energies at Omaha, Oklahoma City and San Antonio, the mean

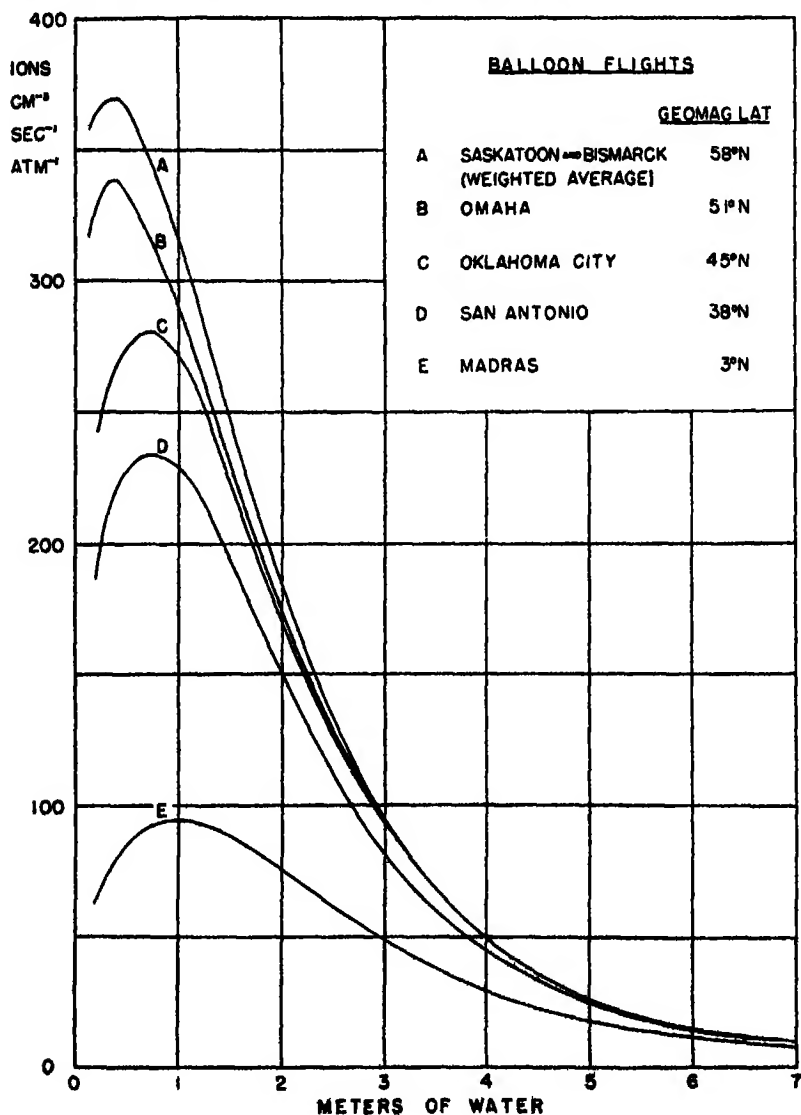


Fig 7

of the readings at Bismarck and Saskatoon. However, the essential results of these observations are the same whether we use this mean or simply the mean of the Bismarck readings. We thus obtain the five curves A, B, C, D, E shown in Fig. 7 corresponding respectively

to mag. lats 56° , 51° , 45° , 38° , 3° and incoming vertical energies 2, 2.9, 4.5, 6.7 and 17.1 billion e-volts.

If, then, we take the successive differences in the areas underneath these five curves and plot them as four rectangles between the appropriate voltage limits in precisely the way in which we built up the three energy bands or blocks of Fig. 5 we now obtain the four bands or blocks of Fig. 8. This seems to show as before a maximum in the region of 6 or 7 billion e-volts and a new one in

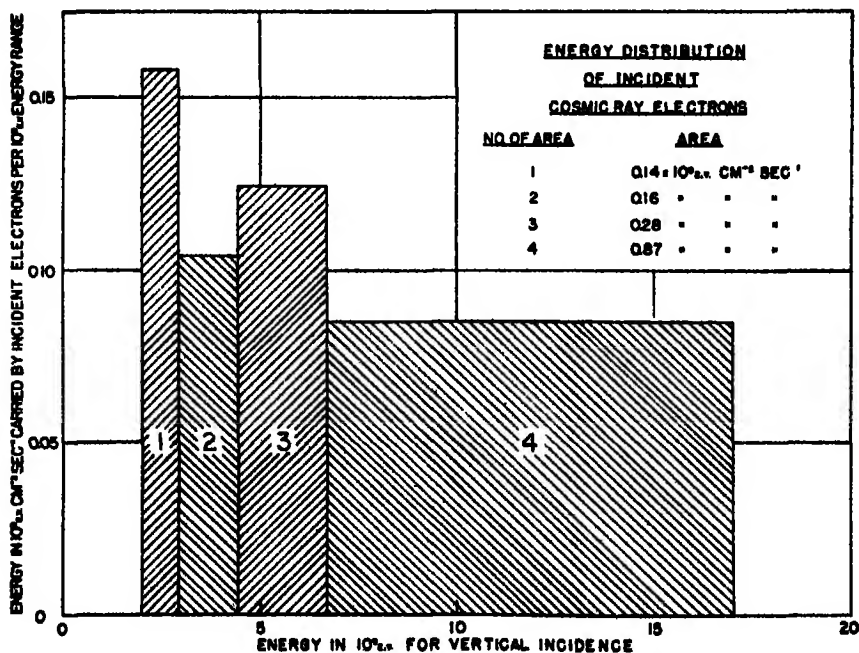


FIG 8

the region of 2 billion e-volts. It is entirely possible, however, that the apparent 2 billion volt maximum is due to changes in the actual incoming cosmic-ray energy that occurred between the summer of 1937 and that of 1938.

A second possible source of uncertainty is found in the fact that the readings at Bismarck taken on different days showed differences which were larger than our observational uncertainties as shown in Fig. 9. Such fluctuations we have never observed at other stations. Since Bismarck is on the edge of the 2 billion e-volt plateau it is at a point that would be sensitive to changes in the earth's

magnetic field due to magnetic storms, as would also all other latitudes farther north. At any rate we can only invoke changes

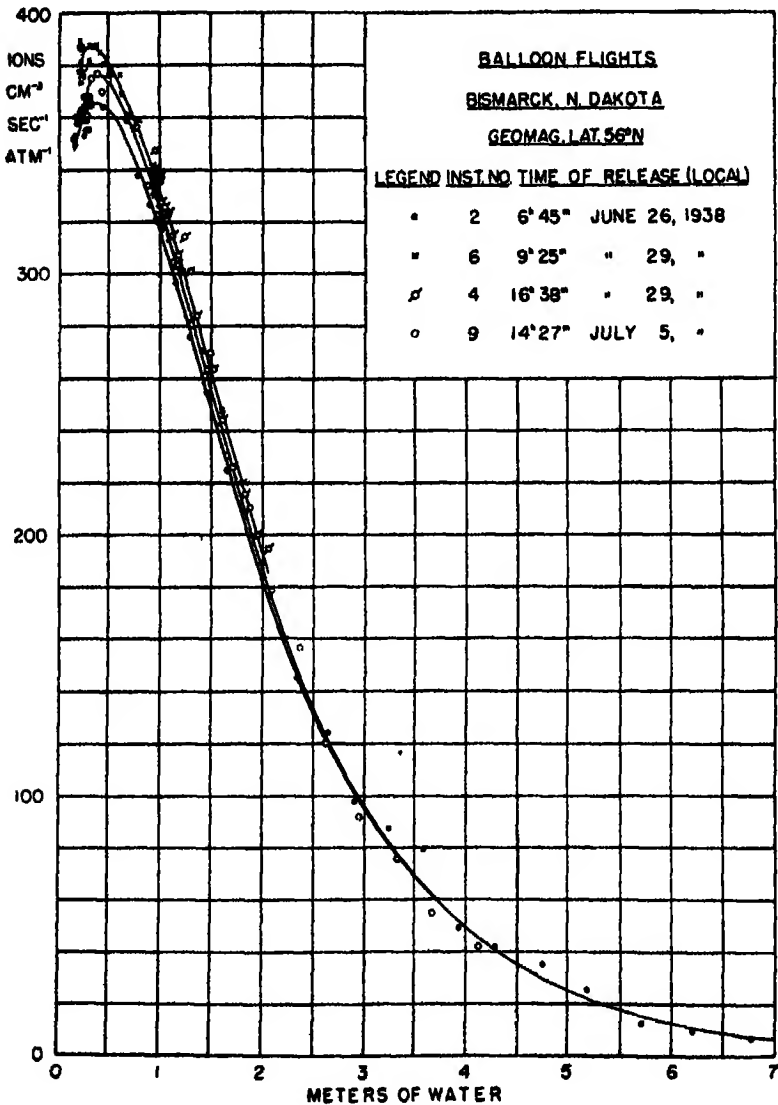


Fig 9

either in the earth's or the sun's magnetic fields to cause these short time fluctuations which are here observed for the first time in experiments of this kind. The foregoing experiments therefore

need repetition to make sure that under all circumstances, especially when the observations at Bismarck, Omaha, and Oklahoma City are made in so rapid succession as to minimize the possibility of being misled by such long time changes in cosmic-ray intensities. We plan such repetition in the near future.

In conclusion we wish to express our gratitude to the Carnegie Corporation of New York, which has by its financial aid, administered through the Carnegie Institution of Washington, made these researches possible.

INTRODUCTION TO THE IONOSPHERE

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(Read April 18, 1940)

THERE used to be a story about a little girl who being told to define the equator, defined it as "an imaginary lion running around the earth." Borrowing some of her language I will define the ionosphere as a cloud of imaginary ions hovering over the earth. The analogy of course is not to be pressed too closely. The little girl's imaginary lion is doubtless imaginary in the popular sense of the word, for quantities of people have been to the equatorial regions without ever finding a lion, or for that matter a line, running around the earth. The physicist however has posted his ions at an altitude so great, that nobody ever has gone there to seek them. Everyone has heard of the stratosphere and of the balloons that have been there, and many may still vaguely conceive of the stratosphere as the ultimate reach of the atmosphere and the jumping-off place for inter-stellar space. But even the stratosphere is low compared with the ionosphere, and to this last the highest-flying balloon has never even approached.

It is, indeed, between one hundred and five hundred kilometres of altitude that the physicist sprinkles the most of his imaged ions. In such an inaccessible region he has great liberty to sprinkle them so as to fulfil his objectives—liberty great, but not complete. His objectives are, to explain the variations in the magnetic field of the earth, and to explain the reflections of radio waves. Let us however examine first the extent of his freedom before we consider the use which he makes of it.

Ions, as we know them in the laboratory, are of various kinds, beginning with free electrons as the least massive variety, they also include atoms and molecules with positive and negative charges, and charged clusters of atoms and molecules. It will transpire that for the ionosphere it is both necessary and sufficient to postulate free electrons, though we are by no means excluded from believing that there are also many ions of much more massive types. Free electrons are produced from ordinary neutral mole-

cules by ionizing agents of various sorts, and one of these is ultra-violet light—light of the *far* ultra-violet, not the near. Well, even at hundreds of kilometres over the ground there must be air and there must be ultra-violet light—an agent for setting electrons free, and material for it to work on. Is there enough of both?

As to the quantity of air, it must be small and yet it seems to be sufficient. I recall an era when it was stated quite dogmatically that the various gases of the atmosphere decrease exponentially in density with rise of height, the exponents varying from gas to gas in such a way that at the very top of the atmosphere there must be nothing left but hydrogen. All this seems to have been discarded, and the distribution of the various gases at these enormous heights has itself become one of the variables which the physicist is authorized to adjust—within limits, of course! As for the total amount of gases of all kinds, the authorities agree that in the lower levels of the ionosphere it is manyfold greater than is needed to supply the requisite number of free electrons, but at the uppermost levels the margin seems to be slight.

As to the ionizing agent it is taken to be mainly (though not entirely) ultra-violet light proceeding from the sun. But does one observe a large amount of ultra-violet light of suitable wavelengths, when one points a spectroscope at the sun? Quite the contrary! One observes no such light at all. The spectra of sun and stars are sharply cut off as if by an axe, before the wavelength is reached where ionizing power begins.

Yet it seems very unpalatable that the sun and the stars should all have conspired together to end their spectra at a wavelength the same for all, especially since the luminous bodies on earth that most resemble the stars are at their brightest beyond that cut-off. Let us, however, suppose that the sun *does* pour out a flood of light of the far ultra-violet, which never comes down to earth because it is all spent high overhead in transforming the upper air into a sea of ions. This amounts to killing two birds with one stone, or rather, inducing two birds to kill one another. ¶

Indeed if there were far ultra-violet light of the sun coming down to the ground, one might be more perplexed at its ability to get here than one actually is at its stoppage. I do not know how often in science a person who postulates something is gladder because he cannot observe it than he would be if he could, but this at any rate is one example. It will be divined that people

often are tempted to account for details of the ionosphere by making detailed assumptions about this unperceivable part of the far ultra-violet spectrum of the sun, and such is indeed the case

Now I come to the major part of this article, which pertains to the adjustment of this imagined ionosphere to the facts about the reflection of wireless signals

When electromagnetic waves shaped into wireless signals are shot up into the sky, they come bouncing back as though from a heavenly mirror. This can be inferred from the well-known fact that wireless messages travel around the world, and it was so inferred at the beginning, nearly forty years ago. For the last fifteen years there have been special methods for exhibiting and examining these reflections, and here I will draw my evidence from one of these,

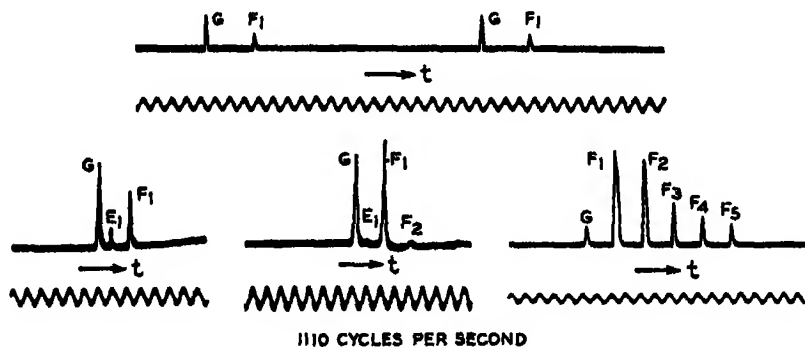


FIG 1 Records of echoes G , outgoing signal, E_1 , echo returned after one reflection from F-layer, F_1 , echo returned after one reflection from F-layer, F_2 , F_3 , F_4 echoes returned after two to five reflections from the F-layer, with intermediate reflections from the ground (Appleton and Builder)

the "echo-method" This method was invented by Breit and Tuve at the Carnegie Institution of Washington—the institution from which, it will be seen, most of my figures come, though the inventors of the method have long since turned their interest to nuclear physics. In this method a wireless signal, or pulse, or group of waves (all of these names are in use) is shot up from a sender, and the observer times the interval between the going of the signal and the returning of an echo. Usually the sender and the receiver are almost side by side, so that the paths travelled by the outgoing signal and the returning echo are nearly coincident and nearly vertical. Oscillograph records of outgoing signals (G) and echoes (E , F) are shown in Fig. 1, the time-intervals between

departure of signal and returns of echoes being indicated by the wavy lines beneath.

It is the ionosphere which is the mirror to the signal. But the ionosphere is an extremely tenuous region, perhaps a trillion times thinner than the thin air which we breathe. Probably this seems strange, and perhaps the reader would feel happier if I were to say that up in the sky there is a hard solid mirror like the celestial dome imagined by the Greeks, made for example of smooth and shiny silver. Well, I cannot say that, but nevertheless I can say what comes to the same. *when visible light is reflected from a hard solid mirror of silver, it is not the hardness of the silver which is doing the reflecting, but an enormous number of roving electrons diffused through the pores of the metal.* A mirror of metal is a container for electrons, and in the upper air there are electrons without a container, and reflection occurs from both.

The ionosphere therefore is the mirror for the echoed radio signals, but now I change the metaphor, and refer to it as the "ceiling"—in the sense lately given that word by aviators. The ceiling for an aviator (as I understand it) is the distance to which one can look up from the ground by the aid of visible light, and the ceiling in my present sense is the distance to which one can look up by means of wireless waves. I introduce the symbols f for the frequency of the waves and N for the number of free electrons per unit volume, e and m for the charge and mass of the electron, and I give the fundamental equation of the theory. *For waves of frequency f the ceiling is at the lowest level where N is given by the formula*

$$N = \pi m f^2 / e^2.$$

When an observer gets an echo for a signal of frequency f , he knows that somewhere overhead there is a level where N has this value.¹ Echoes are observed for frequencies so high that N by the formula comes out at a million (free electrons per cc), sometimes even at ten millions. Signals of higher frequencies yet have been sent up and have failed to return, and therefore N has an upper limit and we are not required to imagine an ionosphere indefinitely rich in its content of free electrons.

Not only does the return of the echo prove the presence of

¹ I pass over a controversy now in progress as to whether the right-hand member of the equation should be multiplied by $3/2$, to take account of the interaction among the free electrons themselves. Most published N -values are based on the equation here given.

the ceiling, but also—and this is far more—it enables the observer to estimate how high the ceiling is. Were signal and echo to travel everywhere with the speed c (of light in vacuo) the altitude of the ceiling would be one-half the product of c by the delay T of the echo. Actually this quantity $\frac{1}{2}cT$ is always computed from the data by the experimenter, and published as his result; but it is called by some such name as “equivalent height” or “virtual altitude,” because the idea that the signal travels always with the speed c is known not to be true. The difference between equivalent height and actual height may be small or it may be serious. Before examining it, we must consider equation (1) a little more closely.

Equation (1) is really a partition between two inequalities. If waves of frequency f fall normally on a stratum of free electrons numbering N per unit volume, then.

$$\begin{aligned} &\text{the waves penetrate the stratum, if } N < \pi m f^2 / e^2, \\ &\text{the waves are reflected from the stratum, if } N > \pi m f^2 / e^2. \end{aligned}$$

Thus the return of an echo of frequency f does not strictly prove the presence of a stratum where $N = \pi m f^2 / e^2$, but only the presence of a stratum where N is equal to or greater than $\pi m f^2 / e^2$. However it is always assumed that N varies continuously with increase of height above the ground, starting from the value zero in the lower atmosphere. Contingently on this assumption, the ceiling for the echo does occur where N just attains the value given by (1). The signals would indeed be reflected from strata of higher N -values if ever they got so far, but they never do.

I may seem to have said that N is assumed to increase continuously with increase of height above ground—but this is not so. The data instead imply a structure of the ionosphere which at its simplest is like that shown in Fig. 2A; the curve of N versus height has maxima and minima. Let us reason back from this inferential curve, and find out of what kind the data must be which imply it.

To do this, let us solve equation (1) for f , obtaining equation (2).

$$f = \sqrt{Ne^2/\pi m}. \quad (2)$$

Again this is really a partition between two inequalities: a layer of electrons of a given N -value reflects all waves of frequency less than $\sqrt{Ne^2/\pi m}$, allows all waves of greater frequency to penetrate it. Let us further apply names “E-layer” and “F-layer” to the two humps in the curve of Fig. 2A; the symbols N_E and N_F to the

ordinates of the two peaks in that curve, the symbols f_E and f_F to the f -values linked by equation (2) with those N -values. With a moment's thought it will now seem obvious that signals of frequency less than f_E will be reflected from the E-layer; that signals for which f is between f_E and f_F will be reflected from the F-layer, that signals of frequencies greater than f_F will not be echoed at all, and that there will be a gradual increase in the delay of the echo as f increases from small values toward f_E , a sudden sharp increase at f_E , and another gradual increase as f continues to mount toward f_F .

Our backward-moving train of reasoning thus leads us to suppose that the plot of data whence came the inferential curve of

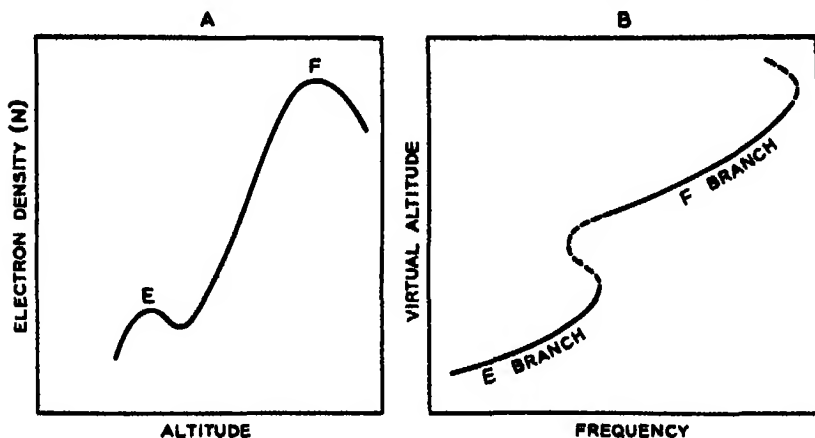


FIG 2 A The "curve of inference" conjectural dependence of number N of electrons per unit volume on true altitude h B The "curve of data" dependence of virtual altitude h' of ceiling on frequency f

Fig. 2A must have looked like the continuous curve in Fig. 2B. There must have been two branches of finite extent, each curving upward toward the right, the second must have begun at a point some distance vertically above the point where the first left off, leaving a gap between.

Actual curves of equivalent height versus frequency will now be shown in great profusion, but they will not look exactly like the continuous curve of Fig 2B, though they will exhibit curving branches and gaps between branches. A few words of explanation of the difference will now be in order.

The difference springs from the fact that in plotting Fig. 2B I was tacitly assuming that equivalent height is in strict proportion

to true height of ceiling, *i.e.* that the speed of the signal is always the same and equal to c , whereas actually this is not so. The signal-speed is less than c , and the equivalent height exceeds the true height. Over most of the length of each branch the excess is thought to be negligible. But (and this perhaps will sound natural) the signal is more slowed down, the nearer N approaches to the critical value given by equation (1)—call it N_{crit} —which turns the signal back. The excess of equivalent over true height is therefore magnified near the gap—and on *both* sides of the gap—between two branches. It is easily seen that each of the branches will thus be curved upward as it approaches the gap, whether from the left or from the right. If N is close to N_{crit} over only a narrow region, the gap will be still clear. But if the region in which N is almost equal to N_{crit} is extended over a great depth of air, the “distortion” (as I will call it) will be very marked. The curve of equivalent height against frequency will then display what looks no longer like a gap, but instead like a peak or a crinkle.

Now we turn at last to a few selected examples of the richly abundant curves of equivalent height versus frequency. They are abundant because nowadays they are made by mass-production. A machine sends up one signal after another, receiving and recording each echo, varying the frequency slightly from each signal to the next, and itself tracing the curve on photographic film, after fifteen minutes it has plotted the curve over the whole frequency-range from 0.516 to 16 Mc (millions of cycles per second) and immediately it recommences, plotting day after day and night after night indefinitely (Figs 5, 7 and 8). These are the characteristic curves or patterns of the ionosphere—not indeed generally so called, and the lack of an acceptable brief name is much to be regretted.² Even if there were no theory at all of them, there would by now be many correlations between their curious shapes and such variables as the time of day, the season of the year, the stage of the sunspot cycle, the presence or absence of magnetic storms. But we have seen that there is a theory, and most of my pictures are chosen to illustrate it.

Figs. 3 and 4 show curves obtained not with the machine, but by ordinary plotting of separate data. They show admirably the gap between E-branch and F-branch at the frequency denoted by f_x , and the F-branch bending upward as it travels off to the right.

² Appleton and his school in England call them “(h' , f) curves”

something entirely different. The signal is split into two by the influence of the magnetic field of the earth, and these travel at different speeds so that one echo returns earlier than the other.

I say that this splitting of the signal is "due to the influence" of the magnetic field of the earth, so as to avoid suggesting a direct effect. No magnetic field of whatever strength could act upon the signals, if they were travelling through vacuum or through

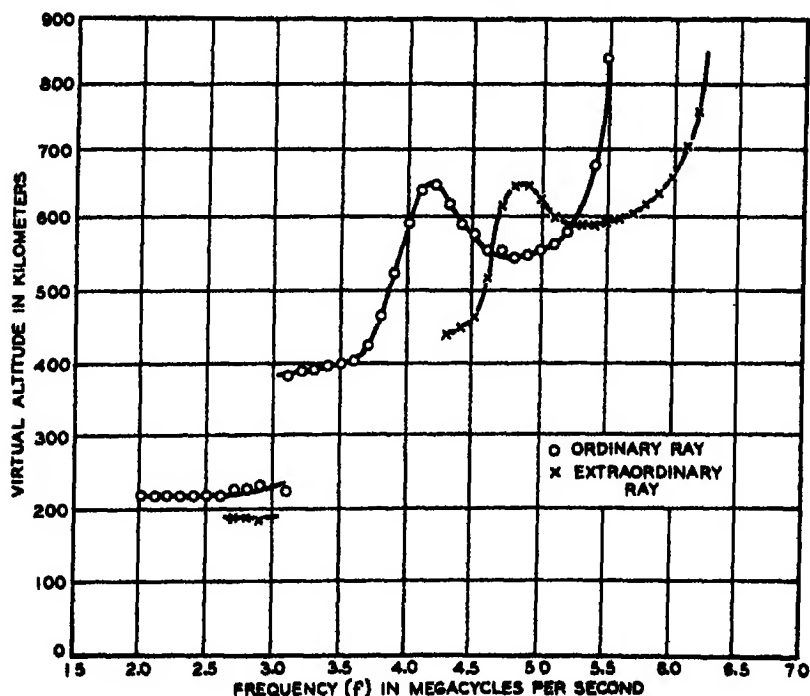


FIG 4 Another example of (h', f) curve taken with sun high in the sky (Appleton)

non-ionized air. Nothing has been said in this article of the fundamental theory, except equations (1) and (2) and their context, but now I add two sentences concerning it. *The ionosphere acts on the signals by virtue of the motions of the electrons, which are set into oscillation by the waves. The magnetic field of the earth modifies these motions, and through them, the signals and the speed thereof.* The modification has many interesting details, of which I can here mention only one more. The sidewise displacement between the curves depends on the mass of the ions, and its value as observed turns out to be the correct one for ions of electronic mass. This is why we must postulate free electrons in the ionosphere. Were

the ions all of atomic mass or larger, the two curves would not be far enough apart to be distinct. But as I intimated above, this argument does not prove that there are not also massive ions in addition to the free electrons.

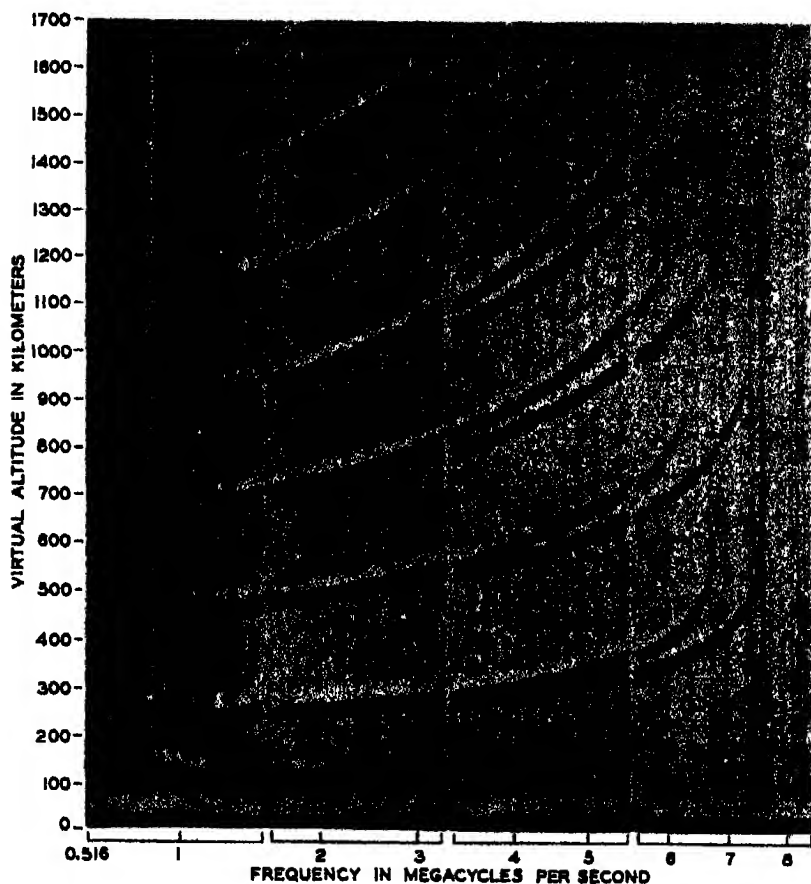


FIG 5 Example of (h', f) curve without crinkle, sun low in the sky, taken with multifrequency apparatus (Carnegie Institution of Washington)

So far the figures have shown the patterns of the normal or healthy ionosphere, such as have been taken by the thousand in the last few years. Data are now preserved from every year since the early thirties, though not nearly so ample from the earlier years as from the later. From these there shines out another connection between ionosphere and sun.

Everyone is more or less acquainted with the "sunspot cycle,"

a sort of periodic fever of our luminary, which manifests itself most strikingly in eruptions of sunspots varying in abundance between a maximum and a minimum. The maxima of the sunspot cycle come about (not very exactly) eleven years apart, the minima fall between the maxima but not necessarily just half-way between. During the early thirties a minimum occurred, during

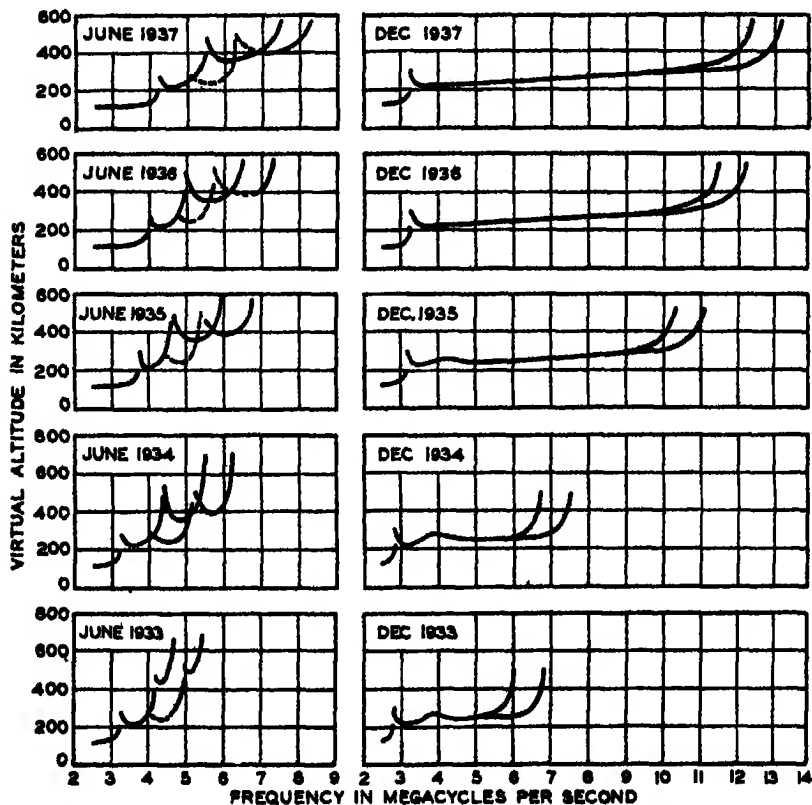


Fig 6 Dependence of (h', f) curve on season and on the sunspot cycle (Smith, Gilliland and Kirby)

the middle and late thirties a maximum was approaching. During this approach to the maximum, the curves exemplified by Figs 3 and 4 and 5 have been stretching steadily farther to the right (Fig. 6)! This means that the ionosphere has been getting capable of echoing signals of higher and higher frequencies, therefore steadily more populous with electrons

The suggestion then is, that as the maximum of the sunspot cycle nears, the sun emits more and more of the kind of light which

ionizes the upper air. This sounds paradoxical. how shall the sun emit more light when its surface is more liable to partial darkening? Well, anyhow we know that it does, even of the non-ionizing light which penetrates all the way through the atmosphere down to the ground. The sunspots are evidently incidental: during the lifetime of a group of large ones the brightness of the sun may be somewhat abated, but on the whole it is at its greatest in the years when spots are commonest. Actually, for the light which comes through to the ground, the difference between years of sunspot maximum and years of sunspot minimum is about 15 per cent. But to judge from the data already assembled for the present cycle, N in the E-layer has gone up some 60 per cent and N in the F-layer no less than fourfold, from minimum to maximum! One therefore assumes that the appropriate kinds of ionizing light, in the far ultra-violet spectrum of the sun, vary much more considerably in strength than does the non-ionizing light. This is an instance of interpreting details of the ionosphere by making postulates about that part of the solar spectrum, alas unverifiable.

Systems in sickness are usually worth attention, because of the sickness itself and because of the light which they may shed on systems in health. The ionosphere seems to be subject to two sorts of malady, sudden in their onset, violent while they last, and vanishing without a trace. These are exemplified in Figs. 7 and 8

In Fig. 7 the neat sharp curves of the pattern are smeared and smudged, and sometimes all but blotted out. It looks as though the ionosphere were in convulsions, and so indeed it is. This is a "magnetic storm," an event of a type well known since the debut of telegraphy; known also in one respect to the Eskimos and other tribes of the far North, since it may be attended with very splendid auroras. Magnetic storms vary in abundance with the stage of the sunspot cycle, and very big ones occasionally synchronize with very big sunspots. They are therefore ascribed to a spasmodic radiation from the sun. Yet this radiation cannot be ultra-violet light, for (to give one reason only) the storms are spread over the dark side of the earth as well as over the sunlit side. It is supposed to consist of charged particles, electrons and other ions, emanating from the sun. These have their paths all twisted by the earth's magnetic field, and may therefore rain down even upon that side of the earth which happens to be turned away

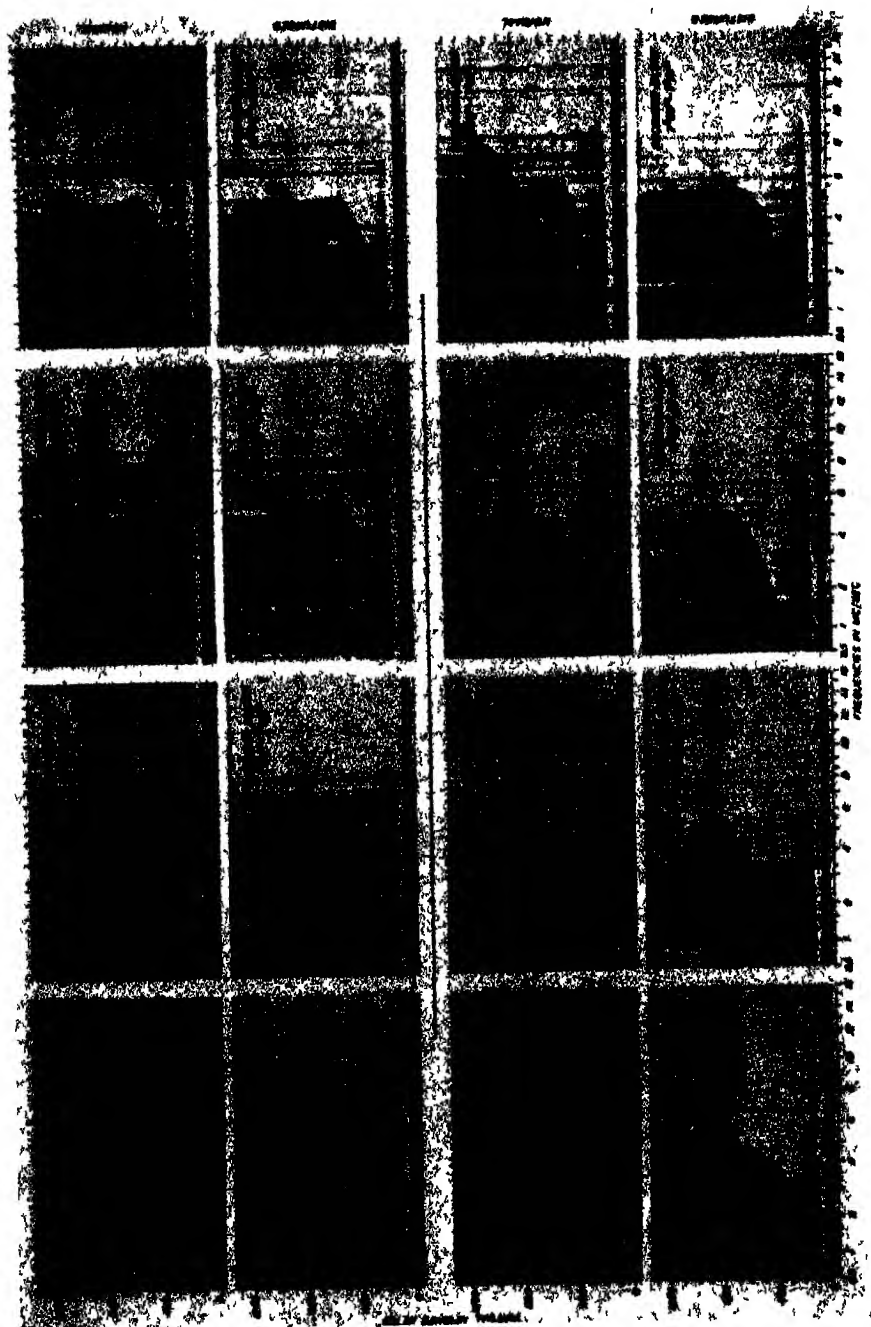


FIG 7 Smearing-out of the ionosphere by magnetic storm. (Carnegie Institution of Washington)

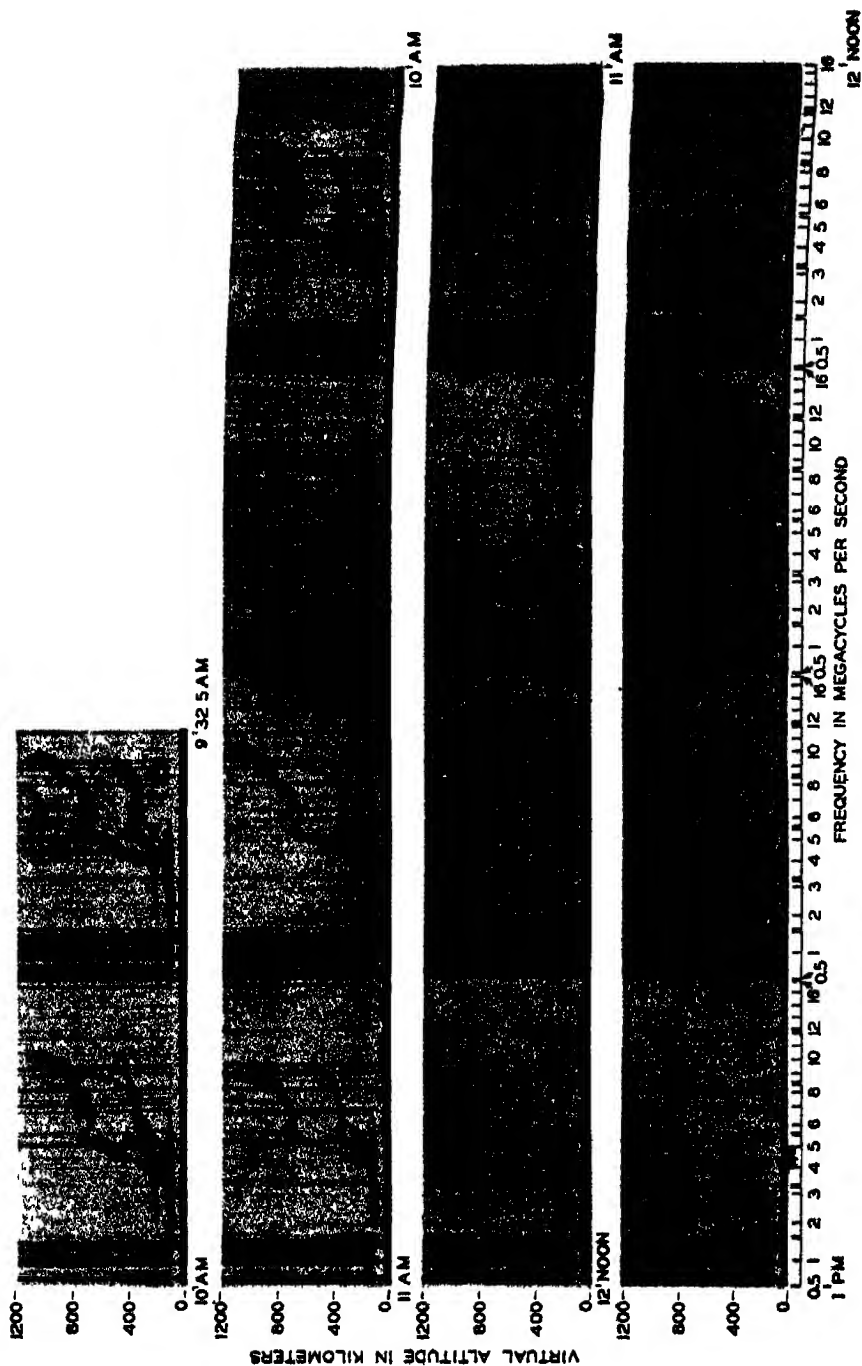


Fig. 8 Advance and recession of a fadeout (Carnegie Institution of Washington)

from the sun. The explanation is not fully satisfactory, and it is hoped that observations on the ionosphere during these storms will help in improving the situation.

In Fig. 8, which shows successive patterns plotted by the machine in consecutive fifteen-minute intervals, the neat sharp curve of the pattern vanishes piecemeal. Subsequent plots show it returning piecemeal. Each bit of the pattern which is present at all is as distinct as though the whole were there, there is no smudging nor smearing. The effect is as if between the ionosphere and the earth a curtain had been dropped, or some sort of an absorbent, engulfing the signals before they reach the ceiling from which they normally would be reflected.

This effect would probably have been called "blackout" had it been discovered during the present war. Having been discovered earlier, it is called "fadeout." It is indeed attributed to a sort of absorbent curtain interposed between the ground and what I have till now been calling the ionosphere, this curtain being a mixture of free electrons and other ions with the atmosphere beneath the E-layer. To put the theory in other words, there is a sudden downward extension of the ionosphere, in which the signals are swallowed up so that they never arrive at the layers where normally they turn about and are sent back to earth.

This sudden downward extension is also ascribed to a spasmodic radiation from the sun. This radiation we may take to be light, for fadeouts are confined to the sunlit half of the earth. Also there is a striking correlation between fadeouts and a special type of eruption (not a sunspot!) on the solar surface: an eruption and a fadeout often begin within a very few minutes of one another. Eruptions are brilliant to the eye and to the photographic plate, and yet the light by which they are photographed or seen is not the ionizing light. One is obliged to postulate a special kind of light emitted from the eruptions, possessing just the qualities required for passing through the F-layer and the E-layer, and then being utterly consumed in making ions in the regions beneath the E-layer. In the broad reaches of the ultra-violet, there is room for imagining a component with properties like these.

But if the ionization is extended downward from the E-layer, why do we not have ordinary echoes returned from a lower ceiling, instead of no echoes at all? This question is answered by referring to the greater density of the air at these lower levels, though I

hesitate to call them "lower" levels, even they being far above the stratosphere and far above the utmost reach of balloons. At these not-quite-so-high levels, then, the air is still so dense that the electrons make many collisions with the molecules as they swing to and fro in the vibration impressed upon them by the signal. At these collisions they lose energy to the molecules, and then replenish their own supply from the energy of the signal, which accordingly is weakened and finally effaced. But if the signals meet no free electrons till they get to the heights of the E-layer, they are not effaced, for at those altitudes the electrons rarely hit molecules, they take from the signal just energy enough to establish their own oscillations, and this small levy weakens the signal only a little.

The reader may wish to be told the actual values of N , the density of free electrons in the ionosphere. This is a difficult request to handle in any limited number of words, because even the normal healthy ionosphere varies so greatly with time of day and stage of solar cycle. Moreover I must confess that there is some doubt as to whether the numerical factor on the right-hand side of equation (1) should be π or $(3/2)\pi$, owing to uncertainty as to whether and how to allow for the forces exerted by the electrons on each other. An order-of-magnitude statement here seems the most that is fitting. The order-of-magnitude of N is 10^5 - 10^6 , that is to say, N is ordinarily in the hundreds of thousands, running at certain heights and times (F_2 layer, in the years of sunspot maximum) past a million. They may be mixed with up to a thousand (!) times as many ions of molecular mass, these still not contributing appreciably to the echoing of the signals.

Luckily the shape of the curve of distribution-in-height of the electrons, with its strange and remarkable maxima and minima, is not affected by failure to be precise as to the numerical value of N . Why the two or more maxima of the curve, the two or more layers of the ionosphere? One maximum or layer by itself would indeed be intelligible. It is obvious that at the top of the atmosphere there are no more atoms to ionize and at the bottom there is no more ionizing light, so that the density of free electrons must be zero at bottom and top and have a maximum between. To explain the several maxima, one ascribes them to different gases of the atmosphere (or perhaps to the same gas in different "states," as atomic physicists say) ionized by different components of the

ultra-violet light. But as yet this is a speculation, for there is no unison as to which layer is to be correlated with which gas

Is it sure that the ionizing agent is only and always sunlight? Quite the contrary! Free electrons persist in the E- and F-layers of the ionosphere throughout the night, even the polar night. This fact does not remove the sunlight from the rôle of major ionizing agent, since the ionization does rapidly fall during twilight and rapidly rises during dawn—a statement which is true not only for ordinary nights but also for those brief periods of obscurity known as solar eclipses, when dawn commences almost as soon as nightfall is complete. But still one must admit extra causes of ionization which operate during the night, and therefore presumably during the day as well when the sunlight predominates over them. These remain mysterious. Inrush of meteors into the high atmosphere has been suggested as one of the causes, and also incessant streams of charged particles similar to those which become intense during magnetic storms

Sunlight is therefore not the only, yet apparently the major factor in maintaining the ionosphere. Not however any sunlight that we ever feel! This portion of the sun's outpourings is so thoroughly consumed above that it never reaches down to the levels where we live. Were it not so consumed, we should not be able to communicate by radio very far over the earth. The reader may think that this is not very important our ancestors lived without radio, why should we worry about lacking it? Well, it is probably quite true that if the ionosphere were not overhead, we should not be worrying about the lack of radio. We should in fact probably not be worrying about anything at all, for we should not be here to worry. The ultra-violet light of the sun, pouring down upon the surface of the globe unhindered, would work changes so severe on organisms as we know them that life would have to be very different, and perhaps impossible. This lethal light is like an enemy, which in attacking a city spends itself in throwing up a barrier against itself, and the barrier not only keeps the enemy out, but is serviceable otherwise to the dwellers in the city.

THE ORIGIN OF THE ANCIENT EGYPTIAN CALENDAR

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(Read April 20, 1940)

ABSTRACT

In 1904 Eduard Meyer stated that the Egyptian calendar was invented about 4231 B.C., and some of the principal Egyptologists of his generation adopted this theory with minor modifications. In recent years it has been realized that 4231 B.C. was far back in the prehistoric period, long before the invention of writing, and of necessity later dates have had to be advanced for the adoption of the calendar as we know it.

Primitive man in Egypt regulated his life entirely by the cycle of the Nile's stages. Nature divided his year into three well-defined seasons—Flood, Spring, and Low Water or Harvest, with the Flood Season, following the hardship of the Low Nile, the obvious starting point for each annual cycle. The Egyptian early recognized the fact that usually twelve moons would complete a Nile year, but his lunar reckoning always remained secondary to his Nile reckoning, and he never adopted solar seasons. However, by about 3200 B.C. he probably recognized the heliacal rising of the prominent star Sothis as a definite phenomenon heralding the coming flood, and he began to count the observed reappearance of the star as his New Year Day. His year he now adjusted to twelve artificial moons of 30 days each, followed by about five days in which he awaited the reappearance of Sothis.

For several centuries the calendar was fixed to the star and thus was approximately correct, but the experience of generations was apparently proving that the perfect year should be 365 days long, and in 2773 B.C. a year of this length was adopted, by the simple expedient of neglecting to readjust the calendar by annual observations. Since no change was ever permitted thereafter, the Egyptian calendar was only correct once in every 1460 years.

THE calendar of the ancient Egyptians was one of man's earliest experiments in almanac making. Certainly it was one of his most enduring, for in the first centuries of the Christian era it was still being used much as it had been during the pyramid age three thousand years earlier. This uninterrupted existence throughout more than half of man's recorded history has given it an almost mysterious quality which has been so intriguing to modern scholars that within my own memory—and even within this last year or so—many an article on its origin has appeared, all differing more or less fundamentally in the story they strive to reconstruct.

The approach to this problem has usually started with a statement made by Censorinus in 238 A.D. to the effect that the Egyptian New Year Day in 139 A.D. fell on July 21, when the bright star Sothis—which we know as Sirius—after having been invisible for

a season, made its annual reappearance in the eastern sky just before sunrise. Since the Egyptian civil year was one of 365 days and that of Sothis was one of $365\frac{1}{4}$ days, this coincidence could only have happened at intervals of about 4×365 years, or in 1317, 2773, and 4231 B.C.¹ Believing that the ancient Egyptian calendar could only have been invented on one of these occasions of coincidence, and further believing that 2773 B.C. fell in the Fourth Dynasty when the calendar was already in use, Eduard Meyer stated in 1904 that the calendar must have been introduced in 4231 B.C.² Eventually Meyer concluded that it was not until 3200 B.C. that Menes, the first historical king of Egypt, united the Two Lands,³ yet he never altered his date for the invention of the calendar, which would thus have been in uninterrupted use for a thousand years before the beginning of Egyptian history—and equally long, we now suppose, before the development of writing. James Henry Breasted⁴ accepted Meyer's theory that the invention of the calendar in 4231 B.C. was "the oldest fixed date in history." Evidently realizing the difficulties which this involved, Breasted eventually attributed the invention of the calendar to a predynastic "First Union" of the Two Lands, which, while it is supposed to have taken place in the forty-third century B.C. and to have lasted for eight hundred years,⁵ has left no written document nor any other tangible trace in history. Eventually Ludwig Borchardt⁶ gave Meyer's theory a momentary support by his

¹ The dates of the so-called "Sothic periods," as given by different historians, vary slightly among themselves. Here, as in the following pages, they are uniformly made to agree with the latest corrected tables by P. V. Neugebauer in *Astronomische Nachrichten*, v. 261, no. 6261, 1937. I owe this reference to the kindness of Otto Neugebauer—who is not to be confused with his namesake, the compiler of the tables.

² *Ägyptische Chronologie* (*Philosophische und historische Abhandlungen der Königlich preussischen Akademie der Wissenschaften*, 1904), p. 41, and (in the same *Abhandlungen* for 1907) *Nachträge zur ägyptischen Chronologie*. In the following pages the references will be cited as Meyer, *Chron.*, or Meyer, *Nachtr.* In 1913 he repeated the thesis in his *Geschichte des Altertums* (3rd edition), § 159.

³ *Die Ältere Chronologie Ägyptens, Nachtrag zum ersten Bande der Geschichte des Altertums* (1931), p. 68, referred to below as Meyer, *Ältere Chron.* This 1931 edition appeared after Meyer's death, and a note by the editor, H. E. Stier, on page 74, calls attention to Alexander Scharff's recent theory that the calendar was invented in 2773 B.C.

⁴ *Ancient Records*, I, pp. 25 ff., *A History of Egypt*, pp. 32, 44.

⁵ "The Predynastic Union of Egypt," *Bulletin de l'Institut français d'archéologie orientale*, XXX (1930), p. 709, *Ancient Times* (2nd edition, 1935), pp. 54, 58. He appears to have been led into this idea partly by one of Sethe's brilliant and seemingly plausible philological exercises, *Der ägyptischen Ausdrücke für rechts und links*, and also by Sethe's *Urgeschichte und älteste Religion der Ägypter*.

⁶ *Die Annalen und die zeitliche Festlegung des alten Reiches* (*Quellen und Forschungen zur Zeitbestimmung der Ägyptischen Geschichte*, Band 1), p. 30. Borchardt's chronology was strongly criticised by Peet, *Journal of Egyptian Archaeology*, vol. VI (1920), pp.

attempt to place Menes close to Meyer's date for the invention of the calendar. This combination appealed to Kurt Sethe whose study of the origin of the calendar,⁷ while unsatisfactory in its conclusions, is a most valuable compendium of all the available material.

In recent years the various modifications of Meyer's theory have been less generally accepted than formerly, and the tendency has been toward the more reasonable hypothesis that the calendar was a product of some later period.

One of the most recent and most ingenious schemes for avoiding this difficulty—but one which unhappily was inspired, I understand, by tempting but false etymologies for the Egyptian season names—was propounded last year by Professor Jotham Johnson of the University of Pittsburg.⁸ He argued that the primitive Egyptian had a lunar calendar until the morning of June 18, 3251 B. C. when Sothis appeared over the eastern horizon just before the dawn of a day on which the new moon occurred. From that day onward the calendar was fixed to Sothis, but gradually the calendar became so far divorced from the terrestrial seasons that it had to be corrected by exactly one whole four-month season on June 18, 2773 B. C.—after which it became the wandering year of the historic period.

Alexander Scharff of the University of Munich had long seen the difficulties inherent in Meyer's theory, and in 1927⁹ he had stated that the calendar must have been invented in 2773 B. C.—a whole Sothic period later than had usually been proposed. Before that date he assumed that the Egyptian reckoned time by some wholly different system, which he did not exactly define but which in one place he seems to say was based on a year of 320 days

149 ff., and by Meyer, *Ältere Chron.*, p. 41, but Borchardt modified it only very slightly in *Quellen*, Band 2, *Die Mittel zur zeitlichen Festlegung von Punkten der Ägyptischen Geschichte* (Kairo, Selbstverlag, 1935). So far as this refers to the XVIII Dyn., it is analyzed—unfavorably—by W. F. Edgerton in *American Journal of Semitic Languages*, LIII (1937), pp. 188 ff. In both parts, although Borchardt's conclusions are unsatisfactory, he makes a great deal of important material available, but I have a feeling that the complexity with which he treats the subject would have made the ancient Egyptian's head spin. References below will be to Borchardt, *Quellen*.

⁷ *Die Zeitrechnung der alten Ägypter*, in the *Nachrichten der K. Gesellschaft der Wissenschaften zu Göttingen, Philologisch-historische Klasse*, 1919–1920. It will be quoted below simply as Sethe, with the pagination of the *Nachrichten*, in which pages 287–320 are of 1919, and pages 28–55 and 97–141 are of 1920.

⁸ *Journal of the American Oriental Society*, 59 (1939), p. 403.

⁹ *Grundzüge der Ägyptischen Vorgeschichte*, p. 54, in *Morgenland, Darstellungen aus Geschichte und Kultur des Ostens*, Heft 12. See note 35 below.

Two years ago Otto Neugebauer,¹⁰ now at Brown University, came out with an extremely intriguing and still more revolutionary theory. He stated that if the primitive Egyptian kept records of the days which elapsed between the successive inundations of the Nile over a period not exceeding fifty years, an average of these periods would infallibly lead to a 365 day year without the observation of any heavenly body whatever. This is unquestionably true in the light of our present day knowledge, but it is doubtful whether it was equally obvious to the Egyptian in the stone age.¹¹ The figures which Neugebauer himself uses give differences in the lengths of the intervals between floods of as much as 80 days in a single generation, and come to exactly 365 days only once in that period.¹² When one Nile year might be only 335 days long and another as much as 415, it is a question whether primitive man would ever, unaided, have arrived at the conception of an average Nile year or would have known how to calculate it, had he thought of it. Setting a calendar by the Nile flood would be about as vague a business as if we set our calendar by the return of the Spring violets. However, Neugebauer's very interesting theory appealed to Scharff as supplying evidence on the nature of the Egyptian calendar before 2773 B.C.—and perhaps even as late as 2000 B.C.—and he now enthusiastically endorses it in part, even if not in all its details.¹³

Before examining the problem of the origin of the calendar afresh a digression appears to be justified on a matter which, even recently, has been the subject of discussions likely to complicate the whole question in the minds of some readers.

¹⁰ "Die Bedeutungslosigkeit der 'Sothisperiode' für die älteste ägyptische Chronologie," in *Acta Orientalia*, XVII (1938), pp. 169 ff. In briefer form, with additional remarks by Jean Capart, in *Chronique d'Égypte*, No. 28, July, 1939, pp. 258 ff.

¹¹ This would require not only a count of the days between successive floods, but a Nilometer, sufficiently massive to withstand the erosion of the inundations, on which comparable stages of the Nile might be measured. Sethe (*Urgeschichte und älteste Religion der Ägypter*, §§ 109 ff.) believed that there was such a Nilometer on the Island of Roda near Memphis, as early as prehistoric times. This is pure hypothesis, as is recognized by Scharff on p. 9 of the article cited in note 13 below.

¹² He uses the figures given by Sir William Willcocks for 1873-1904, before the completion of the Aswān dam. They were doubtless typical of the years before the Nile was artificially controlled. Borchardt (*Quellen*, I, p. 7) uses a non-continuous series of 32 high Niles between 1798 and 1888, which give comparable results.

¹³ *Die Bedeutungslosigkeit der sogenannten ältesten Datums der Weltgeschichte*, read to the Phil.-hist. Abteilung der bayerischen Akademie der Wissenschaften zu München in July, 1939, and (in summary) to the Archaeological Congress in Berlin in August, 1939, and published in the *Historische Zeitschrift*, 161, pp. 3 ff. Scharff seems to approve most of Neugebauer's theory except that (pp. 15, 18) he hesitates to accept a 365 day year as early as the I-II Dyns. See note 35 below.

In modern studies on the historic Egyptian calendar one sometimes reads of a "civil" or "wandering year" and of a co-existent "fixed year" by which festivals might be kept in unvarying relation to the more or less true solar seasons of the inundation, agriculture, and the important reappearance of Sothis.¹⁴ It may be as well at the outset of this paper to state that the ancient Egyptians, from the Old Kingdom to the Roman Period, have not left a single trace of such a fixed calendar. Out of the thousands which have survived from dynastic Egypt, not one document gives equivalent dates in the known "wandering" year and the hypothetical "fixed" year. Furthermore, by the time that relations with the outside world were such as to result in unprejudiced foreign evidence on the customs of Egypt, we find the Egyptian both ignorant of, and unreceptive to the idea.

About 600 B.C.,¹⁵ Thales of Miletus introduced the Egyptian year of 365 days to the Greeks, without hint of any correction being required, and Herodotus, when he was in Egypt about 460 B.C., heard only of a 365 day year and was under the impression that it was not only an accurate measure of time, but that it was the only accurate year devised by any contemporary people. When in 488 B.C. Darius adopted the Egyptian calendar for Persia, it was as an unmodified 365 day year, and after 120 years a whole month had to be intercalated to correct the Persian calendar. The credit for the discovery that a solar year consisted of $365\frac{1}{4}$ days was given by classical authors to Eudoxos of Knidos (408-355 B.C.) whose calculations were probably those used by the Macedonian Ptolemy III Euergetes when, by the Canopic Decree of 237 B.C., he attempted to introduce a $365\frac{1}{4}$ day year in Egypt.¹⁶ In that decree

¹⁴ Sethe, pp. 311 ff., "Das feste Jahr." Meyer (*Chron.*, pp. 31 ff., "Das angebliche feste Jahr,") unanswerably refutes some of the arguments current before the appearance of Sethe's *Zeitrechnung*.

¹⁵ The following paragraph is largely drawn from Sethe, pp. 315-318.

¹⁶ Meyer, *Chron.*, p. 31, translation in J. P. Mahaffy, *A History of Egypt under the Ptolemaic Dynasty* (1899), pp. 111 ff., and in Edwyn Bevan, *A History of Egypt under the Ptolemaic Dynasty* (1927), pp. 207 ff. The decree is definite proof that a fixed calendar was unknown to the Egyptians in the III Cent. B.C. It is dated March 6, 237 B.C., when the flood and the reappearance of Sothis were expected to take place on the last of the Month *Payms* (95 days before New Year Day) and is an attempt to fix the calendar unalterably to the seasons as they were in that year, inconvenient though they would seem to be. It provides that an intercalary day be added, in every fourth year, to the five festivals of the gods at the end of the year, "in order that it may not occur that some of the national feasts kept in winter may come in time to be kept in summer as has formerly happened." Furthermore, in order that Ptolemy Euergetes should always be credited with correcting "the former defect in the arrangement of the seasons," it provides that this sixth god's festival shall be named for the Benefactor Gods—Ptolemy and his wife, Arsinoë.

no reference is made to the idea being native to Egypt, and in fact it appears to have been regarded by the Egyptian people as an abhorrent foreign innovation with which they would have absolutely nothing to do, in spite of the fact that it was said to have the sanction of their own priesthood. It was only in 46 B.C. that Sosigenes of Alexandria¹⁷ evolved for Caesar the Julian Year of 365¼ days, and twenty years afterwards Augustus imposed upon Egypt an era of Julian Years, starting with August 1st, 30 B.C., under the name of the Alexandrian year. Even this—called by the Egyptians the "Greek Year" to distinguish it from the year "according to the Egyptians," or "according to the ancients"¹⁸—was not used by the natives until they had given up their own religion and had adopted Christianity. In short, the whole history of a year with intercalations, as we see it in classical times, is a history of an innovation obnoxiously foreign to the native Egyptian. There is no hint in the whole four centuries and a half covered by the classical literature that the Egyptians had any memory of ever having used a fixed year or ever having recognized its desirability.

The ancient Egyptian calendar of the historical period gives clear evidence that it originated in the climate of the land. Egypt has been, to all intents and purposes, rainless for many thousand years, and all living things in the Nile valley have been dependent on the fluctuations of the river. In the very occasional years when the Nile flood is average, the river is lowest at the First Cataract about the end of May and at the head of the Delta some two weeks or more later. Soon afterwards come the floods from the equatorial rains on the water-shed of the upper Nile during the preceding winter. The river rises slowly at first and then more rapidly, until it reaches its height at the First Cataract about September 1st and a month later at the Delta head where, by the middle or end of October, the highest of the flooded lands begin to emerge once more and the waters fall, until they reach their lowest again the following June.

¹⁷ It was probably Greek mathematicians in Alexandria who told Diodorus (I, 50) in 60-56 B.C. that the Egyptians "reckon . . . their month of 30 days and they add 5¼ days to the 12 months, and in this way fill out the cycle of the year." All other evidence is against such having been the native practise at this time, but the facts were doubtless well known to the Alexandrian Greeks.

¹⁸ Meyer, *Chron*, p. 32. Otto Neugebauer reminds me of the fact that 200 years after the Julian Calendar reform the astronomer Ptolemy was still performing his calculations in the 365 day Egyptian year. This, however, was merely for convenience—not because of chauvinism. The Julian year is still being used in preference to the Gregorian by astronomers, sometimes to the confusion of archaeologists.

During the palæolithic period, whenever the periodical rise of the Nile got under way, the settlements of the primitive Egyptians along the river banks and in the marshes, where they had been established to be near water, would have to be abandoned for others on higher ground. For a space, the Nile people would look down from the desert edge upon a broad lake covering meadows, groves, and swamps, and they would be forced to subsist on fishing, fowling, and hunting. This season in the language of their descendants, the dynastic Egyptians, was *Akhet*—"the Flood." In due course the waters would fall, and the Egyptians would follow the edge of the receding flood across the alluvial plain, pasturing their flocks—once they had domesticated any—on its meadows fast growing green, themselves eating the wild fruits and vegetables which sprang up and ripened in the hot, moist soil, and—when they had learned to save the seed from the last low Nile—strewing it over the wet, black mud where it would sprout and mature very shortly under the cloudless skies. This season in the language of their descendants was *Prōyet*—"the Coming Forth," "the Spring." As the waters descended, man returned to the river and to the permanent swamps where water could be had most easily, and waited for the next flood. This season was called by his descendants *Shōmu*—perhaps meaning at first "the Low Water," but later surely understood as "Harvest-time."¹⁹ Thus the Egyptian recognized but three seasons, and when he adopted a word for "year" he chose a form of the word *ronpy*, "to be young," or "fresh" as of plants, and he considered this year as beginning with the first signs of the rising water which would bring out the verdure once more. These first signs of the awaited flood would be such as primitive hunters and fishermen might learn. First the waters would turn green from the algæ floating down from the swamps of the upper Nile, and the green water would last until the flood was definitely under way. The river itself and the river animals, the hippopotami, crocodiles, and fish whose actions foretold the coming flood, must have been the first harbingers of another cycle of seasons to primitive man. At this stage in his

¹⁹ Sethe, p. 294, Alan H. Gardiner, *Egyptian Grammar*, p. 203. The word *Shōmu* seems to be derived from two words meaning "deficiency" of "water." Later it acquires two meanings: (1) the season of low water, and (2) the harvest. Usually its "determinative" differs with the meaning, but an XVIII Dyn. ostrakon found by the Metropolitan Museum's Expedition (and shortly to be published by W. C. Hayes, *Ostraca and Name Stones from the Tomb of Sen-Mut*, No. 106) writes the word in a date with both determinatives.

development he probably could not count beyond the number of his fingers and surely was not interested in predictions beyond the immediate future

By the time stone-age man first felt the need of some other means of predicting the future stages of the river—probably as agriculture became his chief interest—he must already have become accustomed to counting the phases of the moon. He would early have realized that once the Nile is rising, some four moons must pass before he could sow his seed corn on the emerging mud; how at least four moons again would be required for the grain to ripen, and how a third four moons would pass before the flood would reappear again. Of course, even in the ideally normal year such a count would be only approximate. We know that each moon is theoretically about $29\frac{1}{2}$ days, and twelve moons only 354 days, and that therefore in three successive floods—aside from the irregularities of the river itself—an error of a little more than a moon would have occurred. However, the ideal flood occurs perhaps only once in a generation, and year after year the actual period between one low Nile and the next might be anywhere between 11 and 14 moons. An early or a late flood would sometimes make such a moon reckoning correct, sometimes wrong, but to primitive man the moon still would serve as a ready rule of thumb for predicting the seasons. And after all, the coming of the flood was the start of the new year, regardless of the moon count.

Long after he had evolved a far more practical calendar, the Egyptian still retained some memories of his primitive lunar reckoning. It gave him his subdivision of the year into twelve parts, and the moon gave its name *abod* to each of those parts, as it has to our "months." About 1850 B.C. lunar months, alternately 29 and 30 days long and totally unrelated to the then current civil calendar, still served to set the periods of priestly temple service.²⁰ From then down to Roman times there seem to be traces of lunar months in religious calendars, and it would appear that the coronations of the kings were supposed to take place on the day of the full moon.²¹ In 1100 B.C. astronomical tables still had a technical term for the mid-month which appears to go back to a time when a month was literally a moon and the mid-month

²⁰ Meyer, *Chron.*, p. 52, Sethe, p. 301

²¹ Borchardt, *Quellen*, II, pp. 39 ff., 69 ff. This theory is approved by Edgerton, *Amer. Jour. of Semitic Languages*, LIII (1937), pp. 188 ff. He quotes, however, Černý, *Ägyptische Zeitschrift*, LXXII (1936), pp. 109-118, for an emergency at the death of Ramesses III which caused his successor, Ramesses IV, to be installed immediately.

was full moon time²² Even in Pliny's²³ time it was a popular by-word that the flood might be expected on the new moon next after the summer solstice, and Vettius Valens,²⁴ probably through some misunderstanding of a similar popular saying, supposed that the New Year was on the new moon preceding the reappearance of Sothis.

However, lunar reckoning was always of secondary importance to the Egyptian Those whose calendars are lunar count the start of each day from sunset, when the new moon, the new month, and the new year all take their beginning²⁵ The Egyptians, on the contrary, alone of all ancient peoples, commenced their day at dawn,²⁶ and when their writing was invented the same ideogram stood for both the words "sun" and "day"

Nevertheless, the Egyptian never adopted solar seasons. His seasons were always those of the Nile, whose rise and fall, originating in the distant and unknown south, the prehistoric Egyptian could have had little or no reason to associate with the sun. Only during a brief period in the fourteenth century B C did Egyptian beliefs give full credit to the Sun for its controlling influence on terrestrial life²⁷ But even then the relationship of the Sun to the phases of the Nile was not clearly understood, and it was apparently only in classical times that the solstice was regarded as an omen of the coming inundation. Thus, about 450 B C. Herodotus²⁸ wrote: "the Nile, at the commencement of the Summer Solstice, begins to rise and continues to increase for a hundred days and as soon as that number is passed it forthwith retires and contracts its stream, continuing low during the whole winter until the Summer Solstice comes around again." Later Pliny was told that the river rose at the full moon next after the Summer Solstice, and similar beliefs have been current until modern times²⁹

²² Sethe, pp 130, 136

²³ *Natural History* (ed Bohn), Book 5, Chapter 10

²⁴ Sethe, p 296

²⁵ Sethe, p 119.

²⁶ Sethe, pp 130-138

²⁷ Sethe, pp 28-30 During the reign of Akh-en-Aten (1375-1358 B C) the "Hymns to the Sun" attributed to that heavenly body full control over all nature, including the Nile (Breasted, *A History of Egypt*, pp 371-376, *Development of Religion and Thought in Ancient Egypt*, pp 212 ff) However, Sethe (pp 37 ff) is wrong in assuming an importance for the winter solstice, which actually seems to have played no part in Egyptian thought

²⁸ Book II, 19

²⁹ For Pliny, see above, note 23. For recent beliefs, see E W Lane, *Manners and Customs of the Modern Egyptians* (1836), II, pp. 254 ff, Lepsius, *Chronologie*, p 213

Here it is important to recall certain fundamental points in our problem. First, the rise of the Nile began the new year. Second, the erratic nature of this event was too variable to be itself a measure of time for a people who were becoming more and more cultivated. Third, the moon had proved only a little better. And fourth, the sun did not seem to the Egyptian to have any connection with the question. Yet there is something in man which makes him look to the heavens for his calendar, and the Egyptian, like all others, turned to the sky for some sign that his new year was approaching.

In the cloudless Egyptian nights one of the most prominent, single, heavenly bodies is the great star Sothis. As is the case with all fixed stars, there is a period in each year when Sothis has disappeared from the night sky, rising and setting in daylight. Then one morning its rising is just sufficiently earlier than the sunrise for it to be seen once more for a short time in the dawn as a prominent feature of the eastern sky. About 7000 B C Sothis was visible in the dawn at the head of the Nile Delta around May 21st, which was so long before even an exceptionally early flood that no possible relation could have been seen by any primitive Egyptian between the star and the rising Nile. But since about every 120 years this annual reappearance occurs a day later in the solar year, gradually the star's rising was retarded until, in the latitude of the Delta, it took place just before sunrise on June 17³⁰ in 3500 B C. Very slowly—so slowly that it took generations to make a day's difference—the star's reappearance was delayed further until it came on June 23 about 2800 B C—and the later it came the more certain it was to be regarded as a harbinger of the flood.

The reappearance of this brightest of stars in the dawn is a striking sight. It must have been especially so to primitive man suffering in the fiery heat of an Egyptian June, when the Nile was at its lowest, and his longing for the flood was keenest. Gradually he began to associate the return of Sothis with the first stages of that longed-for high Nile which he grew to expect would follow soon afterwards. When it was that man became conscious of this association we shall never know. Obviously it was an idea which took shape slowly.

³⁰ Throughout Coptic and Arab times, at least, the night of June 17 was celebrated as "the Night of the Drop" when it was believed that a miraculous drop fell into the Nile, causing it to rise. After July 3 the flood was usually obvious enough to be proclaimed daily by criers in the streets of Cairo. Lane, *loc. cit.*

However, it is impossible to doubt the fact that, as early as the dawn of the historic period, the Egyptian was already regarding Sothis as the harbinger of the all important inundation. From one of the royal tombs at Abydos, dating from the first historic Egyptian dynasty, there comes a little ivory tablet which is now in Philadelphia in the University Museum. On it is inscribed a brief and primitive inscription which has been interpreted "Sothis Bringer of the New Year and of the Inundation" ³¹ Coming to us from a slightly later period—but in all probability repeating the words of a much earlier composition—is a passage in the Pyramid Texts describing Sothis as the creator of all green growing things, and hence of the year itself ³²

Here we have statements in the very dawn of history naming Sothis, the recognized master of the annual flood, as the creator of the year—by which of course we may understand the calendar

We need have very little doubt that this association of Sothis with the year was at least as early as about 3200 B C—a date which, it must be realized, can only be fixed approximately—when the Egyptian communities were united by Menes, the first King of Upper and Lower Egypt ³³ Menes also is credited with founding the capital city, Memphis, at the head of the Nile Delta, and it is noteworthy that it was the observation of the reappearance of Sothis at Memphis which was regarded as official throughout

³¹ University Museum, E 9403, Petrie, *Royal Tombs*, II, pls V, 1, VIa, 2, Sethe, *Beiträge zur Älteren Geschichte Ägyptens*, p 63, *Zeitrechnung*, p 294 Borchardt, who apparently never had laid eyes on the tablet, published retouched photographs of it (*Quellen*, I, p 53, n 1), gratuitously adding the hieroglyphic signs for "month 2" in the blank space in its lower right hand corner Unfortunately for his theory, he had not noticed that the inscription on it is incised, and therefore no part of it could have faded out, as he seems to have assumed I have examined both the tablet itself and a photograph which I received through the kindness of Dr Hermann Ranke and can testify that the Petrie publication is accurate More recently, Scharff has described the tablet (*Ältesten Datums*, p 14, note 1) as bearing the notation "the year of the cow counting," but this gives no explanation for the hieroglyphic sign *akhet*, of which Sethe takes account

³² *Pyramid Texts*, 965 a-b, makes the characteristically punning statement that "It is Sothis, thy beloved daughter, which has made the fresh green ('the New Year offering'—*rnp-wt*) in this thy name of year (*rnp-t*)" Scharff, *Ältesten Datums*, pp 17-18, 31

³³ Meyer (*Ältere Chron*, pp 68-69) dates Menes, founder of Dyn I and traditionally of Memphis, to about 3200 B C, admitting the possibility of an error of as much as 100-200 years either way Scharff (*Die Altertümer des Vor- und Frühzeit Ägyptens* (1931), pp 31-32, and *Ältesten Datums*, pp 21-22) dates Menes to 3000 B C However, he seems to approve the recent figures of Farina for the Turin Papyrus, by which the XI Dyn begins apparently in 2143 B C and the I-VIII Dyns. covered 955 years This gives a minimum date of 3097 B C for Menes, without making any allowance for the 18 kings of the IX-X Dyns, except insofar as the X Dyn may have been contemporary with the first half of the XI Dyn.

Egypt during the historic period ³⁴ With Menes began the written records of the lengths of the reigns of the kings, expressed in years, months, and days, which later annalists had no difficulty in combining with later records in the composition of the Palermo Stone, the Turin Papyrus, and the History by Manetho. And another, and most important point, each and every year on the Palermo Stone had an inundation, which would not have been the case had the civil year differed markedly from the natural year, as Scharff has suggested. ³⁵

It must be realized, however, that even when the primitive Egyptian began to recognize the reappearance of Sothis in the dawn as an omen of the coming flood, he had not immediately established what we call a "fixed" calendar. His calendar was without doubt still dependent on an annual observation of Sothis, and a successful observation of the heliacal rising without instruments presents its difficulties. Ludwig Borchardt ³⁶ attempted the observation between 1924 and 1927, with various collaborators stationed up and down the Nile between the latitudes of ancient Thebes and ancient Heliopolis under conditions simulating, as nearly as he could imagine, those of ancient times. Today the reappearance of Sothis is due early in August when a mist often hovers over the inundated valley at dawn, and in addition the modern air is likely to be befogged with chimney smoke. Furthermore, the point of the sunrise on the horizon is nearer to that of

³⁴ This is a tradition preserved by Olympiodorus (writing in 565 A.D.), who stated that the whole land had followed the Memphite observation for the official date of the heliacal rising of Sothis. Cf. Sethe, p. 309. It should be remembered that when Sothis reappeared at Memphis on any given day, its heliacal rising had taken place at Aswān six days earlier.

³⁵ Scharff (*Grundzüge*, pp. 55-56, *Ältesten Datums*, pp. 15, 18) lays great stress upon the fact that the sum of the months and days in two adjacent regnal year spaces at a change of kings on the front of the Palermo Stone totals only 10 months and 20 days. Hence, he argues that the 365 day year was not in use in the first two dynasties. He can not, however, escape the fact that in a similar place in line 4 on the back of the stone, at the change of reign from Sahu-Ré to Nefer-ir-ka-Ré, the total is only 11 months and 13 days, although the 365 day year admittedly existed in the V Dynasty. In these two places where there seem to be intervals between reigns (in the one case of 45 and in the other of 22 days) it is possible that these may be the periods between the death of one king and the coronation of the next, which had to await the presence of the successor in the capital and the occurrence of the full moon. See note 21 above. Further, Scharff forgets that if a year consisted of 320 days only, some years would have no inundation at all.

³⁶ Ludwig Borchardt and Paul Viktor Neugebauer, *Orientalistische Literaturzeitung*, 1926, cols. 309 ff., 1927, cols. 441 ff. In the latter article the authors had the collaboration of members of the Egyptian Survey Department. These experiments (among the most enlightening contributions to the study of the Egyptian calendar) prove that primitive observers could have established a 365 day year only after long experience.

the star rise than it was when the latter took place at the solstice, and the star is therefore more difficult to see in the growing dawn. Hence, the modern observers sometimes did not see the star for as many as five mornings after it should have been visible, and while about 3200 B.C. conditions were better, there must have been many a year when the first glimpse of the star was a day or so late—in which case it would probably be a day too early the next year. To this uncertainty of observation another day would have to be added every fourth year as we add the day to our leap years. Indeed, when the primitive Egyptian first began to keep account of the days between heliacal risings he must have been very far from believing in anything like a fixed year.

Since his year was based on a primitive observation which had to be made annually, each New Year was marked with some uncertainty, but for the First Dynasty Egyptian that was surely not as great a drawback as it sounds to us. The Mohammedan months do not begin, even today, in theory, until one of the faithful has actually seen the new moon in the sunset, and I can well remember how once or twice there was a great deal of doubt, while I was still living in Egypt, as to when the month-long fast of Ramadan might be broken.³⁷ To primitive man a day or so of doubt of this sort would have caused far less bother than it does to his modern peasant descendant, and to him it causes little bother enough.

I suggest, then, that the Egyptian of the time of Menes was starting his year with an observation of the reappearance of Sothis. The divisions of the year were borrowed from prehistoric customs with, however, some important modifications. There were still the three seasons of Flood, Spring, and Harvest—now always of 120 days each. The “moons” were so convenient that they were retained as “months,” even when it was found that they could not coincide with Sothis. From now on for civil affairs they were artificially ordered, each of exactly 30 days—or three ten day “weeks”—and between one reappearance of the star and the next there were always twelve months and a few days over. These extra days “Over and above the Year”³⁸—which came between

³⁷ Lane (*Modern Egyptians*, II, p. 229) describes how the observation of moon-rise was made in Cairo a little over 100 years ago.

³⁸ Sethe (pp. 303 ff.) gives all the existing data on the five intercalary days, but his interpretation—that the year was originally of 360 days only—can hardly be accepted. It fits in, however, with the thesis of Scharff, *Ältesten Datums*, p. 16. The days “Over and above the Year” at first headed the new year (Sethe, *Urkunden des A. R.*, I, pp. 25, 27; Breasted, *Ancient Records*, I, §§ 218, 221), in later calendars they closed the old year.

the last month of the old year and the first of the new, and on which the reappearance of Sothis was to be awaited—were the “Birthdays of the Gods”³⁹ Usually the heliacal rising came after the fifth of them, and according to the now existing texts, on them the births of five gods of the Osiris cycle were to be celebrated. Sometimes six days would pass before the star’s reappearance, and then perhaps the birthday of another god would be celebrated⁴⁰ The next year, or the year after, the star would probably be visible a day before it was expected, in which case the last of the birthdays would be lost for a year. The important thing is that none of the twelve months were ever increased or diminished, and the uncertainty was always confined to these days “Over and above the Year.”

I believe this to have been the situation during the first two dynasties. The commencement of each year was dependent on the heliacal rising of Sothis being observed, with the result that while most years might be 365 days long, every fourth year was probably a day longer, and any other year might be a day or two longer or shorter, depending on the accidents of observation. Yet we know that throughout the later historical period the year differed from the star, and also from the ever variable Nile. The problem, therefore, is when was the Egyptian “wandering” year first used.

Throughout the most familiar part of Egyptian history the “civil” year contained only 365 days, with the result that its New Year Day was “wandering” both in respect to the solar seasons and in relation to Sothis as well. As has been mentioned already, the civil New Year coincided with the reappearance of the star in 139 A.D. and hence, we may suppose, in 1317 B.C. From the period between 1317 and 2773 B.C. there are several items of evidence which demonstrate that the civil calendar was consistently “wandering” throughout that period. From the Eighteenth Dynasty we have calendrical dates for the reappearance of Sothis in 1469 B.C., as recorded in the Elephantine Festival Calendar of Thutmose III,⁴¹ and in 1545 B.C. in the calendar of the Ebers Papyrus

³⁹ Sethe, *Die altägyptischen Pyramidentexte*, par. 1961 c; only in the pyramid of Nefer-ka-Ré (Pepi II). See also Meyer, *Chron.*, p. 40, Scharff, *Grundzüge*, p. 56. Scharff (*Ältesten Datums*, p. 17) states that this passage, while of the Old Kingdom, is not very ancient.

⁴⁰ In the attempted calendar reform of Ptolemy III Euergetes (see above, note 16) the extra day in every fourth year was to be dedicated to Euergetes and Arsinoë in their divine quality.

⁴¹ Sethe, *Urkunden der 18. Dynastie*, p. 827. The calendar is for an unrecorded year in the reign of Thutmose III when the reappearance of Sothis took place on the 3rd

of the reign of Amen-hotpe I⁴² In the Twelfth Dynasty the Kahun Temple Day Book of 1877 B C. fixes the date for that year,⁴³ and in the Eleventh Dynasty the dekan tables symbolizing the heavens on the lids of coffins give dates for the reappearance of Sothis between 2101 and 2021 B C, which are absolutely consistent with the later dates for the same event⁴⁴ Naturally, as we go further back through the Old Kingdom, inscriptions are rarer—both due to the accidents of time and the fact that the earlier Egyptian was less literate than his descendants—and no further observations of the reappearance of Sothis have happened to survive

There are, however, other records which show that the civil calendar was shifting consistently at least as far back as 2350 B C⁴⁵ Meyer showed that the flax harvest in the Twelfth Dynasty, about the year 1940 B C, took place at the appropriate calendrical date in the wandering year.⁴⁶ Furthermore, we know from various in-

Month of *Shōmu*, Day 28 This is 19 days later than the date given in the Ebers Papyrus calendar (see next note), and hence there must have been an interval of about $4 \times 19 = 76$ years between the two calendars

⁴² Sethe, *op cit*, p 44, *Zeitrechnung*, p 313, Meyer, *Nachtr*, p 7, Edgerton, *Amer Jour of Semitic Languages*, LIII (1937), pp 195 ff, where the calendar is dated to 1530 B C The calendar is for the 9th year of Amen-hotpe I when the reappearance of Sothis took place on the 3rd Month of *Shōmu*, Day 9

⁴³ Borchardt, *Zeitschrift für ägypt Sprache*, XXXVII (1899), p 99, Meyer, *Chron*, pp 51 ff Scharff (*Ältesten Datums*, pp 19, 21, 31) seems to believe that observations of Sothis began only at about this time

⁴⁴ They tabulate the stars and constellations as they rose on each of the twelve hours of the night, at intervals of ten days, disregarding—probably for simplicity's sake—the five intercalary days at the end of the year Four coffins from Asyūt (Chassinat and Palanque, *Fouilles dans la Nécropole d'Assiout*, p 127, pl XXV, and p 196, Lacau, *Sarcophages antérieurs au nouvel Empire*, II, p 107, cf Sethe, p 306, n 3, and p 43, n 1) and one from Thebes (published only in a preliminary report by Winlock, *Bulletin of The Metropolitan Museum of Art*, Nov, 1921, Part II, p 50, fig 24) set the reappearance of Sothis in the XII hour of the night between the 171st and 180th days of the year A fifth (Chassinat and Palanque, *op cit*, pp 117–118) is of the same type but only goes to the 160th day A sixth coffin (from Asyūt, Chassinat and Palanque, *op cit*, p 145) sets the reappearance between the 181st and 190th days The situation shown in the first group of coffins was such as existed from 2101 to 2061 B C, when Egypt was reunited by Neb-hepet-Rē Mentu-hotpe That shown in the last-mentioned coffin is the condition as it existed between 2061 and 2021 B C These are dates in the XI Dyn, agreeing very well indeed with our knowledge of Egyptian history and archaeology They, furthermore, show that these Middle Kingdom dekan tables were kept up to date by periodic corrections

⁴⁵ Often we can not be certain of the exact nature of acts described in documents bearing calendrical dates, and therefore cannot use them in controlling the seasons described Thus, builders' marks from Lisht (Lansing, *M M A Bulletin*, April, 1933, II, pp 5–8; November, 1933, II, p 6) are dated between March and September, but they do not define the operations recorded sufficiently to be used as a check on the calendar of the period

⁴⁶ Meyer, *Nachtr*, pp 18 ff.

scriptions that the quarrying season was from January through March in the Twelfth Dynasty, or in terms of the contemporary calendar from the 2nd Month of *Akhet* to the 1st Month of *Prōyet*. In the Sixth Dynasty, when quarrying must have been done at the same season, the corresponding calendar dates were from the 2nd Month of *Shōmu* to the 1st Month of *Akhet*, showing that a shift of about 125 days had taken place in the Egyptian calendar in the five centuries between about 1850 and 2350 B.C.⁴⁷

At each of these several dates the calendar was at variance with the true seasons and with Sothis by about one day for every four years which had elapsed since 2773 B.C. The conclusion is—it seems to me—inescapable that in 2773 B.C. the calendar had been in agreement with the star, and in that year the observations on which this relation had depended were discontinued. The date is astronomically fixed as the start of the wandering calendar of succeeding centuries. We have not, however, sufficient knowledge to do more than guess at what was the historic occasion for this all important change.

In all probability Djoser founded the Third Dynasty about 2778 B.C.,⁴⁸ with the famous sage I-em-hotpe as his vizier, and Egypt entered upon one of its most flourishing periods under an all-powerful, centralized government.⁴⁹ Doubtless the census takers, the tax collectors, and the hosts of royal scribes who were now managing the land found highly unsatisfactory a year whose beginning depended on the chances of an observation of a star in the dawn. The experience of centuries by now had seemed to show that the year should contain 365 days, and this definite figure was adopted for administrative purposes.

But in terms of this new "civil" year the heliacal rising of Sothis gradually came later by a day every four years, until, about a century or so after Djoser's reign, the inscriptions in the Old Kingdom mastababs call for offerings on two separate New Year Days—*Wepy-ronpet*, "the Opener of the Year," and *Tepy-ronpet*,

⁴⁷ Meyer (*Chron.*, pp. 179 ff.) compared these dates when his chronology placed the mean date of the VI Dyn. at 2500 B.C. and concluded that there was a difference in the quarrying season in the two periods. In the above calculation I have used 2350 B.C. as the mean date of the VI Dyn., following his *Ältere Chron.*, p. 68, and the quarrying season of the two periods becomes identical.

⁴⁸ Meyer, *Ältere Chron.*, p. 68. In *Geschichte*, § 231, he credits Djoser with a reign of 19 years.

⁴⁹ Scharff (*Grundzüge*, p. 57, *Ältesten Datums*, p. 18) believes that the 365 day calendar was invented at this time.

"the First of the Year" ⁵⁰ The first of these festivals, in Twelfth Dynasty calendars, is also "the Coming Forth of Sothis", the second festival in all likelihood was the New Year invented for the calendar when it became definitely and obviously separated from nature.

In the meantime, it must not be forgotten that the date of the first appearance of the Nile flood fluctuates between very wide limits, and for several generations after the fixing of the calendar in 2773 B C the "civil" year would still have been, to all appearances, as closely related to the flood as ever. By the time that the flood always fell outside of the calendrical "Flood Season"—*Akhet*—the "civil" calendar had been so long established that no one had the temerity to do anything about it. It was perhaps at this time, while the "civil" calendar was becoming less and less dependable in foretelling the true seasons, that the conservative priesthood invented the coronation oath which called for the new king to swear—as we are informed—"never to intercalate a month or a day nor to vary a festival but to preserve the 365 days as they were ordained of old." ⁵¹

And so for the next three thousand years the Egyptian obstinately refused to follow a fixed calendar, until he adopted the Alexandrian year with Christianity—and to this latter year the Coptic priest still adheres as uncompromisingly as his ancestors followed the ancient calendar.

In conclusion, it is my belief that his calendar was not an invention made by the Egyptian on any one day at dawn, when a series of phenomena happened to coincide. On the contrary, it was a gradually developed method of predicting approximately the almost unpredictable rise of the Nile. For a few centuries before 2773 B C. it depended on the observation of the reappearance of Sirius, and the resulting self-adjusting year was as true a measure

⁵⁰ Meyer, *Chron*, pp 36, 40, Sethe, p 303. Scharff (*Ältesten Datums*, pp 16 and 19, note 2) seems to be sceptical of this theory of Meyer's, firstly, because in New Kingdom inscriptions the Egyptian himself confused the two festival names, and secondly, because his own theory denies the existence of a Sothic year in the Old Kingdom.

⁵¹ So far as I am aware, we do not know of this oath before its mention by P. Nigidius Figulus of the 1st Cent B C (Sethe, p 310, Meyer, *Chron*, p 31), but it unquestionably goes back to some period when there was a lively memory of such attempts and a reasonable fear of their repetition. No office holder is ever called upon to abjure a crime which has never been invented. Of course, this oath might have been inspired by the attempted reform of Ptolemy III in the III Cent B C, but it is unlikely that such an oath could have been wrung from a king in so enlightened a period.

of solar time as was the much later Julian year. However, as man became more civilized he felt the need of some method of time reckoning more definite than nature itself. In 2773 B.C. he dropped his New Year's observations and took up the 365 day year, which actually brought his seasons back into their original places only once again during his whole history.

A NEW MARBLE BUST OF MENANDER, WRONGLY CALLED VERGIL

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(Read April 30, 1940)

ABSTRACT

This paper presents a new marble bust recently acquired for the Robinson Collection in Baltimore. It comes from Tarentum, a Spartan colony in S Italy, where Livius Andronicus first adapted Greek plays into Latin and where plays of Plautus and Terence based on Menander were popular. It is probably a Roman copy of the head of a statue of Menander sculptured by Cephisodotus and Timarchus, sons of Praxiteles, at the end of the fourth century B.C. or the beginning of the third century B.C. The original stood in the theatre at Athens, where the inscribed pedestal has been found, dating from the end of the fourth century B.C. This type of head has been called Pompey by some scholars and recently one of Germany's leading authorities on sculpture, Lippold, has proposed the name of Vergil. The arrangement of the hair in gracefully curving locks, to be sure, resembles that on portrait heads of the Julio-Claudian family and would be appropriate to Vergil but this treatment is found in sculpture as early as the time of Epicurus in the third century B.C. We know that Menander was much praised by Alexandrian scholars, one of whom ranked him second only to Homer. Julius Caesar was fond of his writings. He was handsome but suffered from strabismus. He was a distinguished man of slender stature, with a look of sadness and seriousness but with a restrained smile of superior humor. All the characteristics fit the Baltimore head of which there are some forty other replicas,—four on this side of the Atlantic, one in Toronto, two in or near Boston, one in Philadelphia, which are not as realistic likenesses.

THERE has recently come from Italy into my collection in Baltimore an interesting marble bust said on good authority to have been found at Tarentum (Fig. 1).¹ It does not seem to be

¹ Perhaps it is the one mentioned by Crome, "Das Bildnis Vergils," *Reale Accademia Virgiliana di Mantova, Atti e Memorie*, XXIV, 1935, p. 70, no. 37, as seen by Arndt and Hartwig at the house of Jandolo on the Via del Babuino in Rome. "Neue neu Altes Palazzo Stück mit Renaissance politur Gute Arbeit, aus auf nackter Büste, die bis unter den Nabel ging, mit Schnitt anstatt hinten ausgehöhlt war." If this is the same bust, the modern nose and bust have been removed, without leaving any trace of such restorations. There is no Renaissance polishing, and Mr. Jandolo insists that our bust is different and dug up in 1938 so that Arndt and Hartwig could not possibly have seen it. Greatest height, 0.375 m., greatest breadth, 0.197 m.; greatest depth, 0.238 m., height from bottom to under chin, 0.139 m., height from bottom to lobe of ear, 0.208 m., height at back from bottom of neck to hair line, 0.017 m.; width of mouth, 0.048 m.; height of chin, 0.047 m., from bottom of chin to lower end of nose, 0.076 m., breadth of nose at bottom, 0.084 m., breadth of nose at bridge, 0.018 m.; length of eyes, 0.046 m., greatest height between lids, 0.012 m.; height from bridge of nose to hair-line, 0.064 m.; breadth of forehead, 0.156 m., height of

of Greek but of some fine-grained Italian marble. It is a Roman copy of some Greek statue, probably of the Greek comic poet Menander, and not an original bust of Vergil, as the type has been called. It is well preserved except for the broken nose, a few scratches on the upper part of the two cheeks under the outer corners of the eyes, on the right forehead, on the lower right cheek, some corrosion on the hair, a few breaks including a small one to the left of the middle of the upper lip which has been restored. On the curving convex bottom is an ancient dowel hole, 0.06 m. from the right side and 0.106 m. from the left side. Though it is not in the middle, it probably helped to fasten this bust with a rather unique base, by means of a dowel, into the top of a full size draped statue. There is a curving depressed line, 0.045 m. from the back, which, with the much greater height of the neck at the front than at the back and with the bottom's smooth slanting edge, would also strengthen the joint with the torso into which it was set. There is a larger hole in the top of the head (Fig. 2) which surely is not original.² It is too large to have held the bronze support of a *memiscus*³ or iron crescent bar, such as is mentioned by Aristophanes and Horace, to keep off the birds. Probably the hole was made in later times to attach the head to something.

The modelling is fine and delicate, truer and subtler than in most of the other replicas. Especially good is the treatment of the skin to suggest sensibility. There is more realism and life than in most of the other similar heads. The head is big and broad at the back (Fig. 2), narrower in front but deeper at the bottom of the face than at the forehead, the face tapering toward the hair. The hair is abundant and curly. It is well cut and arranged with accuracy of detail in interesting, graceful, curving, vibrating, pointed locks, radiating from the crown in two or three major divisions. At the base of the neck is a depressed line⁴ below which hang down twelve or more crescent-shaped

ear, 0.064 m., greatest width of ear, 0.034 m., width of bottom, 0.197 m. hole in bottom, approximately square, ca. 0.081×0.035 m., depth, 0.03 m., hole in top, approximately oblong, ca. 0.05×0.07 m., 0.05 m. deep.

² A similar square hole, not original, occurs in the back of the replica in Toronto, cf. p. 470 below.

³ Cf. Aristophanes, *Birds*, 1114, Horace, *Satires*, 1, 8, 6-7. *At importunas volucres in vertice arundo terret, fusa vetatque novis considere in hortis.*

⁴ This is seen also in many of the other replicas, for example, Crome, *op. cit.*, fig. 14, in the Brandegee, Philadelphia, Boston, and other heads mentioned below.

locks, most of them double; above the forehead at the head's right there are three large double curls turning to the left above two little curls in front of the right ear. Then come three small locks and then after a space three great double swirling curls which start on top of the head and like a reversed sigma curve down over the forehead to the left and end in four points. The locks at the right curve in the opposite direction almost like a spiral down in front of the left ear, leaving a blank triangular space running back into the hair where a curving narrow line like a parting of the hair continues to the crown. The arrangement of the hair, with every strand clearly defined, reminds one of the busts of the Julio-Claudian period, and especially of the time of Caligula. Somewhat similar is the hair on the forehead of busts of Augustus,⁵ such as I excavated at Pisidian Antioch, where the curving toward one another of the fifth and sixth curls from the head's left side and the curving to right of the three main curls on the right side are characteristic of almost all Augustus heads. This does not mean that our head cannot represent an earlier type than one of the Augustan age, even though the bust may have been made at the time of Augustus. The motive of curving locks goes back even to Polychytus and the method, seen in our head, of representing the hair in long disordered strands can go back to the middle of the fourth century. It occurs on busts of Epicurus⁶ and many Hellenistic representations of Alexander and the Diadochi and in busts of Pompey,⁷ a name given previously to our type of head.⁸ It gives a light accent of pathos and emphasizes a certain seriousness or even sadness in the face. The man represented is a man in middle life or even older, say about fifty years of age. The forehead has the horizontal division called "the Michelangelo bar." There are two horizontal slightly sunken, flaccid wrinkles across the forehead and two deep vertical lines at the top of the nose. The eyes are deeply set beneath heavy, swollen, and con-

⁵ Cf. Robinson, *AJ A.*, XXX, 1936, pp. 125-136, O. Brendel, *Ikongraphie des Kaisers Augustus*, L. Montini, *Il Ritratto di Augusto*, F. Frigerio, *Augusto, gesta e immagini*, Alföldi, *RM.*, LII, 1937, pp. 48 ff., Rodenwaldt, *Die Antike*, XIII, 1937, pp. 160 ff.; Dorothy Hill, *AJ A.*, XLIII, 1939, pp. 401-409, L'Orange, *Δαίτυμα Νύσσων*, 1939, pp. 288-296.

⁶ Cf. *Neue Jahrbücher für das kl. Altertum*, XXI, 1918, p. 22, fig. 5.

⁷ Cf. Brendel, *op. cit.*, pp. 58, 59, L'Orange, *loc. cit.*, p. 291.

⁸ Cf. Bernoulli, *Römische Ikonographie*, I, p. 112, pl. 7, *Griechische Ikonographie*, II, p. 118, 21, Studniczka, "Das Bildnis Menanders," *Neue Jahrbücher für das kl. Altertum*, XXI, 1918, pp. 18-15.

tracted eyebrows. The rolls of flesh are especially heavy at the outer corners and produce a big bulge at the top of the nose. The eyebrows project heavily, making the forehead prominent at its base. The eyelids droop. The small, almond-shaped eyes in the inner corners are thrown into a deep Scopasian shadow. The temples are very flat and almost hollow. The nose is long and thin but not sunken and sharply set off at the base as in many other replicas. Deep lines run obliquely downward from the wings of the nose and divide the mouth with thick protruding lips from the broad, flat, sunken cheeks. The cheek bones are heavy and prominent and so emphasize the hollowness of the cheeks which taper toward the nicely rounded chin, which is somewhat flattened and has no dimple as in some other replicas. The high sloping neck shows rolls of flesh with two grooves on the bust's right side (Fig. 3) and a large protruding Adam's apple in front with a hollow beneath it. One interesting feature which occurs in most of the other copies is a certain asymmetry about the face. The nose is not directly in the centre but slants to its left and the wrinkle in the left cheek near the nose is slightly higher than that on the right. The head bends to its right and the ears are not symmetrically placed. Both are swollen but the cartilage of the left ear (Fig. 4) is much more so than that of the right, as if that of a boxer or as if it were a little deformed.

The Baltimore head resembles many of the heads which probably represent Menander but which differ in themselves from one another but not enough to argue for more than one Greek original. Including five on this side of the Atlantic, there are now some forty⁹ replicas known (more than seven, however, are doubtful). One is in the Boston Museum of Fine Arts.¹⁰ Another, after two years of litigation, was allowed by the Italian government to come from the Museum at Tarquinia into the possession of Mrs. Edward D. Brandegee at Faulkner Farm, Brookline, Mass., where I have secured new photographs and

⁹ Crome, *op cit*, pp. 67-71, gives a list of thirty-eight ancient replicas and ten modern ones.

¹⁰ Upper part of a herm and with swollen left ear as in our head. Cf. Bernoulli, *op cit*, II, p. 113, no. 18, pl. XIV, Delbrueck, *Antike Porträts*, pp. xxxiv-xxxv, pl. 20, Hekler, *Greek and Roman Portraits*, pls. 106, 107, Studniczka, "Das Bildnis Menanders," *loc cit*, p. 17, pls. 6, 1, 7, 8; Caskey, *Catalogue of Greek and Roman Sculpture*, pp. 163-164; Crome, *op cit*, pl. IX, 19, pl. X, 20-21, Miss Bieber, *The History of the Greek and Roman Theater*, p. 164, fig. 220.

measurements.¹¹ This head (Figs. 5-7), here published by permission of Mrs. Brandege, resembles our head more closely than the other replicas but the hair is slightly different, with deeper cuts between the locks. An extra one is inserted at the head's left in the triangular space left blank on our head, and other slight differences can be seen in the illustrations. The two lines on the forehead are narrower and deeper. The curve of the lines is more depressed in the middle and less convex. The two vertical lines are carried much higher above the bridge of the nose and touch the lower of the two curving horizontal lines. There are lines beyond the corners of both eyes, "crow's feet", which are lacking in our head. The mouth and especially the lower lip differ. The nose has a much greater depression at the top. But both heads represent the same person at about the same age. In our head there is a more worried expression in the face, a sadness in the eyes and mouth, but a gravity and dignity which seem to resist the vanity of the world with strength, and a vigorous pose of the head. The Brandege and Baltimore heads have more freshness, naturalness, more subtlety, and much better and truer modelling than the somewhat clumsy, lean herm in the Boston Museum,¹² with a slenderer neck but more prominent cartilage, with much weaker eyebrows, and more shallow furrows in the forehead (Figs. 8-9). Our head resembles somewhat those in Corfu¹³ and Copenhagen¹⁴. That in Corfu has a more rounded skull in profile, a higher forehead, and more open eyes. It has deeper wrinkles and many crow's feet at the corners of the eyes and represents an older man but it is the same person. That in Copenhagen has the lips more divided but it has the high triangular space in the hair at right

¹¹ It was briefly mentioned in Bernoulli, *op cit*, II, p 112, 9, *Boston Museum of Fine Arts Bulletin*, XI, 1913, no 65, pp 46-47, fig 3, by Caskey, *op cit*, p 164, also by Studniska, *loc cit*, pp 23, 25, pls 6, 3, 7, 1, 9, 3 (with modern lower bust, removed in my photographs), Hekler, *op cit*, pl 105 b (as in Tarquinia [Corneto]); Crome, *op cit*, pl. XVIII, 39. It was for a time in the Museo Civico at Tarquinia. Total height, 0.34 m, from chin to crown of head, 0.23 m, breadth across eyes, 0.175 m.; thickness from forehead to back of head, 0.23 m.

¹² See note 10 above.

¹³ Bernoulli, *op cit*, II, p 112, 11, Studniska, *loc cit*, p 17, Bieber, *op cit*, p 164, fig 221, Arndt Amelung, *Einselaufnahmen*, 610, 611, Crome, *op cit*, pls VII, 16; VIII, 17-18.

¹⁴ Bernoulli, *op cit*, II, p 113, 17, Jacobsen, *Billedtavler til Kataloget*, pl 31, no 429, Winter, *Kunstgeschichte in Bildern*, 1, 320, 4, Studniska, *loc cit*, p 17, pl 10, 3 and frontispiece; Poulsen, *Ikongraphische Miscellen*, p. 32, fig 10, Crome, *op cit*, p 68, 14.

and otherwise it is almost an exact duplicate, one of the best replicas in which there is a Lysippan character and a connection with the Apoxyomenos, the Agias, the Azara helm, the sandal-binder, and the Eros of Lysippos.¹⁵

The Toronto replica¹⁶ (Figs 10-13) seems to represent the same man but somewhat older, with deeper flaccid wrinkles in the forehead, crow's feet at the outer corners of the eyes, more sunken cheeks and jaws, mouth more open and bigger. On the other hand, the hair is thicker and has not lost the extra lock, which leaves a higher triangle in the retreating hair over our head's left forehead. This may be a forgery, as Crome says¹⁷ without having seen the head. Crome even claims that it is a modern copy of the head from Tarquinia in the possession of Mrs. Brandegee. But the cheeks, mouth, and many other features are different and it resembles just as much other replicas as the Brandegee head. The hair is not "unantik" and the preservation of the nose is no argument against genuineness nor the big hole at the back (Fig. 11). On the contrary, the hole, even though well cut, is modern and shows that the bust was attached to a support in later times. The hole is cut into the ancient hair, though the back of the head and the neck are perfectly finished and meant to be seen. The marble is ancient Italian marble and there are some root-marks and incrustation such as a forger would have difficulty in reproducing to-day. To be sure there is a freshness in the marble and little trace of weathering on the exposed parts. The surface in such protected places as the nostrils and the furrows above the upper eyelids retains marble dust from the working and has a milky whiteness quite different from the mellow tone of the normal well-preserved ancient marble. This freshness is made more conspicuous by efforts to conceal it. The surface of the breaks at the back of the neck has been daubed with mud and appears utterly fresh when the mud is flaked off. The corrosion in the left cheek and the more shallow damage on the right side of the head seem to

¹⁵ Studniczka, *loc cit*, pp 20-21, pl 10, 4-6

¹⁶ Published by me in *Bulletin of the Royal Ontario Museum of Archaeology*, Jan 1926, pp 1-6. Total height, 0.314 m, greatest breadth, 0.20 m. Unknown to Miss Bieber, *op cit*, p 165. Purchased from an English dealer who said he had bought it in Rome in 1925. Not copied from Brandegee head, which was in Boston 1912.

¹⁷ *Op cit*, pp 20, 21, 71, pl XVIII, 40. But S Reinach, *Gazette des Beaux Arts*, VI, 1931, no 106, pp 90-92, reproduces (figs 21-22) the Toronto head and says: "Le plus bel exemplaire a été acquis par le Musée de Toronto."

have been caused by acid, since it is deeper in the hollows than on the high parts, whereas the reverse should be true of normal weathering or mechanical abrasion. Certain details of the break above the right ear are suspicious. One would like to attribute the damage either to the violent removal of whatever may have filled the hole in the back of the head (though far from the hole) or to a fall which should, however, have broken the top of the ear. The break would seem to have been caused by one or two blows of a blunt point which has left its mark just above the ear. The acid was evidently poured over that part of the break in which these marks occur. In the middle of the slight abrasion on the chin Professor Homer Thompson, who has been most kind and helpful in studying the bust, found the concave bottom of a tiny crater which suggests that the abrasion was caused by the hasty removal of a puntillo, something unparalleled on Roman heads. There are many variations, however, in these ancient replicas and the Toronto head might possibly be ancient.

More akin to our head is that which since 1901 has been in the University of Pennsylvania Museum (Figs 14-16)¹⁸. It formerly belonged, from 1897 to 1901, to Mr E. P. Warren of Lewes House, Sussex, England, who bought it in Rome. It was wrongly said, seventeen years later, by Studniczka in 1918, to be in Boston. Mrs L. W. Drexel of Philadelphia gave it to the Philadelphia museum in November 1901. With its big wrinkles almost in relief, with its more sunken Scopasian eyes and deep lines in the face, it seems to represent the same man a little older. It has the nose and mouth perfectly preserved, whereas they are damaged in our head. The mouth is shut and the expression more energetic, whereas the original bronze showed the upper teeth as in Studniczka's bronze cast made from the replica in Copenhagen.¹⁹ The broad, strongly rounded chin is somewhat

¹⁸ Published as a Roman Portrait Head by E. H. H. (now Mrs Dohan) in *The Museum Journal*, V, 1914, pp 122-124, fig 68, with no reference to the illustration and publication in *Not Scav*, 1897, p 148 (she wrongly says it was found at Pausola in Italy). This is the same as Bernoulli, *op cit*, II, p 112, 15 (Lewes House), and Studniczka, *loc cit*, pp 13, 14, 18, 24, pl 8, 1 (three replicas wrongly given as in the Boston Museum, including the Brandeggee and Philadelphia heads), Luce, *Cat of the Mediterranean Section*, 1921, p 172, no 22 (where two replicas are said to be in the Boston museum), Poulsen, *op cit*, p 25, pl 18, Lawrence, *Cl Sculpture*, p 286, pl 102, *Later Gr Sculpture*, p 15, pl 18, Crome, *op cit*, p 69, 22

¹⁹ Studniczka, *loc cit*, frontispiece, p 1

flattened in front, as in the Copenhagen and Dresden²⁰ copies, and the pretty mouth, especially the lower lip, resembles somewhat that of the Apollo Belvedere. The neck, mouth, cheeks, the furrows in the forehead, the ears, and hair are similar to those in the Copenhagen and Dresden heads, but the triangular indentation into the hair at the right is deeper in our head. The upper eyelids are broader and more set in under equally heavy and swollen eyebrows in our head. In the Philadelphia head the upper eyebrows are higher and the corners less sunken than in our head. The lines about the mouth are deeper. Both have a sad and serious expression. In both heads, as in the Boston head, the nose continues the line of the brow, as it does not in many of the other replicas, with a slightly curved profile.

The Baltimore and Philadelphia heads are very similar to those in Corfu and Copenhagen, but finer. The broad skull at the top, the straggling locks of hair, the wrinkles on the forehead, the realistic drooping eyebrows, the deep-set eyes,²¹ the lean face, the well defined, weary mouth, the keen, determined look, the carefully carved neck, and other features show that the original was modelled from life and represented an individual still in the prime of life. But there is a graceful nobility²² and heroic distinction in the head which a realistic Roman head with its physical peculiarities would not have. This is not a Roman head, but the portrait of some Greek who is sad and serious, who knows the frailties of human life, who has experienced himself weariness and ill-health. This must be some important Greek. The type in some ways seems to belong to the second century B.C., but it can easily go back to the beginning of the third century B.C. The tired, drooping eyelids, the drawn face, the suffering expression of the deep-set staring eyes, the pathos expressed by the open mouth agree with what we know of the great Greek comic poet Menander, "the bright and morning star" of the New Comedy, even preferred in the first century after Christ by Plutarch and Dion Chrysostom to Aristophanes. Professor L. A. Post says²³: "Purely as a dramatist, Menander is unsurpassed. He achieves greatness with a minimum of abstraction

²⁰ Herrmann, *Vergleichnisse der ant. Originalbildwerke*, no 198

²¹ Suidas speaks of Menander as having strabismus, στραβὸς τὰς ὀφθαλμοῦ, ὅθεν δὲ τὸν τοῦτ

²² Athenaeus, 248 d and 364 d calls Menander ὁ καλὸς

²³ Cf *Transactions and Proceedings of the American Philological Association*, LXIX, 1938, p 42

and a maximum of intimacy." Miss Bieber²⁴ says that "the portrait of Menander shows traces of physical suffering patiently borne and of a nervous constitution coupled with sensitivity and refinement." Born in Cephisia, a fashionable suburb of Athens, Menander lived from 343/2-292/1 B.C.²⁵ He was well educated. His father was a general but he was induced by his uncle, the comic poet Alexis, to write comedy. He associated with such men as Theophrastus and Epicurus. He attended the lectures of the former and was undoubtedly influenced by his book of "Characters." He served in the army for two years (eighteen to twenty years of age) with Epicurus, whose philosophy influenced him deeply with its idea of serenity in the midst of suffering. He also knew Zeno, the Stoic, and may have learned from him his stoic courage in bearing ill-health, which he mentions in a letter in Alciphron.²⁶ To overcome his nervousness he often went swimming and met his death while bathing at the Piræus. He knew the rulers of the day, Demetrius of Phalerum and Ptolemy Soter who invited him to their courts. He was said to have loved a girl named Glycera who is represented along with Menander in his studio in a marble relief in the Lateran,²⁷ if she is not a personification of the stage. Menander and Glycera also appear in a recently discovered mosaic at Antioch on the Orontes.²⁸ Menander developed the stock characters of the New Comedy which were imitated by Plautus and Terence,²⁹ five of whose extant plays are modelled on Menander. Of Menander's more than 100 comedies we have large parts of five and many fragments of others. Especially interesting are his "Arbitrants", the "Girl from Samos", the "Girl with Bobbed Hair", and the "Hero". Menander was the first exponent of feminism. It is unfortunate that we do not know more about the details of Menander's physical appearance, except that he

²⁴ *Op cit.*, p. 165

²⁵ Cf. Cappa, *Four Plays of Menander*, 1910, Allinson, *Menander* (Loeb Cl. Library), 1921, with reproduction of the Boston head (frontispiece) and the relief in the Lateran (p. 130), L. A. Post has a translation of three plays in Oates O'Neill, *The Complete Greek Drama*, 1938, II, pp. 1121-1190

²⁶ *Epistulas Amatorias*, IV, 15 *οἷστα γὰρ μου τὰς συνθέτας ἐσθραίας, ἄς οἱ μὴ φιλοῦντις με τραπὲς καὶ σαλευώνας καλεῖν εἰδῶσιν*; cf. also Phaedrus, *Fab.*, V, 1, 12 *veniebant gressu delicato et languido*.

²⁷ Bernoulli, *op cit.*, II, pl. XV, Studnicska, *loc cit.*, p. 29, pl. 9, 1, Schreiber, *Hellenistische Reliefbilder*, pl. 84; Bieber, *op cit.*, p. 166, fig. 323, Sieveking Brunn Bruckmann, *Denkmäler Gr und Röm Sculptur*, 626; Hekler, *op cit.*, pl. 108

²⁸ This will be published in *Antioch*, III, 1941

²⁹ Cf. Suetonius, *Vita Terenti*, "O dimidiata Menander"

was an intellectual and handsome man of slender stature, with a look of sadness due to ill-health but with a resigned smile of superior humor. All this fits the marble busts which we have been considering and Studniczka⁵⁰ was undoubtedly right in calling them portraits of Menander because of the similarity to the likeness of Menander in the Lateran relief⁵¹ and on a marble *imago clipeata* medallion with a bust with similar hair in Marbury Hall, Cheshire, England, which is inscribed with the name of Menander.⁵²

Menander was placed second only to Homer by Aristophanes the Grammarian so that I believe that certain double herms (Fig 17)⁵³ represent Menander and a Homer who is not blind as on the coins of Amastris, not Hesiod and Vergil, as Crome says. Surely Aristophanes is not represented, as Miss Bieber⁵⁴ says, since Aristophanes was bald and no baldness is shown. Eupolis⁵⁵ calls him *φαλακρός* and Aristophanes himself jokes about his baldness in the "Peace"⁵⁶. From Plutarch⁵⁷ it would seem that for many years down into late antiquity Aristophanes was known as "the bald man", *ὁ φαλακρός*.

Recently two great German archaeologists have expressed the opinion that our type of head represents the famous Augustan poet, Vergil. Lippold, one of the world's leading authorities on sculpture, in 1912 in his book on *Griechische Portratstatuen* (pp. 89-92) objected to the label "Menander" and in 1918 in *Römische Mitteilungen*, XXXIII (1918), pp 1 ff. definitely put forward the theory of Vergil.⁵⁸ In 1935 Crome published a long article or monograph⁵⁹ in which he assembles all

⁵⁰ *Loc cit*, cf also Bernoulli, *op cit*, II, pp 104-106; Delbrueck, *op cit*, p xxxv, A Korte, in Kroll Pauly Wissowa, *Real Encyclopädie*, sv "Menandros," p 714

⁵¹ Cf especially Bieber, *op cit*, p 166, fig 224, where the detailed resemblance is best shown

⁵² Cf Studniczka, *loc cit*, pl 6, 2, 7, 2, Crome, *op cit*, pl XX, 45

⁵³ Crome, *op cit*, pp 14 ff, figs 1-4, 48-49, 62, Bieber, *op cit*, p 164, fig 222 Cf also Crome, *Arch Anz*, 1935, pp 1 ff. I reproduce the double herm in the Villa Albani, where Menander is at right and not at left as in that pictured by Miss Bieber

⁵⁴ *Op cit*, p 165, n 4

⁵⁵ Schol Aristophanes, *Clouds*, 554, *Knights*, 1291, F Welcker, *Alte Denkmäler*, V, 43

⁵⁶ *Peace*, 765, Schol *Knights*, 550

⁵⁷ *Sympos*, ed. Dübner, II, 1, 12, 634 D, Suidas, s.v. *Μηνροφάνης*. On the meaning of *φαλακρός* cf Phrynichus in Becker, *Anecd Gr*, 1, 16 and *Arch Zeit*, 1859, p 87

⁵⁸ Cf also his *Kopien und Umbildungen Griechischer Statuen*, p. 92.

⁵⁹ *Op cit* (see note 1), pp 1-71, 30 pls with 62 figs.

the busts which are of this type. He argues that the division between the hair on the back of the head and on the neck cannot occur before 50 B.C. but this depressed line of division is found even in the fifth century B.C. in the portrait of Sophocles in the Ny Carlsberg Glyptothek in Copenhagen,⁴⁰ in the marble busts of Plato⁴¹ and of Chrysippus at Copenhagen,⁴² and in a Lysippian head,⁴³ whereas such a sinking generally disappears in Roman heads. Crome points out, as I have above, the similarity in the treatment of the hair to that in busts of Augustus, especially one from Samos.⁴⁴ He finds parallels in a bust of Cicero and other portraits of the first century B.C. But as has been said above, this style of hair is found in busts of Epicurus,⁴⁵ a friend of Menander. It occurs in a portrait of a Greek poet in Copenhagen, dated by Poulsen about 200 B.C.⁴⁶ It is really Greek, found even in portraits of Demosthenes.⁴⁷ It is not Roman and rare in Roman portraits. Poulsen knows only one such example, the Lansdowne bust, now in Copenhagen.⁴⁸ Crome rightly compares a portrait head from Delos⁴⁹ but the publisher Michalowski himself dated this in the second century B.C., and some have dated it even earlier. A bearded head in the Uffizi in Florence shows similar curly locks on the forehead which no one would date later than the busts of Epicurus.⁵⁰

As Poulsen has said,⁵¹ one need only compare the heads shown by Crome in Figs 5-9 and 33-39 with the marble head of the Alexander portrait from Pergamum in Istanbul⁵² to see that the type is not Roman but Hellenistic Greek or even earlier. I have personally examined in detail the signed medallion of

⁴⁰ No. 413, *Einselaufnahmen*, 1980-1981

⁴¹ Hekler, *op cit*, p. 23

⁴² No. 425 a

⁴³ Arndt, *Glyptothèque Ny Carlsberg*, pls 130-131

⁴⁴ *Op cit*, pl XXV, figs 54-55

⁴⁵ Cf. Studniczka, *loc cit.*, p. 22, fig. 5; Poulsen, *Greek and Roman Portraits in English Country Houses*, p. 42

⁴⁶ Poulsen, *Acta Archaeologica*, I, 1930, 31 ff., figs 1 a-c, pl. 3

⁴⁷ Lippold-Arndt-Bruckmann, *Gr und Rom. Portraits*, pls 1113-14

⁴⁸ Poulsen, *Greek and Roman Portraits*, p. 66, no. 47, *Gnomon*, XII, 1936, p. 92, where Poulsen cites his article in *J d I*, XLVII, 1932, p. 77, pl. 6, where he publishes a similar head, comparing it even with Demosthenes' busts and other copies of originals of the third century B.C.

⁴⁹ Michalowski, *Les Portraits Hellénistiques et Romains*, p. 25, pl. 21 (*Délos*, fasc. 13, pl. 1 ff.). Cf. also Krahmer, "Hellenistische Köpfe," *Nachrichten von der Ges der Wiss zu Göttingen*, I, 1936, p. 237, fig. 17

⁵⁰ Lippold-Arndt-Bruckmann, *Griechische und Römische Porträts*, pls 679-680

⁵¹ *Gnomon*, XII, 1936, p. 98.

⁵² Hekler, *op cit.*, pl. 59

Marbury Hall, as Poulsen has, and find that in the structure of the face, in the depth and position of the eyes, the open mouth, the locks of hair on the forehead, it agrees with the so-called busts of Menander. The locks above the ears, to be sure, do not show the same contrast in direction as on the busts but in a late work and in a shield-relief this change is not noteworthy. Moreover, the busts themselves vary in details and the Roman copyists often varied details. In any case the similarity is far greater than between the "Menander" busts and representations of Vergil in the mosaics from Hadrumetum in Susa and of Monnus in Trier⁵³ I can see no resemblance at all. The face of Vergil in the mosaics, the only sure likenesses we have of Vergil, is rough and rectangular, with a heavy under jaw (*facie rusticana*, as Suetonius says), whereas Menander was handsome (*ὁ καλός*). There is a resemblance to the statue of Marcellus in the Capitoline Museum but that does not prove the Roman origin of our bust. The type is similar to that in the Marbury Menander medallion which in the manner of the chiton and himation agrees with the Marcellus-type. So far from proving that our head is Roman, the Marcellus-Menander connection shows that the original goes back to the third century B.C. Menander lived at that time and was very popular at Tarentum where our head was found and where Andronicus and other early Latin writers first brought Greek literature to Italy, translating at first Greek works. Tarentum abounded in popular artists⁵⁴ and loved the drama and all sorts of comedies and farces. Even the originator of the farcical phylaces, Rhinthon, lived and wrote at Tarentum. The fact that six or more replicas have been discovered in Greek lands, at Corfu, Athens, Alexandria, even Iconium speaks rather for Menander than Vergil. The ivy wreath on some of the busts clearly points to a tragic or comic poet. In the theatre of Dionysus at Athens was a seated statue of Menander, the base of which has been preserved with the names of the sculptor, Cephisodotus and Timarchus, sons of Praxiteles,⁵⁵ who were active at the beginning of the third century B.C., and could have represented Menander between the ages of forty and fifty, since he died in 291 B.C. Such

⁵³ Crome, *op cit*, pl XXVIII, fig 60; pl XXIX, fig 61. The resemblance of our head to that of Menander in the Antioch mosaic (see note 28) is greater.

⁵⁴ On the whole subject of Tarentum cf Little, *Harvard Studies in Classical Philology*, XLIX, 1938, pp 218 ff.

⁵⁵ Paus., I, 21, 1, Dio Prus., XXXI, 116, Deibruock, *op cit.*, p. xxxv; Stud nieska, *loc cit*, p 4; Lowy, *Inscr. gr. Bildhauer*, no 108, I.G. II-III, 3, 3777.

a seated statue of Menander with the same type of head appears in the Lateran relief mentioned above.⁵⁶ I am inclined to think that our head is a copy of the statue by the sons of Praxiteles but that there was also in the building at Athens called the Pompeum a painted portrait of Menander by Cratinus who was a painter as well as a comic poet⁵⁷ There was also a picture of Isocrates⁵⁸ and Lysippus' statue of Socrates⁵⁹ in this same building, just inside the walls from the Dipylon cemetery. The base of Menander's portrait was found about ten years ago⁶⁰ The letters of the inscription with the broken-barred alpha cannot date earlier than 150 B.C. and must belong to a renewal or restoration of the walls of the Pompeum building, of which the German excavations have found many traces. The original painted inscription was renewed by one carved on stone. The painting in Athens may have been the inspiration for later paintings such as that found a few years ago at Pompeii, representing Menander seated and reading from one of his plays. This painting has given the name Casa di Menandro to the well-preserved house where it still graces one of the walls⁶¹ Unfortunately, it is not very distinct and cannot be used for or against the identification of our bust as Menander.

So I am inclined to think that the type of our bust copies the statue erected in the theatre of Athens by the sons of Praxiteles, Cephisodotus and Timarchus, about 291 B.C. Arguments have been presented in this paper to show that Vergil is not represented as Crome has argued in his monograph of more than seventy pages. We have as yet no identified statue or bust of Vergil, but we probably do have some forty ancient busts of the greatest Greek comic poet next to Aristophanes. The very number proves that some important person was sculptured. Among these busts in the United States are two similar and very fine examples in Baltimore and Philadelphia, a less fine example in the Boston Museum, and a very good example in Mrs. Brandegee's collection near Boston.

⁵⁶ Cf. Bieber, *op. cit.*, p. 166, figs 223-224

⁵⁷ Cf. Pliny, *N.H.*, XXXV, 140: *Cratinus comoedus Athenis in Pompeo pinxit*

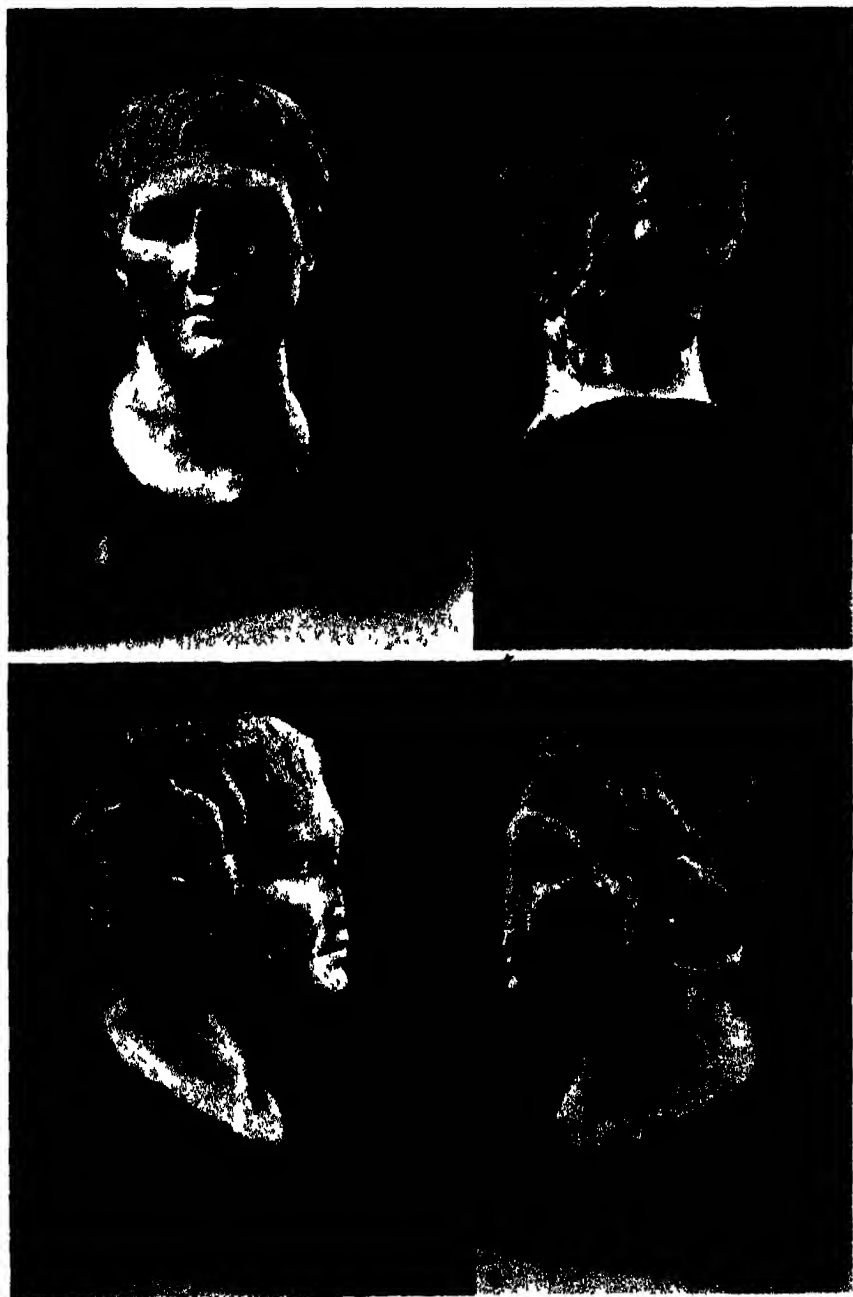
⁵⁸ Plutarch, *Vit. Or.*, 839 c.

⁵⁹ Diog. Laert., II, 43.

⁶⁰ *Ath. Mitt.*, LVI, 1931, p. 12. For inscription cf. *IG*, II-III², 3, 4256 Cf. also *Hermes*, XXX, 1895, p. 535, Pfuhl, *Malerei und Zeichnung der Griechen*, II, pp. 829, 831, 918, Rumpf, in Thieme-Becker, *Allg. Lex. der bild. Kunst*, s.v. "Kratinos", Lippold, *Pauly-Wissowa*, R.E., s.v.

⁶¹ Cf. Maiuri, *Bollettino d'Arte*, I, 1931-2, pp. 241 ff., idem, *Casa di Menandro*, pp. 111 f., *Arch. Anz.*, XLVII, 1932, pp. 501 ff., fig. 22

PLATE I



FIGS 1-4 HEAD OF MENANDER IN THE ROBINSON COLLECTION IN BALTIMORE, MD

PLATE II



FIGS. 5-7 HEAD IN MRS. BRANDEGEE'S COLLECTION IN BROOKLINE, MASS. FIGS. 8-9 BUST IN BOSTON
FIG. 17 DOUBLE HERM IN THE VILLA ALBANI, ROME, ITALY

PLATE III



Figs. 10-13 MARBLE HEAD IN TORONTO

PLATE IV



FIGS. 14-16 HEAD IN THE UNIVERSITY OF PENNSYLVANIA MUSEUM IN PHILADELPHIA

THE POPULARITY OF THE "AMERICAN" ON THE FRENCH STAGE DURING THE REVOLUTION *

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(Read April 18, 1940)

ABSTRACT

During the French Revolution, the French public showed a constant interest in the United States, an interest which had its dramatic counterpart on the stage. Although theatrical tradition prior to 1789 did not favor the presentation of Americans, the advent of the Revolution changed the situation and opened the way for new types of plays.

The American personality most frequently depicted on the stage is that of Benjamin Franklin. He appears in several plays, usually in the company of Voltaire, Rousseau and the philosophers of antiquity, he is glorified as the founder of liberty and the benefactor of mankind. One production shows the printers' guild taking an oath before the bust of Franklin. Then, too, Franklin's philosophy is apparent in so many plays that he would seem to rank next to Voltaire and Rousseau as an influence on public thought directed through the theater during this period.

George Washington, portrayed as the stern but wise leader of the American people, personifies the virtuous citizen produced by the Revolution.

The symbolic "American" appears frequently in patriotic galas during the Reign of Terror. He is endowed with all the qualities which the French considered admirable.

The Revolution served to introduce the real American to the French theater, especially through the character and philosophy of Benjamin Franklin.

DURING the French Revolution, the French public showed a constant interest in the United States. Lafayette's participation in the American Revolution and Franklin's popularity in France had engendered a bond of understanding between the two nations. Now that the French were trying to re-form their own government, they seemed eager to learn as much as they could about the American experiment. News letters from Boston, Philadelphia, New York and Baltimore were printed regularly in Parisian papers, giving full details of economic and political events, Congressional action, movements of important personages, editorials were published on the achievements of the American people. The existence of the United States, if not the appreciation of it, was a vivid thing in the public mind.

This enthusiasm for things American had its dramatic counter-

* With the support of a grant from the American Philosophical Society

part on the stages of Paris, for perhaps more than at any other time in history, the theater in France was a faithful mirror of the changing moods of the French populace. There were, however, two factors which militated against the presentation of Americans on the stage. In the first place, writers knew little or nothing of the country and the life of the people who lived there. The few plays concerning the new world which had appeared before 1789 pictured America as an exotic wilderness inhabited by savages. Then, there was the barrier of theatrical tradition. At the outbreak of the Revolution, there were only three important theaters in Paris—the Opéra, the Comédie Française and the Théâtre Italien—each with privileges of ancient standing and with a definite type of play in its repertory, all catering to the elite society of Paris. Dramas about frontier life or American problems would hardly have fitted into the requirements of any theater. The small theaters, limited almost entirely to the presentation of pantomimes, ballets, or “opéras-bouffe,” were not yet important.

But, with the advent of the Revolution, the situation changed. Theatrical tradition and censorship were abolished along with feudal institutions. New enterprises, aiming to amuse the common people, sprang up along the boulevards, and new types of plays began to appear. Dramatists cast aside the sacred classic rules and wrote about subjects close at hand, usually daily events, seasoning their dialogue with the spirit of the speeches in the National Constituent Assembly. Little by little, plays adopted an aggressive or educational tone. Political leaders soon saw the value of the theater as an instrument for spreading propaganda and directing public opinion, and during the Terror more than thirty theaters supplied diversion flavored with indoctrination to suit all tastes, from the ultra-royalists to the radical republicans.¹

The American personality most frequently depicted on the stage was that of Benjamin Franklin. Franklin's fabulous reputation, acquired during his residence in France, continued through the Revolution with such vigor that, as Bernard Fay put it, “. . . admirer Franklin, l'adorer était une sorte de mode”.² His death in 1790, the anecdotes about him in the American news letters and the publication of his *Mémoires* kept his name before the public

¹ During the Revolution, the word “républicain” was the name applied to the most violent group of sans-culottes.

² Fay, Bernard, *L'esprit révolutionnaire en France et aux Etats-Unis* (Paris, Champion, 1925), p. 196. Also cf. Trahard, Pierre, *La sensibilité révolutionnaire* (Paris, Boivin 1936), p. 40.

eye. Franklin's absence from the American scene during the struggle for liberty may have minimized his usefulness as a dramatic character, and his life did not lend itself to the type of plays which were in vogue during the Revolution, but French writers found the means of injecting their affection and admiration for him into many plays.

Franklin first appears, I believe, in *Le journaliste des ombres*³ which was performed after the news of his death reached France. A journalist has gone to the afterworld to discuss the glorious events which have just occurred in France. Rousseau, Voltaire, the abbé de Saint-Pierre and other illustrious thinkers applaud the new French constitution. Le Maréchal Fabert announces the arrival of the latest passenger in Caron's boat (sc. 19)

Rhadamante le voit, court, l'embrasse & s'écrie,
C'est le rival des Dieux, le Dieu de sa patrie,
Le Vengeur de l'humanité,
L'Apôtre de la liberté
Le Sage de Philadelphie

and Franklin enters. After effusive greetings between Franklin, Voltaire and Rousseau, Voltaire asks Franklin why he has left the earth now that liberty and justice are beginning to reign. Franklin answers

Qu'avois-je à voir sous le ciel qui l'éclaire?
Tout avoit de mon cœur rempli les vœux ardents
J'ai vu l'égalité, ce supplice des grands,
Jeter dans l'univers ses racines profondes.
J'ai vu la chute des tyrans
Et la liberté des deux mondes

Rousseau addresses his praises to Franklin

Toi, qui dans tous les cœurs as gravé ta mémoire,
Bienfaiteur des peuples unis,
J'ose prendre part à ta gloire,
Je leur ai souhaité les dons que tu leur fis

Voltaire tells Franklin that he should be on the banks of the Seine to receive the honors that are being showered upon him by the grateful French citizens, for "Boston te vit naître et l'Europe t'admire". The play closes with Franklin's expression of appreciation for Lafayette's help in the American Revolution.

For Franklin to be so eulogized by both Voltaire and Rousseau

³ Théâtre de la Nation, 14 juillet 1790 By Aude.

shows in what high esteem the French held him. The fervor of the praise must have been shared by the public, for *Le Journal de Paris* (16 juillet 1790) reports that "Cette pièce a été souvent applaudie".

Franklin, the printer, is the subject of panegyrics in *L'imprimeur, ou la fête de Franklin*.⁴ The scene is laid in Germeul's printing shop in which the bust of Franklin is to be dedicated. Germeul swells with pride as he anticipates the ceremony (I, 6)

" . . c'est la que sera placé le buste de francklin, ce patron que j'ai choisi et que toute l'imprimerie ne pouvait manquer d'adopter avec quel plaisir nous allons y contempler son image!"

The play has a thread of romance running through it and also a complication in which Germeul is unjustly accused of printing seditious literature. But the most stirring scenes are those in which the printers of the guild come to pay homage to Franklin.⁵ In a demonstration characteristic of plays of this type, soldiers and printers enter with the bust of Franklin and place it on the pedestal. Then Germeul addresses the group. (I, 10)

ce buste vous offre les traits de francklin· ses cendres
sont à Philadelphie, son nom est partout il vécut libre, et
nous montra comme on doit l'être, rendons nous en dignes,
Et qui peut mieux nous l'apprendre que celui qui né pauvre
et obscur, est devenu le précepteur des nations, le sauveur
d'Amerique, le patriarche de la liberté

André

cette phrase me platt, je la garde.

Germeul

Comme nous, mes amis, francklin commença par être garçon
imprimeur, et tant qu'il le fut, jamais une ligne obscène ne
sortit de ses presses, jamais, il n'emploia ses caractères qu'a
des ouvrages avoués par le gouvernement, et par les mœurs,
par les loix et par la vérité Sa conduite sera la mienne .
j'en fais serment

⁴ Théâtre de Monsieur, 8 avril 1791 By Desfontaines Unpublished Information from manuscript copy No 9284 in the Bibliothèque Nationale. The mistakes in the manuscript have been transcribed

⁵ The idea for the play is undoubtedly derived from a "fête" given in Franklin's honor by the Parisian printers' guild some months earlier. See *Le Journal Général de France* (14 juin 1790) for complete details of the celebration

André

Et le serment est le' notre
*(ils entourent le buste et lèvent la
 main comme Germeuil; brust des cymbales
 et des trompettes.)*

Tous

Oui, nous le jurons

In the finale, the actors sing couplets in praise of Franklin, such as (II, 12).

Si vous voulés au vrai civisme
 décerner l'immortalité
 offrir à qui l'a mérité
 l'hommage du patriotisme,
 Ah! c'est toujours, toujours francklin
 que nommera notre refrain

Chœur

oui, chaque jour, couronne, et gloire
 Chantons célébrons sans fin
 le mémoire
 de francklin

Once more I have turned to the newspapers of the day for the public reaction to this idolatry of Franklin and I find that the play was a favorite during the season. *Le Moniteur Universel* (22 avril 1791) says that ". . . sans avoir eu le succès brillant de quelques ouvrages distingués, elle a fait cependant beaucoup de plaisir". *Le Journal de Paris* (11 avril 1791) approves the "bons principes sur la liberté de la presse, & des traits agréables qui n'ont pas échappé aux Spectateurs" and the critic then goes on to comment on the timeliness of the play "L'action est supposée très moderne: car il y est question de la perte encore bien sensible d'un des plus illustres Fondateurs de la Constitution françoise".⁶ Some days later, *La Feuille du Jour* (17 avril 1791) points out that "le succès de *l'imprimeur ou la Fête de Franklin* s'est soutenu par l'intérêt de l'ouvrage, par les leçons utiles et bien senties qu'il présente sur la liberté de la presse . . .".

As the National Assembly was finishing its task of writing the

⁶ The latter remark could have referred to the death of Mirabeau, who died April 2, 1791, or to Franklin, who was supposed to have contributed to the writing of the French constitution. The fact that there is no direct reference to Mirabeau in the play substantiates the allusion to Franklin.

French Constitution during the summer of 1791, Franklin's popularity on the stage increased. He appeared in a "pièce de circonstance" which was written shortly after Mirabeau's death, *L'ombre de Mirabeau*.⁷ "Voltaire reçoit Mirabeau aux Champs-Élysées et lui pose sur le front la couronne civique. Cicéron, Démosthène, J.-J. Rousseau, Mably, Franklin et Brutus joignent leurs félicitations à celles de Voltaire."⁸ According to *Le Moniteur Universel* (23 mai 1791), the play was given "avec succès . . . Elle a été . . . fort applaudie."

When Voltaire's remains were transferred to the newly dedicated Panthéon in a pompous cortège (11 juillet 1791), "des Médallions portés comme des bannières offroient les images de Franklin, Fondateur de la Liberté en Amérique, de Mirabeau de Rousseau . . ." (*Le Journal de Paris*, 13 juillet 1791). The event was dramatized in *Le panthéon français*, which unites Voltaire, Rousseau and Franklin along with other notables who have passed on from this life.⁹

A contemporary non-dramatic pamphlet makes Franklin the companion of the greatest philosophers of antiquity. Rousseau invites his friends in the Champs-Élysées to follow him to a spot where he has built a temple to patriotism, and

Voilà que sur la route
Ces Messieurs recontrent Solon,
Lacurgue, Franklin et Platon!¹⁰

It seemed fitting to Sauvigny to mention Franklin in *Vashington ou la liberté du nouveau monde*¹¹ when the French ambassador visits Washington on the battlefield to form a treaty of alliance between France and America. In his speech of acceptance, the ambassador refers to Franklin as (IV, 5)

Ce génie immortel, l'homme de tous les tems,
Qui dirigea la foudre et chassa les tyrans,
Politique profond et philosophe austère,
Franklin, cher aux Français

Then again, Chaussard pays homage to Franklin in *La France régénérée*.¹² The allegorical prologue shows "Le Temps" and "La

⁷ Théâtre Italien, 7 mai 1791. By Dejaure.

⁸ Welschinger, *Le théâtre de la révolution* (Paris, 1880), p. 471.

⁹ C. M. Crist, "Voltaire in the Eighteenth and Nineteenth Century Theater" (*Modern Language Notes*, 51, March 1936, p. 151).

¹⁰ *Les Sabates Jacobites*, No. 33-34, 1791, p. 118.

¹¹ Théâtre de la Nation, 13 juillet 1791.

¹² Théâtre de Molière, 14 septembre 1791.

Gloire" discussing the marvelous changes recently inaugurated in France "La Gloire", recounting her travels through the centuries from antiquity down to the present time, finally arrives in America

L'Amérique appelloit mes pas,
 Francklin y fut mon guide,
 Je contemplai de près ce mortel intrépide
 A ses côtes je dûs rester long-temps
 Et ma main sur ses cheveux blancs,
 En partant pour ces lieux déposa ma couronne

The rest of the play is composed of a series of scenes depicting the advantages of the new order of things. The versification and structure may have been faulty, but it was the sort of amusement the public liked in those days. As *Le Mercure Universel* puts it, "*La France régénérée offre un tableau trop riant pour ne pas lui sourire*". (18 septembre 1791)

There is another aspect of Franklin in the theater which is more important than his participation as a character in plays—it is the diffusion of his philosophy. His wise precepts on good citizenship and integrity of character are echoed in many plays of the Revolutionary period. To be sure, the sayings of Poor Richard are not translated verbatim, in fact, dramatists do not acknowledge the source of their ideas—probably because Franklin thought was in the air, so to speak, for any writer to draw on as he pleased. One is as conscious, however, of Franklin's homely philosophy as of Voltaire's ideas on intolerance or Rousseau's pleas for social justice. Almost every new play performed at the small theaters along the boulevards contained some amiable maxim which might well have been culled from *Poor Richard's Almanac* or the *Autobiography*. Sometimes during the Reign of Terror, Franklin's wisdom is mixed with passionate revolutionary dogmas, making a combination which seems incongruous to us today. But the spirit of Poor Richard is always to be found in those plays which attempted to teach good citizenship.¹³ It would seem, judging from my research to date, that Franklin the philosopher ranks next to Voltaire and Rousseau as an influence on public thought directed through the theater during the French Revolution.

George Washington comes into prominence in *Vashington ou la liberté du nouveau monde*, already mentioned. The play deals with

¹³ Cf. *Le club des bonnes gens*, *La famille patriote*, *L'époux républicain*, *Le district du village*, *Encore un curé*, etc.

the attempt of the English to spread discontent in the army of the American revolutionists and ends with the celebration of the treaty between France and America. Washington is the stern but wise leader of the American people, respected by Congress and triumphant in his dealing with the soldiers. In the presence of Congress, the president Laurens addresses Washington in these glowing terms: (III, 4)

O vous! dont la sagesse, en nous rendant vainqueurs,
Sait désarmer la haine et conquérir les cœurs.
Entre tous les noms célèbres par la guerre,
Qu'ont transmis jusqu'à nous les fastes de la terre,
Nul ne peut opposer au peuple Américain
Un héros plus modeste et sur-tout plus humain.

To which Washington replies

J'ai rempli les devoirs d'un citoyen fidèle, . .

And the French ambassador, after eulogizing Franklin, turns to Washington (IV, 5)

Emule du grand homme, immortel comme lui,
Un peuple immense en vous, vengeur de son injure,
Voit l'heureux créateur de sa grandeur future,
Et fier de vos exploits, sur votre front guerrier,
attache avec transport l'olive et le laurier.
Washington, il est beau qu'un si brillant hommage
Soit le prix des vertus, autant que du courage

Elsewhere in the play, Washington is called "le héros si cher à la patrie" (II, 1) and "le sauveur d'un peuple" (II, 2).

It is only natural that Washington should be treated so favorably, for after Franklin, he was probably the American best known to the French public. His exploits as general of the revolutionary army had been publicized some years earlier and now in 1791 his presidential activities and messages to Congress were reported in full in Parisian papers. Washington, like the other Americans in this play, personified the virtuous citizens produced by the Revolution.

As a theatrical production, the play fared badly because it was devoid of literary merit and dramatic interest. However, one critic saw "sous le voile de la révolution Américaine, le tableau d'une révolution plus moderne" and "une Assemblée du Congrès ressemblante à une séance de l'Assemblée Nationale" (*Le Journal*

de Paris, 13 juillet 1791, supplément 80). For the first time the American people and the American scene are faithfully presented on the French stage

The next year, 1792, offered nothing interesting concerning Americans. During the early months of the year, the theater underwent a comparatively calm moment. The Constitution had been written and accepted, and for many people, the purpose of the Revolution had been attained. There was a decided increase in the number of performances of plays from the classic repertory, and new plays lacked the militant spirit recently in vogue. But soon after the fall of the monarchy and the establishment of the republic (August–September, 1792) the radical revolutionists came into power and brought with them the Reign of Terror. Theaters began again to dramatize daily events, to spread propaganda across the footlights and to indulge in dramatic excesses at the behest of political leaders

America was represented anew to the French public in *Asgill*¹⁴ The Asgill incident was much talked about during the Revolution. The English had committed an unwarranted crime by hanging the American prisoner, Captain Huddy. Congress ordered Washington to select a British prisoner of the same rank to be hanged in retaliation. Asgill was chosen by lot. He was finally saved from death by the intervention of the French monarch on a plea from Asgill's mother.

The version performed in 1793 was not printed, but from the newspaper reviews, it is evident that Washington is considered a sympathetic but unyielding general of heroic proportions. The author, while adapting the situation to the taste of the moment, created a play of great dramatic power that assured its success. "L'intérêt qu'inspire le sujet de représailles est d'autant plus vif qu'il retrace des malheurs auxquels désormais nous ne sommes plus étrangers. L'affluence étoit considérable, tout le monde pleuroit, et le succès a été complet" (*La Quotidienne*, 3 mai 1793)

Thereafter, the vogue for "faits historiques", which were in reality patriotic demonstrations, opened the way for a new type of American to appear on the stage: the "American citizen" as a symbol of the man who has shaken off the bonds of monarchical domination and has attained the stature of a free man.

¹⁴ Théâtre Italien, 2 mai 1793. By Marsollier. An unperformed version of the Asgill affair had been written by Le Barbier in 1785. It was devoted almost entirely to a display of American gratitude for French intervention. A third version by Comberousse was played in 1796.

For example, the news of the seizing of Toulon by the Revolutionists threw Paris into a jubilant frenzy. Six theaters presented spectacles in honor of the victory. Picard's version, *La prise de Toulon*,¹⁵ contains an American who has been seized in London, and forced to serve in the army against his will. While he is doing guard duty on the ramparts of Toulon, he expresses his hatred for the English (sc. 2)

moi, Américain, Bostonien, je me battrais contre mes
amis, mes alliés, contre ceux à qui je dois la liberté de mon
pays! non, non, jamais, jamais

Tyrans, tyrans vous m'êtes en horreur,
Craignez ma haine et ma fureur,
Vous avez sauvé ma patrie,
O vous! Français, nos bons amis,
Je ne servirai point parmi vos ennemis

An escaped French prisoner enters cautiously and commiserates with the American on the treachery of the English. The American is merely waiting for a chance to turn on the English. (sc. 3)

Avant la fin du jour, j'aurai déserté, j'aurai joint
mes amis, mes frères. . .

English officers and French royalists plan a sumptuous feast to celebrate the expected defeat of the revolutionary troops which are approaching. The American, seeing his chance to escape, makes his exit saying (sc. 11)

Je cours, braves Français, me mêler dans vos rangs
Et combattre avec vous les Soldats des Tyrans

In the ensuing battle, the revolutionists drive out the English and the royalists. The republican troops rejoice on the ramparts with typical revolutionary bravado. "Le Représentant du Peuple" speaks to the American (sc. 12)

Touche-là, digne Américain, je me réjouis de te voir des nôtres
Tu es un brave homme, je ne te connais que depuis un instant,
& tu t'es déjà bien battu.

L'Américain

Je me bats pour vous, comme vous vous êtes battus pour moi,
je ne fais que mon devoir.

¹⁵ Théâtre de la rue Feydeau. 13 pluviôse an II [1 février 1794]

The "American" is endowed with all the qualities which the French considered admirable, especially his hatred for the English. He must have pleased the French audiences, for one critic reproached the author "de n'avoir pas dessiner plus fortement le rôle de l'américain qui eut été de plus grand intérêt" (*La Feuille du Salut Public*, 12 ventôse an II [2 mars 1794]).

One of the most successful plays in those cataclysmal weeks before the downfall of Robespierre was an allegory called *Le dîner des peuples* by Duval.¹⁶ Mother Nature has invited to dinner all the different peoples of the globe, those in slavery as well as free men. Representatives of various peoples enter and explain their hardships or their joys " . le peuple d'Amérique amis des républicains puisqu'il l'est de la liberté qu'il a conquise comme nous par son courage et sa persévérance, se rencontre avec le peuple français son allié, et l'embrasse " A dinner is prepared, but the nobles blindfold the common people and eat the meal themselves. "Le Temps" cuts the bands from the eyes of the slaves, who kill the nobles and proclaim liberty and equality triumphant.

Here again, the "American" is not an individual, but the symbol of liberty, the friend of the French, the enemy of monarchs and tyrants.

During the Terror, Franklin's popularity continued unabated. The citizens of the Pont-neuf section asked to have one of their old streets re-named rue Franklin (*Les Annales de la République Française*, 9 février 1793) and at a popular festival at Versailles, there was a parade headed by the bust of Franklin (*Le Journal Universel*, 29 août 1793). Even the Jacobins tried to take advantage of the charm that seemed to accompany the word. Franklin.

At the théâtre de la Cité, which was a center of intense Jacobin activity, the actors presented *L'époux républicain*.¹⁷ The central figure is a patriot who has renounced his aristocratic sounding name "Leroi" and has adopted the more republican name "Franklin".¹⁸ The French "Franklin" explains to his friends (I, 10) " . . . dès qu'il a été permis de changer de patron, j'ai pris Franklin pour le

¹⁶ Théâtre du Vaudeville, 15 floréal an II [4 mai 1794]. The play was unpublished, but there is a full account of it in *La Feuille du Salut Public* for 19 floréal an II [8 mai 1794].

¹⁷ 20 pluviôse an II [8 février 1794]. By Pompigny.

¹⁸ It was common practice during the revolution for characters to cast aside their traditional names and adopt the name of some great patriot associated with the fight for liberty, such as Brutus, Aristide, etc.

mien, & j'aime à porter ce nom. Il annonce la franchise de son caractère & la liberté de son pays"

Later in the play, however, "Franklin" pushes his patriotism to the point where he denounces his wife and sends her to the guillotine for conspiring to escape from France. It should be explained, for the sake of propriety, that the author had made her "Franklin's" second wife, young, frivolous, aristocratic and not the mother of his sons. Even so, this is an extension of the American Franklin's character that goes beyond the bounds of probability. Yet this same rabid republican tries to instill into his sons the virtuous precepts of his newly adopted patron

The play had a notable success among the radical audiences for which it was intended. According to *Les Spectacles de Paris* (for 1794, 2, p. 86), it is a work "qui respire le plus pur patriotisme qui mérite un succès brillant." The critic for *L'Abrévateur Universel* (19 février 1794) approves of the play, but invites the author to give "un nom de patriot français à son *Epoux républicain* au lieu de Franklin qui n'est pas national." *La Feuille du Salut Public* (28 pluviôse an II [16 février 1794]) boasts of the fine sentiments expressed in the play. "Ce n'est pas une de ces pièces qui n'ont d'autre mérite que celui des circonstances." Cette pièce républicaine a été jouée avec un ensemble qui fait honneur aux artistes de ce théâtre."

During the Thermidorean reaction, when the hysteria of the Terror was waning, plays with Americans continued to appear, but the "American" element was reduced to a negligible quantity. In *Bella, ou la femme à deux maris* (1795) the characters are all Americans who live in a village near Boston, but one would never suspect it from the situation or dialogue. The chief reference to America is the ever recurrent theme of gratitude to the French for their aid during the Revolution. The next year, 1796, Bouilly wrote *La famille américaine*, in which the "American" content is even less noticeable. An indigent American widow and her children are living in Paris through the romantic aid of an unknown benefactor. The latter turns out to be a suitor for the eldest daughter of the group. Both plays are routine dramas characteristic of the time, and one has the impression that the "American" element is so much novelty added for effect.

If the "American" degenerated into a rather colorless figure for a few years thereafter, at least the French public had learned

that Americans were not savages and the country was not an exotic wilderness. In the following century, the "American" will be developed as a social being, the product of American culture. The Revolution had served, however, to introduce the real "American" to the French theater, especially through the character and philosophy of Benjamin Franklin, through the patriots exemplified by George Washington, and through the symbolic "American" fêted in the revolutionary festivals of the Reign of Terror.

MORE NOTES ON SHAKESPEARE

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(Read April 20, 1940)

In the play *Hamlet*, when the young prince was called home from college on account of the death of his father, why were the reigning king and queen so opposed to his going back to his studies? He himself had naturally planned to return, but King Claudius says

	For your intent In going back to school in Wittenberg, It is most retrograde to our desire, And we beseech you, bend you to remain Here, in the cheer and comfort of our eye, Our chiefest courtier, cousin, and our son
Queen	Let not thy mother lose her prayers, Hamlet, I pray thee, stay with us go not to Wittenberg
Hamlet	I shall in all my best obey you, Madam

This reply seems immensely to gratify the King, it is as if a great anxiety and burden had been removed from his mind

By insisting on this, the King and Queen brought about their own death. One would have thought the King would have preferred to have Hamlet return to college, for in the midst of the celebrations following the coronation, and the gayety day and night, Hamlet, dressed in black, must have got on their nerves, he was an awkward person in such festivities.

It is strange that Hamlet, after two months, did not know that Horatio was at the court, he supposed he was still at the university. His question,

And what make you from Wittenberg, Horatio?

is answered with a little embarrassment by his friend

Hamlet's amazement and horror at his mother's second marriage are natural enough, no young man ever thinks of his mother as a woman; she is always "mother". That she can be sensual, fond of flattery and compliments, attracted by males, seems to

the son either comic or degenerate, and the additional fact that in his eyes she was an old woman, probably about 43, makes her susceptibility to love grotesque.

In the scene in the third act, he addresses her as follows

O shame! where is thy blush? Rebellious hell,
If thou canst mutine in a matron's bones,
To flaming youth let virtue be as wax,
And melt in her own fire proclaim no shame
When the compulsive ardour gives the charge,
Since frost itself as actively doth burn,
And reason panders will

The one thing all human beings have in common is egotism, the egotism of youth, which assumes that romance and sentiment cease to exist after thirty, is matched by the egotism of old age, which still dramatises itself.

In this connexion we must not think like the majority of readers, we must not take King Claudius at Hamlet's valuation. To those who see or read the play with an unprejudiced mind it is clear that Claudius is a powerful personality, in the presence of Laertes and his tinpot rebellion the King is fearless, in complete command of the situation. When the young rattlepate who acts before he thinks because he is incapable of thinking, rushes into the room at the head of a mob with his sword drawn, King Claudius ironically remarks,

What is the cause, Laertes,
That thy rebellion looks so giant-like?
Let him go, Gertrude do not fear our person

But there is something in Claudius besides courage and energy and resolution, I am convinced that incredible as it seemed to Hamlet, Claudius as a *man* was more attractive to Queen Gertrude than her former husband. Here are a few remarks from Hamlet descriptive of Claudius as the young prince saw him

That it should come to this!
But two months dead: nay, not so much, not two
So excellent a king, that was, to this,
Hyperion to a satyr, so loving to my mother
That he might not beteem the winds of heaven
Visit her face too roughly Heaven and earth!
Must I remember? why, she would hang on him,
As if increase of appetite had grown
By what it fed on, and yet, within a month,

Let me not think on't. Frailty, thy name is woman!
 A little month, or ere those shoes were old
 With which she follow'd my poor father's body,
 Like Niobe, all tears, why she, even she,—
 O God! a beast, that wants discourse of reason,
 Would have mourn'd longer,—married with mine uncle,
 My father's brother, but no more like my father
 Than I to Hercules within a month,
 Ere yet the salt of most unrighteous tears
 Had left the flushing in her galled eyes,
 She married

There is no doubt that her first husband treated her with respect, that he regarded her as needing his protection and she, obviously of the clinging kind, clung to the strong man who was at once the ruler of the state and the head of her house, but his brother, who appeared so inferior in Hamlet's eyes, had something that attracted Gertrude more than noble and puissant qualities. Her first husband was an ideal citizen, a paragon of morality and patriotism, the second loved the woman as a woman and he loved her so much that he committed murder to get her; for his murder was not motived by ambition but by passionate love.

Again in the scene with his mother after the death of Polonius,

Look here upon this picture, and on this,
 The counterfeit presentment of two brothers
 See, what a grace was seated on this brow,
 Hyperion's curls the front of Jove himself
 An eye like Mars, to threaten and command
 A station like the herald Mercury
 New-lighted on a heaven-kissing hill
 A combination and a form indeed,
 Where every god did seem to set his seal
 To give the world assurance of a man
 This was your husband look you now, what follows
 Here is your husband like a mildew'd ear,
 Blasting his wholesome brother Have you eyes?
 Could you on this fair mountain leave to feed,
 And batten on this moor? Ha! have you eyes?
 You cannot call it love, for at your age
 The hey-day in the blood is tame, it's humble,
 And waits upon the judgment, and what judgment
 Would step from this to this? Sense sure you have,
 Else could you not have motion, but sure, that sense
 Is apoplex'd; for madness would not err,
 Nor sense to ecstasy was ne'er so thrall'd

But it reserv'd some quantity of choice
 To serve in such a difference What devil was 't
 That thus hath cozen'd you at hoodman-blind?
 Eyes without feeling, feeling without sight,
 Ears without hands or eyes, smelling sans all,
 Or but a sickly part of one true sense
 Could not so mope
 O shame! where is thy blush?

The dead King speaks more wisely as a Ghost to Hamlet; it is clear that while he knew he himself was a nobler character than Claudius, he did suspect that Claudius had more wit and more charm

Ay, that incestuous, that adulterate beast,
 With witchcraft of his wit, with traitorous gifts,—
 O wicked wit and gifts, that have the power
 So to seduce!—

Was Shakespeare smiling when he wrote those lines?

It will not do to appraise these two brothers at Hamlet's valuation, the second loved Gertrude like a woman and she felt drawn to him because of it. And even if Hamlet were right about the two men, who can judge of the choice a woman's passion may make? Personally, I think she had a better time with King Claudius. With the former husband, duty, honour, patriotism came first, with the second, *she* came first.

It is amusing that some of the German critics of the 19th century thought Ophelia was a "bad girl" because in her madness she sang obscene songs. We know better. Shakespeare knew much that Doctor Freud knew three centuries later; had these songs been sung by a good girl in a play written in 1935, everyone would have said that Shakespeare had advantageously studied Freud. Incidentally, even if Ophelia had not been broken-hearted by Hamlet's behaviour and by her father's death (for if Juliet could be shattered because that pompous ass her cousin had been killed in fair fight by her lover, one can easily understand Ophelia's horror when her father was killed by the man *she* loved) she might have been driven mad by receiving too much advice. Her father, her brother, the Queen, and Hamlet all tell her specifically what to do—and the affectionate silence of Horatio must have been soothing. And indeed Horatio well represents that admirable person—the loyal, steadfast, tactful, silent *friend*.

The great Russian novelist Turgenev said, "For every age love has its tortures"

Ophelia is a very difficult part to act, for although the actual number of words she speaks is not large, she has to appear not only as the docile and obedient sister swallowing advice from a brainless brother, from a tedious old fool of a father, from a Queen whose thoughts were elsewhere, but as an unwilling stalking-horse to the man she loves, whose apparent madness is a dress rehearsal of her own, and finally as a woman downright insane. When we consider the tremendous demands made on the actress, it is a wonder that any woman has wholly succeeded in such a rôle. Of all the Ophelias I have seen, and I must have seen twenty, Lillian Gish is the best. She has the advantage of being the most beautiful of them all, but she is also a consummate artist. She is respectfully obedient to both Laertes and Polonius, showing precisely the natural difference in her attitude toward each, she is obedient without servility, modest without prudishness, affectionate and passionate yet preserving virginal dignity, and in her only *solo*quy, she laments the loss to the State and to Society of Hamlet, and also her agonising loss of such a lover. Lillian Gish interpreted these various moods and experiences without a false note.

While we are speaking of love and its tortures, we might for a moment consider the famous subject of Shakespeare's first narrative poem, *Venus and Adonis*. I am afraid that most unregenerate middle-aged and elderly men regard Adonis as a fool—how could he be such an ice cold nincompoop to resist such an opportunity? Now I think Adonis was neither ice cold, nor a nincompoop, under different circumstances, he might have been equal to the situation. We must look at Venus not with our eyes, but with his. He was a young athlete as passionately fond of hunting as the typical Englishman, he had planned this particular hunt for the boar, and had set his heart on it, now on the way to the hunt, he is interrupted by a woman of experience; to us she seems irresistible, but to him she seems like a mature woman, possibly a bit overripe, who makes shameless advances. Let us suppose now that an athletic undergraduate is on his way to play in a tennis tournament or in any great athletic contest or in a sport which commands his entire devotion. On his way, filled with high anticipations, he is interrupted by a beautiful woman (alas, 35

years old) who tries to draw him away from his sport by spending the day with her. This is a delicate subject, but all matters dealing with human nature are interesting. I maintain that the attitude of Adonis is natural, had he been fifty instead of twenty, . . . but had he been fifty she would not have interrupted him.

In *King Henry IV*, the magnificent speech of Hotspur to the King, which runs so fluently it seems as if Shakespeare must have written it at a sitting, and which reveals so perfectly the attitude of a proud, courageous fighting man to his sovereign, there are several interesting passages that deserve comment. In the early twentieth century Hotspur might have been Theodore Roosevelt and the courtier who addresses him been what Mr. Roosevelt was fond of calling a mollicoddle. In the days when Mr. Roosevelt was leading the strenuous life, he was frequently described in the manner in which Shakespeare's Prince Hal speaks of Hotspur.

I am not yet of Percy's mind, the Hotspur of the North, he that kills me some six or seven dozen of Scots at a breakfast, washes his hands, and says to his wife, "Fie upon this quiet life! I want work!" "O my sweet Harry," says she, "how many hast thou killed today?" "Give my roan horse a drench," says he, and answers, "Some fourteen," an hour after, "a trifle, a trifle."

In this speech of Hotspur's to the King, he describes with ironical scorn the affected dandy who addressed him on the field immediately after the battle.

When I was dry with rage and extreme toil,
Breathless and faint, leaning upon my sword,
Came there a certain lord, neat, and trimly dressed,
Fresh as a bridegroom, and his chin, new reap'd,
Show'd like a stubble-land at harvest-home

There are two interesting points in these last three lines, dealing with the fashions of the day. This dandy cultivated as the extreme of fashion what would now be regarded as unpardonable. That is, he let his beard look like stubble; we should say that he had not had a shave in three or four days. Well, the authentic portrait of Shakespeare, prefixed to the First Folio, which his friend Ben Jonson said was a perfect likeness, reveals Shakespeare's beard, in the same extreme style as that worn by the dandy; his chin shows like a stubble-land at harvest-home. One more bit of evidence that Shakespeare wished to look like an Elizabethan gentleman.

If one should search Shakespeare's plays for what are examples of twentieth century American slang, a good many cases could be found, many persons I believe have noticed in *King Henry IV* Hotspur's speech (I.iii 194) "If he fall in, good night!" used exactly like American slang, and probably with the accent on the noun. But an instance that so far as I am aware, no one has noticed, is in the same famous speech addressed to the king, where again Hotspur is ridiculing the dandy

for he made me mad

To see him shine so brisk and smell so sweet
And talk so like a waiting-gentlewoman
Of guns, and drums, and wounds,—God save the mark!
And *telling me* the sovereign'st thing on earth
Was parmaceti for an inward bruise

There was a learned book written more than fifty years ago, *Shakespearean Grammar*, which tried to prove that the grammar used in Shakespeare's plays was correct. But the entire theory breaks down when we remember that the Authorised or King James version of the English Bible appeared in 1611, when Shakespeare was alive, and judged even by the strictest standards of today, the whole Bible contains only one grammatical error—WHOM DO MEN SAY THAT I AM? Whereas Shakespeare is full of expressions like "Between who" and "I cannot go no further" and "She is taller than me." The only way we can understand this difference is that the English Bible was a literary masterpiece, written by scholars, whereas Shakespeare, a man of the theatre, used language more colloquial.

The invincible egotism born in every human being—perhaps more in males than in females—and which appears often in unexpected moments, is revealed in the aged King Lear when, with Cordelia dead in his arms, he speaks those words of ineffable tenderness

I might have sav'd her now, she's gone for ever!
Cordelia, Cordelia! stay a little. Ha!
What is't thou sayst? Her voice was ever soft,
Gentle and low, an excellent thing in woman.
I kill'd the slave that was a hanging thee

Officer 'Tis true, my lord, he did

Lear Did I not, fellow?
I have seen the day, with my good biting falchion
I would have made them skip

Even at that moment, it is the old man boasting of his past prowess.

CHARLES THE WRESTLER

Whenever one compares a character or incident in Shakespeare with the original sources, one almost invariably observes that the poet, in fusing his 'live soul and that inert stuff', has consciously or unconsciously betrayed some touch of fine feeling, some human tenderness, which transfers the whole situation to a higher plane. This is clearly the case when we place the duke's champion of *As You Like It* alongside the uncouth Norman of Thomas Lodge's romance. Shakespeare has been in America as everywhere else, a tremendously *civilising* force. I cannot remember a single instance of false sentiment in his works, and even his minor characters reveal the innate nobility, purity, and gentleness of the world's supreme dramatist.

Charles has never received his due—either from textual critics or from the audiences which for three centuries have applauded his defeat. Naturally Orlando misunderstood him, and threw him with moral as well as physical zest. On the stage Charles is represented as a loud-mouthed braggart and bully, the ridiculous ease with which he is 'knocked out' makes even the skilful laugh.

Shakespeare, in altering many details in Lodge's *Rosalynde*, really made Charles not only human, but decidedly attractive. Charles is a professional athlete; like most men of his class, he is a good fellow, and is so presented in the play, if we read it attentively, without preconceived opinions. Shakespeare has given us information withheld from Orlando, Rosalind, and Celia. Charles is liberal and kindly in disposition, and means to fight fairly for his reputation. He answers in the most delightful fashion the queries of Oliver concerning the banished Duke, the forest of Arden, and the Lady Rosalind; but this is not the object of his visit. Departing entirely from the original, Shakespeare makes Charles wait on Oliver for the express purpose of saying that he has heard that Orlando is to wrestle against him, disguised; and as it does not occur to his honest, affectionate nature that the boy can be hated by his own brother, he asks if something cannot be done to prevent Orlando's injury and humiliation. Charles speaks in a manner both modest and masculine; his motive in seeking the interview is wholly admirable. It is only after Oliver has told him a series of lies about Orlando, that Charles's attitude to

the latter changes, and accordingly he speaks roughly to him just before the combat. The professional quite rightly regards himself not as a competitor with an amateur gentleman in an athletic contest, but rather as a policeman whose duty it is to destroy a dangerous criminal. I sincerely hope that Charles was only slightly injured, and that he subsequently learned the facts of the case. At all events, I maintain that he is a 'good' character.

In the *Notes on Shakespeare* which I had the honour of reading to the Philosophical Society in April 1939, I discussed the famous account of the death of Falstaff given by Mrs Quickly and in particular the phrase, "his nose was as sharp as a pen, and a table of green fields" For the last two hundred years editions of Shakespeare have followed Theobald's emendation "and a' babbled of green fields" Locke Richardson suggested Falstaff was trying to repeat the 23d Psalm, the green fields meaning green pastures. Then I suggested that if this is true, we should keep the word "table" which word also occurs in the 23d Psalm. Showing this statement to my friend Charles D. Stewart of Wisconsin, author of the excellent book *Some Textual Difficulties in Shakespeare* (Yale University Press), he comments with a new suggestion so good that I incorporate it here

"I am quite sure that Theobald is wrong, and that the line should read, *and a' tabled of green fields*" This has the virtue of sticking more closely to the original

To table, meaning to write in a table or tablet, was a verb in common usage in Shakespeare's day. He says in *Cymbeline*, (I iv vi) "Though the catalogue of his virtues had been tabled" The New English Dictionary says that "tabled" was first used between the years 1450 and 1550, that Shakespeare used it in 1611, and that Sir Thomas Hope used it in 1630. And now as to our taking the words "a Table" and making "a' tabled" of them. All through this passage Mrs Quickly uses the expressions "a made," "a bade," "a parted," "a should," etc. It would be quite in line then to supply an apostrophe and make it "a' tabled" to bring out her evident meaning, just as the editors have done with these other words—a' made, a' bade, a' parted, and a' should. At any rate we see that "a' tabled," meaning *he tabled*, or wrote, was good English and good Shakespeare. It now remains to understand what Mrs Quickly meant by it, taking into account, as we should, the character of the woman, the character of Falstaff, and the general sense of the scene. . .

There can be no doubt that Falstaff in this dying scene is quoting from the Twenty-third Psalm; and her report that he spoke of green fields refers to the "green pastures" of the Psalm. This is a point of humor such as Shakespeare was always making with Mrs Quickly

Now in this Psalm of six short verses, which Falstaff evidently knew by heart, we have the words, "Thou preparest a table before me in the presence of mine enemies", and this is where Mrs Quickly got her idea of tabling. People of her class usually move their lips or pronounce the words as they laboriously read or write, and with Falstaff moving his lips and fumbling with the sheets she got the idea that he was speaking of "tabling" something. Hence her confused and characteristic report of his death.

Moreover, just before this puzzling line we have her remark that "His nose was as sharp as a pen", and the idea of tabling would naturally follow that, such was her muddled and mixed-up conception of things. The whole passage thus holds together in all ways—her character, the words of the Psalm, and the other words in the immediate context. It is a pathetic and uproariously funny death-bed scene, as told to the life by Mrs Quickly.

I have ventured the opinion that Theobald and all modern editions are wrong in this most famous emendation. I think I might go farther and say that they are undoubtedly wrong. "Babbled" is pretty, but in this connection it is not Shakespeare. The thing has got to be ignorant and funny, else it is not Mrs Quickly, and it must be explainable, as all her mistakes are."

And here is a speculation based on a fact. If only there were some good evidence that it were true! Fortunately there is no evidence to the contrary.

Seven years after Shakespeare's death, his wife died, 6 August 1623. Well, in this same year, 1623, appeared the complete edition of his works in one volume, called the First Folio. It was registered at Stationers Hall, 8 November and soon was on sale at one pound the copy. Here is my speculation, it is possible that Shakespeare's wife may have been aware of this forthcoming volume, so interesting to her, and it is possible that she may have seen it either in manuscript or in the proofs. I hope she did. Anyhow, it is interesting that she died the very year of its publication, and it is an exciting speculation that she, in her last illness, may have looked over the proofsheets of her husband's book.

One undoubted fact. It is strange that of all the books ever published in all languages, the two most important single volumes—I mean most important for their literary value—should have appeared in the same period, and each in one volume. The Authorised Version of the English Bible, 1611, and the Complete Plays of William Shakespeare, 1623. Both in one reign and what eternal honour for King James!

A CLASSICAL INDIAN FOLK-TALE AS A REPORTED MODERN EVENT: THE BRAHMAN AND THE MONGOOSE

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(Read April 18, 1940)

ABSTRACT

The classical Hindu story of the Brahman and the mongoose, with details corresponding to those of the *Pañcatantra textus simplicior* and Pāṇabhadrā's *Pañcatantra*, has been recorded in the language of the Kotas of South India as an event which happened to members of the tribe about twenty years ago. Investigation in the tribe and interviews with the persons involved established the conviction that the event really happened. This is then a documented case of independent origin of what we should otherwise, following the theory on which all scientific studies of folk-tales must proceed, regard as a borrowing of a motif. The paper discusses the bearings of the case on the general theory which rejects polygenesis as a principle of wide application.

THE Kotas of the Nilgiris in South India tell a story which, when it was dictated in their language in the year 1937,¹ began "About eighteen years ago, at the village Kurgo ĩ, the man whom we call Kala ypučn took as his wife the woman whom we call Pu čgi n and he begat a son. The name of the boy is To l." The story goes on to tell how, in order to get honey to be given medicinally to the baby, Kala ypučn took with him his classificatory brother Mundn and went off to find a bee-hive in the forest on the upland pastures belonging to the village. They found a hollow tree in which there were bees and Kala ypučn climbed it, leaving Mundn below to catch the honey which he should remove from the nest. Two snakes were in the nest feeding on the honey and in succession Kala ypučn dropped them onto Mundn, whether intentionally or not is not made quite clear. Both men were shaken by the

¹ The Kota story treated in this article was discovered by Dr. D. G. Mandelbaum during his ethnological field-work on the Kotas and was, within a few days of the discovery, recorded by me in the Kota language as a linguistic text. Together we made the investigations here described into the authenticity of the story. I have to express my deep gratitude to Dr. Mandelbaum for his generosity in relinquishing his prior rights to the subject and in lending me his notes. Prof. Franklin Edgerton has made invaluable suggestions during the preparation of the paper, and Prof. W. Norman Brown has generously given me bibliographical data from his files on Indian folk-tales. Finally, I owe a debt of gratitude to the American Philosophical Society which from the Penrose Fund made possible the linguistic field-work during which this story was collected.

experience and went to the upland pasture to find someone to take the honey for them. Another classificatory brother, Čo rn, was brought first, but he, after investigating the hive, denied through greed that there was honey there and went off to the village with the intention of returning with his wife for the honey. The two original actors meanwhile brought Gorčal from the pasture and he removed from the tree a large quantity of honey and larvae, which the three men consumed in part on the spot and shared out in part for use at home. On their return to the village, they met Čo rn and his wife coming with a pot for the honey, and shamed them.

The tree contained also a mongoose's nest inhabited by a mongoose and two young ones. Kala ypučn and Mundn each took one of the young ones to keep at home as pets. Mundn's mongoose soon died, but Kala ypučn's flourished under the loving care of himself and his wife and became expert at snake-killing. Special care was taken by these two people and by all their neighbors to prevent the children of the village from harming the pet. Its good score at snake-killing made the parents of To l easier in their minds when the child had to be left alone in the house and they relaxed the usual village custom of closing the house-door and leaving the baby carefully shut up in the house when both parents had to be absent at the same time.

On one occasion, Kala ypučn and another of his classificatory brothers, Kormu kn, went in the early morning to do some business at the nearby Toda village of To ro r with their Toda economic partner Sirja r. Details are given in the story of the transaction, but they need not be rehearsed here. While Kala ypučn was away, his wife Pu čgi n, the child To l, and the mongoose basked and played in the sun on the veranda of the house for a while. The child fell asleep at about the time when Pu čgi n had to go to the village water-channel for water for the cooking of the mid-day meal. She laid the sleeping child in the usual place, the floor under the broad bench in the main room of the house, and went off with her water-pot, leaving the door open and telling the mongoose to look after the child.

As soon as she had gone, a snake glided into the room, and was attacked and killed by the mongoose, which bit it into pieces, ate some of it, and then went out to the veranda and resumed its basking in the sun. When Pu čgi n returned with her full water-

pot on her head, she saw that the mongoose's mouth was smeared with blood, thought that it had killed the baby and threw down on it the full water-pot, which crushed the mongoose to death. Then she rushed into the house, found the baby playing happily, and saw the pieces of snake lying around. She felt remorse and moreover, fear of the beating that her husband, 'an angry man,' would give her when he returned. She lamented loudly, was asked why by Čo rn, who happened along, but made no answer, and then took up the child and went off to her father's house in the exogamous division of the village called i ke r (all the previously mentioned male characters were members of the a ke r division)

The two men who had gone to the Toda village returned in time for their meal. Kala ypučn found his wife gone and when he asked his neighbor's wife where she was, this woman could tell him only that Pu ěgi n and Čo rn seemed to be talking together a little previously. Kala ypučn then found the dead mongoose and the snake, and being an 'angry man' and the experience with Čo rn in the matter of the honey still ranking, he came to the conclusion that Čo rn had killed the mongoose and he ran off to Čo rn's house and attacked him without giving or asking an explanation. The two men were separated by two of Čo rn's neighbors.

Meanwhile, Kormu kn had gone to his house and as he entered unannounced, he found his wife Do ny and Čo rn's son Čal having intercourse in the kitchen, the usual place for such affairs. This was a case of breaking the taboos. Kormu kn and Čo rn were men of the same exogamous division and of the same generation and so "brothers." Kormu kn hence is Čal's "father" and Kormu kn's wife Do ny is Čal's "mother" and so forbidden to Čal as a sexual partner even in casual affairs. Kormu kn accused Čal in these terms and the two fought and were separated by two men of the neighborhood.

Because of the two fights, a village-meeting was at once summoned and the headman proceeded to investigate and adjudicate everything involved, both the case of incest, and the quarrel between Kala ypučn and Čo rn. Pu ěgi n was summoned to tell her tale of having killed the mongoose, and arising from the Kala ypučn-Čo rn quarrel, the whole tale came out of the snakes being thrown onto Mundn at the time when the honey was being got. For this Kala ypučn was fined six rupees, and being unable

to pay money, he paid with Pu ċgi n's silver ornaments which she gave up without question in the knowledge that she was going to escape a beating. Kala ypuċn was admonished not to beat his wife for having killed the mongoose under pain of a very heavy fine, and Pu ċgi n was told not to act thereafter without reflection.

The story ends with a short statement of how Kala ypuċn died some years later, how his classificatory brother Kaba l took Pu ċgi n as his second wife, the first being childless, and how she bore him a son and was then together with the new-born child turned out of Kaba l's house and sent back to her own house by the first wife, who had Kaba l under her thumb, both of them caring for nothing except that there was now a son to Kaba l's name, and how the boy To l now supports his mother, his younger brother, and himself by doing day-labor for wages.

Prima facie, there are two possible ways of regarding this story, either as the account of an actual event, or as a piece of fiction of the folk-tale type. We shall examine the pros and cons for the two views in the reverse order.

The central motif of this story, the unreflecting killing of a pet mongoose, is well known as a classic motif in Indian story-telling, under the catch-title "The Brahman and the Mongoose." This story occurs in the Pañcatantra in book five. It runs as reconstructed by Prof. F. Edgerton in *The Panchatantra Reconstructed* briefly as follows. There was once a very poor brahman, whose wife bore him a son. On the tenth day after childbirth she went to the river to perform her purificatory ablutions and the brahman stayed behind to look after the child, since they had no servant. During her absence a message came for the brahman from the palace to come and perform a ceremony. He could not afford to miss the fee, and went off, leaving to guard the child a pet mongoose that he had reared like a son. A cobra appeared and approached the child, whereupon the mongoose killed it and tore it to pieces. When the brahman returned, the mongoose ran joyfully to meet him, with its muzzle and paws stained with blood. He thought that it had eaten the child and so killed it with his stick. When he entered the house, he found the child asleep and the dead cobra near it, and he lamented remorsefully. His wife returned, was told what had happened, and admonished her husband against unreflecting action.

The wanderings of this motif from India through the Near East

and Europe as far as Wales, where it appeared as the story of Llewellyn and his faithful hound Gelert, form one of the best authenticated cases of such diffusions of folk-tales. It is possible to trace every step of the wanderings and to localize exactly the changes from mongoose to weasel to dog, from snake to wolf (in the final form of the Welsh tradition), and from brahman to officer or knight or nobleman. This chain with all its links capable of identification has been regarded by students of the tale (notably by Prof. Edgerton) as one of the most cogent arguments for the basic assumption in folk-tale studies that there is no polygenesis in the case of closely similar and well-articulated folk-tales, but only one invention or one occurrence of an event and subsequently diffusion of the tale from the place of origin to all the places where it is later told. Polygenesis is to be sure not impossible, for we cannot rule out the possibility that almost exactly similar events may occur, but it is to be admitted only when stringent proof can be given or when the counts against the probability of diffusion are of the heaviest. In fact, if polygenesis is admitted as a possibility except under such conditions, no study of folk-tales can possess any validity. It follows that, even when links are untraceable, the student of folk-tales is forced by the basic assumption on which all his study rests as a scientific study, to regard all closely similar versions of a tale as having spread by diffusion from one time and place of origin.²

We should then at first sight interpret our Kota story as stemming from the Pañcatantra story, and this was the first reaction of the recorder when he heard the story. Further study of the various versions of the tale within India only served to strengthen this reaction. The Pañcatantra story outlined above is that of the reconstructed original text. However, two old Pañcatantra texts, the *textus simplicior* and Pūrṇabhadra's Pañcatantra, give a significantly different version from that reconstructed. In them, the brahman's wife goes off to fetch water, leaving the child with her husband with the admonition to protect it from the mongoose. The brahman, however, went off to receive alms from his parishioners, leaving the child and the mongoose together. The killing of the snake follows as in the reconstructed text, and then the

² See Prof. Stith Thompson in N. M. Penzer's *The Pentameron of Giambattista Basile*, Vol. 2, p. 298. "It is assumed that every well-articulated tale had a definite beginning, in oral or written form," and in general his statement of the historical-geographical method as a technique in folk-tale studies, *ibid.*, pp. 297-301, also his sketch of the polygenetic theory with some bibliography, *ibid.*, p. 289.

discovery of the mongoose with bloody muzzle, but with the very important difference that it is the brahman woman who makes the discovery and kills the mongoose by throwing onto it from her head her water-pot full of water, exactly as in the Kota story.

These are the two Pañcatantra texts which have contributed most to the modern vernacular Pañcatantra texts of India, and an examination of Johannes Hertel's volume *Das Pañcalantra, seine Geschichte und seine Verbreitung*³ makes it clear that most of these vernacular versions have the motif in the form just outlined, with the brahman woman killing the mongoose

A Tamil literary text, the Alakéśa Kathá⁴ (which is not the Pañcatantra) has included the Pañcatantra story. In it, the brahman woman killed the mongoose with a knife with which she had been cutting herbs in the garden while her husband was absent.

The only oral versions that are available come from Ceylon. Three versions have been reported,⁵ all essentially the same, though that given by Steele is too sketchy to be useful. In them it is a widow (not specifically so, in Steele) who kills the mongoose. In one version she does so with a bundle of firewood, in another with a rice-pestle (in Steele no means is specified).⁶

We are unable to find much in these oral Sinhalese tales or in the Tamil literary source that can be considered close to our Kota version, except the detail that a woman does the killing. The means of the killing is different in these versions from that of the Kota story or of the Pañcatantra versions in which the killer is the woman. If we are to derive the Kota story from any of the other Indian stories, we must go directly to the *textus simplicior*

³ Especially pp 91-290 and 307-38.

⁴ To be found in W. A. Clouston, *A Group of Eastern Romances and Stories*, 211-4, in Mrs Howard Kingscote and Pandit Nāṭha Śāstri, *Tales of the Sun*, 162-4, and elsewhere, all the occurrences are based on Nāṭha Śāstri's translation.

⁵ H. Parker, *Village Folk-tales of Ceylon*, Vol 3, pp 27-8; H. A. Pieris in *The Orientalist*, 1, 218; Thomas Steele, *Kusa Jātakaya*, 250-1.

⁶ We may neglect for our purpose the version from North-west India, given by W. A. Clouston, *Popular Tales and Fictions*, Vol 2, pp 179-81 (from an apparently anonymous volume *Past Days in India*), in which the mongoose is tied up and unable to save the child from the snake, but finally breaks loose and goes to the jungle to get "snake-root," a supposed antidote for snake-bite. Before it could apply it to the child, the mother (merely a woman of poor family) thought that it was the mongoose that had killed the child and she killed the animal by dashing it on the ground. The antidote was then found in its mouth and the child revived. Though it is a woman in this story who kills the mongoose, it is clear enough that there is represented here a different line of tradition from the one we are considering. It is noteworthy however that in this version, as in the geographically widely separated Sinhalese version given by Pieris (see note 5), the child was playing with a pan or tub of water and the snake was attracted to the water by thirst.

or to Pūrṇabhadrā's version of the Pañcatantra or to one of the vernacular versions stemming from these ⁷

Would such a derivation be plausible? Certainly not if we think of the Kotas learning the story from any such source by reading. Sanskrit is an unknown language to all of them, and Tamil is read, and that with difficulty, by only a few. The informant from whom our story was first heard could not be called a literate man and it may safely be said that he was innocent of remaking a Tamil story that he had read. The same may be said of the informant who dictated the story to me, for he told it in the same way as the first informant, and on being questioned denied that any books involved either in his Tamil or his sketchy English education had contained a story similar to the one he dictated. These remarks would apply with even greater force to the Kotas of twenty years ago.

But the possibility of literary transmission is not the only one. The Kotas are accomplished tellers of stories and their social organization provides many opportunities for the exercise of the art. Notable are the wakes after the death of any Kota, during which his relatives are prevented from excessive lamentation by the playing of music and the telling of stories, the practice of maintaining houses in which young unmarried people and young married couples spend the nights, always occupying the early portion of the night with music and story-telling, and the practice of old men or women gathering together the children and telling them the traditional stories. Many of the stories are, as we might expect, narratives of actual events or of events remade in the telling so that they will fit Kota predilections. Many, on the other hand, are quite obviously stories current throughout South India or all over India, or even introduced by Europeans, but these have been completely Kotalized in personnel and material and social framework and fitted out with a wealth of circumstantial detail. Such are e.g. stories of men who were devotees of God and attained supernatural powers, the story of the talking parrot, the story of

⁷ It is unfortunate that few Tamil versions of the Pañcatantra are in print. The only one available to me, that of Dubois (in French translation), follows the type of the reconstructed text. It is even more unfortunate that no collections of folk-tales have been published for Mysore, for the still existing Jain community in this area might be expected to have popularized the form of the story found either in the *textus simplicior* or in Pūrṇabhadrā's Pañcatantra, both of which are Jain by origin, and this might easily have reached the Kotas either directly from the Mysore area or through their neighbors, the Badagas, who are Mysorean by origin and still have some contact and perfect linguistic mutual intelligibility with their country of origin.

the tiger and the demon fooled by a peculiar Dravidian linguistic phenomenon,⁸ the story of the girl with long hair, and, the most extreme case, the story of the Garden of Eden told of the ancestors of the Nilgiri tribes and concluding with a set of Indian motifs in which various animals are cursed.⁹ All these widespread tales have undoubtedly been acquired by the Kotas orally from their neighbors or from chance oral sources, in a fashion that is well-known for India in general, and then remade according to the technique of Kota story-telling, by which stories are generally told of Kotas of ancient times and revamped to fit Kota practices and ideas. The story that we are considering might well belong here, and it undoubtedly shows as recorded nothing out of the way in the process of adaptation. Perhaps the most striking bit of adaptation, if it is such, is the account of the village-gathering and the settling of the affair by the headman. This is a feature common in Kota life and in Kota story-telling as well. Several of the stories that I have recorded end in the same way, including even that of the Garden of Eden, in which God at the end in the fashion of a Kota village-gathering calls before him all the people and animals involved and deals out penalties for all that has been done wrongly. We shall see below that one man at least did know the classical mongoose story in the form which is most widespread in the vernaculars and which our tale most closely resembles.

We have now examined the story both from the point of view of a possible source and method of diffusion and from the point of view of its fitting into the pattern of Kota story-telling and have found that there are no counts on these scores against the diffusion theory for its origin.

But there are counts on the other side as well, for its being the story of an actual event, which are at least as heavy.

In the first place, the informants stated with vigor and certainty that it was a real event, and this is seen in the first sentence of the recorded text where it was stated that at the time when the story was dictated, eighteen years had passed since the event. This of course made all the circumstances something that had to be investigated closely on the spot. The woman Pu čgɪ n and her son To l were interviewed. The latter knew of the event, but only by

⁸ For this story-motif see my article "An Echo-word Motif in Dravidian Folk-tales" (*J A O S*, 53, 553-70) and "Another Example of the Echo-word Motif in Dravidian Folk-tales" (*J A O S*, 59, 503-5).

⁹ These stories will be included in my projected volume of Kota linguistic texts.

hearsay since he could of course not remember of himself an event of his babyhood. The woman vouched for the story in so far as it concerned her directly, i.e. the rearing and killing of the mongoose, the quarrel between her husband and Čo rn, and the village-meeting. She added an incident between the latter two, in that Kala ypučn came after her to her father's house with a stick, threatening to kill her just as she had killed the mongoose. Her father prevented her being beaten there. Her narrative was peculiarly convincing, for all that she really wanted to talk about was the fear that she had had of a beating and her good fortune in escaping it, and the rest was told in a matter-of-fact fashion. Other men of the Kurgo j village were interviewed and in turn vouched for the story.

The village sceptic, an old man, was interviewed along with one of the priests of Kurgo j, they denied any knowledge of the event. The old man did however tell the story in the form in which the *textus simplicior* and Pūrnabhadra give it, making the male actor a "raja" and the baby a girl. In the dénouement the raja's wife killed herself by pulling out her tongue (a feature found elsewhere in Kota stories) and the raja also on his return home killed himself. Whether these two men's denial of knowledge of the event vouched for by other villagers of Kurgo j is to be taken seriously is doubtful, for, as we have said, one is the village sceptic. Moreover, his memory seems not to have been as good as one would wish. In his account of Pu čg n's marital affairs after the death of Kala ypučn he gave vague information which was not substantiated by the woman herself or by our first informant. We may therefore safely neglect his denial of knowledge, attributing it either to his scepticism or to senile loss of memory. The other man's denial is more difficult to explain, he may have been merely showing respect for the other's age. Or, by some accident, neither may have heard of the event at the time, since they belong to a different exogamous division from the principal male actors of the story.

The wealth of circumstantial detail in the recorded story is, as we have seen above, a mark of Kota story-telling in general and is irrelevant as an argument one way or the other. Certain details of the story however suggested themselves as subjects for investigation, to determine whether they were out of harmony with Kota life, but nothing of the sort could be found. The taking of honey,

the economic partnership with Todas, the domestic details (most of which are omitted from the story-outline at the beginning of the paper), the case of incest, the village-council settling all the affairs (as noted above), the keeping of pets, all are natural and usual. All that seems to emerge to the point is the peculiar collocation of motifs in the story, i.e. the mongoose incident, the incest incident, the incident of the snakes in the tree, a combination that seems unmotivated unless we are dealing with the notoriously unmotivated connections of everyday life. And further study of the story in its entirety gives us a set of particularly believable characters—Kala ypučn, Mundn, and Čo rn, who by the working of well-marked psychological traits set the train of events in motion.

It should be said also that, though any borrowed story is told of Kotas, they are Kotas of ancient times, as was said above. No other classical motif is known to me to be attributed to Kotas of the present day.

The conviction was soon fairly well established, and has since grown stronger, that the story is one of actual events. We have here then one of the decidedly rare cases when we are able to produce a case of independent origin of what we should otherwise regard as a folk-tale spread by diffusion, authenticated as well as we can hope for without the recorder being an eye-witness of the event. Further discussion of the theoretical bearings of this case may well be left to the historical-geographical school of students of folk-tales.

We have been assuming that the case is clear-cut, either the tale is a borrowing, or it is the record of events exactly as they happened. Two other possibilities may be mentioned. Was it the case, by chance, that something did happen which was not exactly as it is told now but was remade in the classical form because of knowledge, either antecedently or subsequently gained, of the classical story? Or, was it the case that the events narrated happened without outside interference up to the point where the woman discovered the mongoose covered with blood, and that at that point antecedent knowledge of the classical story subconsciously forced her to act as the brahman woman did in the story? This latter would be a peculiarly subtle way of diffusion producing an event which duplicated the motif diffused. We cannot in the nature of the case either prove or disprove either of

these hypotheses; the informants would present a perfectly impenetrable front in the latter case, for they cannot bring to light their subconscious processes. In the former case likewise, which will probably be the preferred explanation of many folklorists, investigation met a blank wall, for, as was said above, knowledge of the classical story was denied by the chief informants and by the principals of the story, and the lapse of time since the date of the event made it almost impossible to gain any data of the required nature for that period. Again, the students of folk-tales must be left to pass judgment. The former possibility would fit their basic assumptions well, if they are willing to assume also that our informants were guilty of concealment, either deliberate or working as an inhibitory but subconscious process. The latter possibility would probably be as unsettling for the basic assumption of the study of folk-tales as the conclusion that our story is one of an actual event.

SYMPOSIUM ON
CHARACTERISTICS OF AMERICAN CULTURE
AND ITS PLACE IN GENERAL CULTURE

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SYMPOSIUM ON CHARACTERISTICS OF AMERICAN CULTURE AND ITS PLACE IN GENERAL CULTURE

FRIDAY MORNING, APRIL 19, 1940, 10.30 o'clock

FREDERICK P KEPPEL in the Chair

PRESIDENT MORRIS We owe to Mr Keppel, President of the Carnegie Corporation, the suggestion of the very interesting program that is to be given this morning I am now going to ask Mr Keppel to take the Chair and preside over this morning's and this afternoon's sessions He will tell you something of the nature of the discussion and of the very distinguished scholars that we have here to participate Mr Keppel, the Chair is yours

MR FREDERICK P KEPPEL Mr President and members and friends of the American Philosophical Society, Anthony Trollope is credited with having said that if a man starts to write a novel he can carry it on under his own control up to a certain point, but sooner or later the characters themselves take charge, and after that, he becomes little more than a scribe

I have had a somewhat similar experience in this matter which brings us together here As our President has said, in a way I am responsible for this type of meeting I made so bold last year as to ask the President a leading question as to whether it might be practical to have a meeting at which everybody started from scratch Nobody would have any artificial handicap, either from the presence or absence of special qualifications in scholarship or research And I suggested this general topic of our American culture

Well, quite properly I was punished for my suggestion, and I was sentenced to prepare a scenario and also to set up what they call in Hollywood "a casting list"

With the approval of the Program Committee I went ahead, and I confess I felt rather important and rather creative, but that didn't last very long, because just as soon as these characters in my conversation piece were evoked, they took charge I don't know what happened to my scenario I haven't seen it for a long time, and I don't know just what is going to happen today

One or two of the speakers showed me something they then thought they might say if they didn't change their minds in the meantime I don't even know how they are going to tackle the very tricky question of deciding what culture is All that I can hope is that they will not spell it with a "k."

If anybody in this room has any curiosity as to how this kind of meeting is going to turn out as a whole, I can only assure you that my

fellow conspirators and I share that curiosity to the full We don't know Perhaps if things go well, there will be some resemblance to the *entretiens* which have become so interesting a feature of the international gatherings of the various committees on intellectual cooperation, and there is an exciting thought that perhaps some of the scholars in the room can trace some faint resemblance to the proceedings of the Junto which Benjamin Franklin organized in 1727, and whence we sprang That, however, is a long shot

There is only one thing on which we are all agreed, and that is we want to provide the fullest opportunity for general discussion This is a meeting where everybody is to start from scratch In order to provide the minutes and seconds for such discussion, the speakers have agreed to rather rigorous time limits To keep within their time limits, you may from time to time see them glance at a manuscript, but I assure you that nobody is going to "present a paper "

The Chairman will try to do his bit in this saving of time by bringing these introductory observations to a close and proceeding with the business.

The first of our speakers has an official standing He is Associate Editor of *Harper's Magazine*, but he has also come to have an unofficial standing in this country through his books, "Only Yesterday," and "Since Yesterday " And there is something delightful in the fact that it is in this time-honored building and before this venerable Society that at last he reaches "Today."

I present our guest, Mr Frederick Allen

TODAY

FREDERICK LEWIS ALLEN

Associate Editor, *Harper's Magazine*

(Read April 19, 1940, in *Symposium on Characteristics of American Culture and Its Place in General Culture*)

IN the year 1917—twenty-three years ago—a distinguished American literary critic, the late William Crary Brownell, brought out a little book entitled “Standards,” in which he examined the condition of American culture and found it depressing. Mr. Brownell was disturbed to find the field of art and letters becoming, as he put it, “less and less a sheltered enclosure and more and more open to the winds of the world.” Noting the wide popularization of education and of what passed for art and letters of a sort in the slick-paper magazines, the Sunday newspapers, the sugary best-selling novels, and the upstart movies, he quoted the dictum of Manet that “art always loses in height what it gains in breadth.” He saw a great, half-tutored, indiscriminating mob invading the cultural domain where fastidious, aristocratic taste had once ruled, and he realized with dismay that this mob would try to make it—as he said—“an absolutely unenclosed domain—the common of civilization, so to say, whose weedy aspects and worn places and rubbish heaps are as legitimate details as its cultivated area. Ought not,” asked Mr. Brownell, “ought not access to this territory to be made more difficult, as difficult as possible?”

I quote Mr. Brownell not because he was a critic representative of his times—for even in 1917 he was a classical-minded conservative, a backward looker; I quote him because he described so well what *has not happened*. The twenty-three years since 1917 have played a good joke at the expense of his thesis. Of course it may be said that some parts of the gardens of American art and letters have been trampled into bare patches. But the gardens have also been vastly enlarged; and what Mr. Brownell called “the common of civilization,” with its “weedy aspects and worn places and rubbish heaps,” is now springing into green at a score of places.

To leave metaphor behind, I believe that we Americans are now a distinctly more mature people, a more culturally enlightened people, than we were a generation ago; that we are on the whole better off, rather than worse off, for the participation of the millions in cultural things that were once considered chiefly the affair of the few, and that we are now witnessing, if we will but realize it, a flowering—or at least a budding—of an American culture of which we may be proud. It may even be fair to say that we have here the basis for an “American renaissance”—if we refuse to be misled by the connotation of the first two letters of that word. They suggest to most of us, I think, a repetition of something that has gone before. We shall better understand what is happening in this country if we rid ourselves of any expectation of seeing duplicated here what happened in the Athens of Pericles or the Florence of the Medici or, for that matter, in nineteenth-century England or France. For the essence of this American flowering is that it is new, that it takes unprecedented forms, and that it is manifold.

I realize that anybody who speaks in such terms as these may seem to be—in the expressive phrase of the day—sticking his neck out. Certainly when we look at the Europe of today it seems queer to be speaking of a renaissance. It may seem queer even if we are speaking simply of our own country, wrestling as it is with the overwhelming problems of an obstinate depression and overhung as it is by the clouds of war. Nor do I deny that as one examines the American culture of 1940 one finds plenty of evidences of undisciplined or corrupt taste. Listen to some of our radio programs—indeed to most of them, read the concentrated pap which passes for fiction in many of our magazines for the millions; sit through some of the Class B pictures at the movies, or look at the shoddy contractor-built suburban developments and devastated regions which lie at the edges of our American cities; and you may well wonder what in heaven’s name I am talking about. Even if you examine the American performances in some of the traditional arts during the nineteen-thirties—in poetry, for example—you may question my generalization. I am well aware, furthermore, that when an editor of one of the last remaining magazines of what used to call itself proudly the “Quality Group,” a cultivator of one of the last remaining flowers in what used to be considered the garden of

distinguished monthly journalism in America, talks of an American cultural blossoming, he is sticking out his neck particularly far.

Nevertheless I stand by my guns I think this country is making cultural progress in a new and exciting way, and, I may add, in a way which Benjamin Franklin would have appreciated and welcomed.

Now before I begin to haul out my evidence and lay it before you I am afraid I should define my terms In the first place, I am *not* using the word "culture" in the anthropological sense, meaning simply a people's way of living No, I use it in its more usual non-anthropological sense But even in this sense it is an inclusive word. To most people, perhaps, it connotes refinement and familiarity with, and appreciation of, choice and tested things But in a somewhat different shade of meaning it may connote an eye and ear for beauty, a sense of order and graciousness, whether cultivated or instinctive, and whether accompanied by wide learning or not. Under the shelter of the word "culture" there must also, I think, be room for a more dynamic ingredient added by the person who can produce fine things. A Shakespeare may not possess refinement and wide learning, but he enriches the soil of our civilization, the periods which we think of as the great flowerings of culture were periods not merely of appreciation but of production preeminently; indeed, any culture would be sterile which was not animated by the devouring curiosity of the discoverer and experimenter, the fierce energy of the creator

I emphasize these latter shades of meaning because it is in these latter respects that American culture seems to me to be showing special progress What is happening is that innumerable Americans are becoming more sensitive to beauty and order, and that our creative energy is stirring

First of all, I should like to call to your attention the enormous expansion of cultural opportunity which is taking place in this country. In no other cultural flowering in history has more than a small fraction of the population been involved. The picture here and now is amazingly different. In the United States of 1917 Mr. Brownell complained that the domain of arts and letters was becoming unenclosed; he would be amazed to see how the fences have come down even since then.

First, look at the musical scene. By means of the radio, vast numbers of Americans now hear great music and enjoy it increasingly. It is estimated at the NBC, for example, that Toscanini's weekly symphony concert has an approximate audience of four and a half million people, and that the Metropolitan Opera broadcasts reach some ten million people. It would take over a hundred Yale Bowls to seat that astronomical audience of listeners to the opera. Walter Damrosch's NBC music appreciation hour, to pick only one example from among many, is heard each week by several million school children, to say nothing of a million or so adults. Have any such opportunities for the mass of the population to hear good music finely played ever existed—and been taken advantage of—before in the world? And it has all come about since Mr. Brownell wrote. (Incidentally, speaking of radio audiences, those of you who enjoy "Information, Please" may be interested to know that, according to recent estimates, that program is heard by twelve million people.)

It is true that the piano is no longer the standard household ornament that it once was, and Mr. David Cohn reminds us that Sears Roebuck's sales of fiddles and guitars have dwindled in the past generation; but can we measure the amount of participation in the making of music in America without noting also that there are now some thirty or thirty-five thousand school orchestras in this country, and without noting, on a somewhat higher level, how many of our school and college glee clubs have become choruses singing fine music which no glee club would have thought of singing in Mr. Brownell's day?

Until now our American composers have been handicapped by living in an environment uncongenial to creation, and this is still true in some degree. Most of our important conductors and impresarios are even now foreigners who, whatever their transcendent merits, are not likely to be able to judge new American music except as something alien to their natures. Yet the audience is being prepared, the ground is being cultivated for a native expression in music.

Next, consider reading. It is true that current book sales on the whole have shown little increase during the past generation. But there can be small doubt in the mind of one who compares the best-seller lists of today with those of 1900 or 1917 that the books which sell very widely now represent, on the average, a

considerably higher level of quality than they used to, thanks partly to the intelligent selections of the Book-of-the-Month Club. We must remember, too, that the book as a form of entertainment and instruction occupies a different position now from what it did a generation ago. Not only does it compete, as entertainment, with the radio and the movies, but the book-reading public is now underpinned—and presumably reduced—by a magazine-reading public such as did not exist then here and exists now nowhere else.

If the magazine which I represent seems to be one of the last flowers to remain in bloom in a fine old plot, the death of the other flowers in that plot is not necessarily to be attributed to a deterioration in the public taste, one important reason may be that the flowers in the neighboring plots have improved in quality. It is true, I believe, that *Harper's* contains a kind of thorough and untrammelled discussion of contemporary problems which is rare in the United States in 1940 and is greatly needed. But it is also true that many of the good things which magazines like *Harper's* and *Scribner's* and the *Century* and the *Atlantic* used to bring us fifty years ago are now being brought, too, by other periodicals—not only by the *New Yorker*, for example, which during the past fifteen years has set a new standard in American humor, but also by magazines of huge popular circulation. Fifty years ago there was not a single magazine in the United States with a circulation of a million. Now there are twenty-six of them. Of these, thirteen have circulations of over two million, and five—*The Saturday Evening Post*, *Woman's Home Companion*, *Ladies' Home Journal*, *Collier's* and *McCall's*—have circulations of over two and a half million. Some of the popular periodicals are full of literary marshmallows and shy at the expression of an idea which might possibly offend a perceptible number of readers or advertisers, yet I think it is safe to say that if we take these magazines as a group and remember the scores of millions who consume them, and think how many good things are to be found among them, they offer an impressive exhibit of mass culture.

When we turn to the fine arts, we note that the popular magazines are now producing articles about them, with reproductions in color, to an unprecedented extent, and that during the past year or two there has been a well-rewarded rush to bring out

books of masterpieces of art, old and new And if there has been a dismal downturn in the collecting of painting by private individuals since 1929, there has also been a large increase in the sale of good color reproductions I visited recently a college where some 150 reproductions of fine paintings, from Giotto to our contemporary Americans, were in steady demand for rental by undergraduates. I shall leave to Mr. Taylor the change which has been taking place in the function of our art museums, but no one who is aware of the number of schools and colleges in which boys and girls are painting and modeling and the huge attendance at traveling art exhibits, such as the Van Gogh show, will be disposed to deny that the American public which is actually excited about art is growing fast The recent Picasso exhibit at the Museum of Modern Art in New York was attended by 99,503 people during its fifty-one days; and if you don't like Picasso, let me add that the Italian Masters, at the same Museum, were seen in seventy-three days by 277,794 people an astonishing record

Nor should we leave this question of the breadth of the base of our culture without discussing the growth in educational opportunity That many of our universities have elephantiasis—and also footballitis—I should be the first to agree. The inspiring fact that millions of Americans have wanted a higher education for their children has put a heavier load on the educational machinery than it could carry without creaking here and there But again, consider the other side of the picture The opportunities are there; and thanks to such Foundations as that which Dr. Keppel heads, there are so many agencies now looking for young talent and ready to smooth the way for it that I have heard it said that in the United States of 1940 there need be no mute inglorious Miltons If there is in Pennsylvania or Georgia or South Dakota a boy of genius, he will be found out and aided to bring his gifts to fruition. That statement may be exaggerated—but that it could be seriously made is something excitingly new And do not forget what the WPA has done in recent years; do not forget the men who had not sold a picture for years—and then were given post-office murals to paint; the half-starved musicians who found themselves playing to big audiences in WPA orchestras, the companies of WPA actors who gave life to the languishing theatre Call this boondoggling if

you will, admit that most of the talents thus aided were negligible; but ask yourself if it does not represent a new conception of the responsibility of the general public to see that potential artists shall have a chance to be artists, no matter what their circumstances. Yes, the democratic base of our culture has been widened.

In the second place, I should like to remind you how many new arts have sprung up beside the seven arts of tradition. Let us forget for a moment the traditional assumption that one measures the state of a culture chiefly in terms of such familiar vehicles as books, plays, paintings, sculpture, architecture, and music. Let us assume that other vehicles may offer a means of expressing the impulse to create and enjoy beauty, and let us look about us.

New arts? One thinks immediately of the movies, which after a long period of high technical competence and singular evasion of reality are now showing signs of growing up. One thinks perhaps of that awkward and often ridiculous fledgling, the radio drama. One thinks with somewhat more assurance of that remarkable subdivision of the movies, the animated cartoon drama, realizing that in Disney we have an artist using a medium which hardly existed twenty years ago. One thinks of the remarkable increase in interest in photography—of the hundreds of thousands of people who are taking pictures in the true spirit of the amateur of the arts. But let us look farther.

Drive over the magnificent parkways being built in the outskirts of some of our big cities—especially about New York, thanks to the energy and vision of Robert Moses—and see how the highway engineer and the landscape architect have joined hands to create majestic avenues in peculiarly twentieth-century style. Look at some of our new bridges and dams—are they not works of art as well as of utility? Is there any of us who does not see, let us say, the George Washington Bridge without a lift of the heart at its extraordinary beauty, especially at night when the great sweep of its cables is picked out with lights? And if you will permit a New Yorker to crow a bit more among Philadelphians, may he suggest that the incredible effects achieved in lighting the New York World's Fair demonstrate effectively the possibilities of another virtually new art—that of lighting with color?

Think of the numerous uses to which the industrial designer has brought the art of functional design. No automobile manufacturer decides upon his new model nowadays without the most anxious consideration of the way in which millions of possible purchasers will react to the grace and sweep of its lines. Look at that functional masterpiece, the present day airplane. I wonder if ever until the past decade a designer has been called in to plan a railroad train as a unit, as some of the new silver streamliners were planned. Compare the best of the new railroad coaches and dining cars with their equivalents of the vintage of 1917. Notice the way in which the packaging of goods has been revolutionized: compare any well-remembered cereal package of 1917—let us say the old Shredded Wheat box, with its picture of the factory with all flags flying—with the packages of today. Go into a modern kitchen and see how much of the equipment there has been carefully designed with due regard for the functional principle and the gay use of color. Look at some of Frank Lloyd Wright's or Albert Kahn's factories; why, even factories and the machinery inside them have now been brought inside the enclosure of the arts, as if they too were intended to be seen! Little by little we are re-learning what we had forgotten during the latter nineteenth century. that useful things can and should be not ugly but beautiful. Look, for that matter, at the best of Woolworth's glassware: I can give you no better example than that of the cultivating of the common of our civilization.

Go up to the attic and pull out a pile of the magazines of 1900—or even of 1917—and compare them with their equivalents of today; in type and format the advance has been remarkable. The improvement has extended to books and to every sort of use of type, even to the designing of letterheads; if you occasionally receive, as I do, a letter from a railroad office which has not changed its letterhead within the memory of the oldest employee, you will wonder who could ever have hit upon such an absurd combination of discordant types.

Do we not see, too, the beginnings of an art essentially new to America in the groping efforts here and there toward town planning and regional planning, toward the designing of our communities in the large? The sort of overall design represented in, say, Rockefeller Center, and in a masterly way at

Jones Beach on Long Island, and our beginning attempts to lay out Radburns and greenbelt villages, may be the early steps toward the development of new techniques for harmonizing and rationalizing the work of architects, landscape architects, engineers, and what we may call social engineers. And the result may be nonetheless culturally valuable for being collectively rather than individually created.

There is an almost perverse element in this flowering of new arts: it almost seems as if we made the most striking progress in those areas where there are no academic standards and traditions to limit us. I live in New York near an avenue of department stores whose windows provide an ever-changing spectacle of bold patterns in color and light and ingenious, imaginative compositions, and I often notice, as I stroll up this avenue at night, how many of the other strollers are manifestly not so much window-shopping as enjoying the show—as one might enjoy a visit to a gay museum. Then I wonder whether the designers may not be doing better work for the absence of any cultural expositors to talk to them about classical tradition and make them self-conscious and imitative. If there is a half-truth in this, let us take good cheer from it, for it is a sign that there is a very widespread popular instinct for good design today, for the thing that looks well. It never occurs to most of the people who exercise this instinct that they are rendering art judgments. They may think they are outside the sacred enclosure of the arts—but they are inside it all the time. For the fences have been moved.

Very rapidly we Americans are getting away from the Colonial attitude. Already it is a long time since we talked of the "great American novel" in tones which suggested that it would burst upon our immature culture suddenly, as a child expects that his twenty-first birthday will find him abruptly a new being. It is a long time since we took it for granted that American novels should be respectable imitations of the best English works. Now we know we have our own tradition: in a literary sense, we are grown up. And we are beginning, too, to be far less subservient in other arts. If we still make pseudo-Venetian furniture in Grand Rapids, still design bank buildings to look like Parthenons, we are apparently approaching the end of this

phase Our new streamlined trains are not Byzantine, or Louis XV, or Dutch Colonial

I do not say that this national cultural independence is wholly good The classicist will hasten to remind us that there is little to be gained by throwing away the past—and of course he is right The political scientist may add that autarchic nationalism is the curse of the twentieth century, and ask us if it is not even worse to close the cultural trade-routes than to close the economic trade-routes—and he, too, is right We want no tariff walls against the best products of foreign civilizations Our American culture must not try to walk alone, without benefit of the past or of the contributions of its neighbors. Yet what is to grow in our soil must be what is adapted to that soil We may compare, we may learn, but I am glad we are coming to build for ourselves. For that is the only way in which anyone can build greatly

One closing word if I have said little about the peaks of our cultural landscape, if I have dodged the question as to whether our finest products in arts and letters are better today than they used to be, or better than they are elsewhere, this, I must confess, is because I would prefer to dodge a question which would let this company in for endless and possibly heated discussion. One may be conscious, as one drives across country, that one is climbing on to rising ground, and yet lack the surveyor's instruments to judge the precise altitude of the surrounding summits But if I have avoided that sort of estimate, it is also partly because I wish to focus your interest upon the groundswell of the land all about us Whether or not the very finest things that we produce are better than they used to be, at least the conditions are being made more congenial for the production of fine things in the future. If Mr. Brownell were here today, and were to lament our supposed lack of authentic contemporary American masterpieces, I should be tempted to quote to him those familiar lines of Arthur Hugh Clough's.

"In front the sun climbs slow, how slowly!
But westward, look, the land is bright!"

MR. KEPPEL. If what Mr Allen has said to us has stirred up in you any questions, just keep them in mind—tie a figurative string around your finger—because we will all have a chance to speak soon

I once heard the next speaker describe himself as a grave-robber. On this occasion it would be much more fitting to present him in other terms, and I shall do so as an archaeologist-anthropologist, and the Chairman of the Division of Historical Research of the Carnegie Institution of Washington. He will look backwards—as it happens, a good long way backwards. I present our fellow member, Mr A V Kidder.

MR. ALFRED V KIDDER. Mr Allen very accurately defined the anthropologist's conception of culture as the way the people live, although if he had attended their anthropological meetings or read of the hair-pullings that arise in an effort to get a proper definition, he would realize that there are many discrepancies. However, that is the anthropologist's definition of culture, and the archaeologist, who is the mouldier variety of anthropologist, deals with the culture of the past, and, of course, in general, with the material culture of the past, because that is all that is left to him.

LOOKING BACKWARD

ALFRED V KIDDER

Chairman, Division of Historical Research, Carnegie Institution of Washington

(Read April 19, 1940, in Symposium on Characteristics of American Culture and Its Place in General Culture)

THE archaeologist is the only grave robber whose activities are contemplated by society without abhorrence, the reason, of course, being that the men and women whose tombs he violates have ceased to be persons and have faded, nameless and unremembered, into the mists of the past.

To this loss of individuality, this merging of the great with the humble, the good with the bad, this universal levelling and averaging, is due the major distinction between the study of archaeology and that of recorded history. Although it has its drawbacks in obscuring the enormous influence that single men of outstanding ability must always have exercised in directing the march of events and in the development of human culture, it permits a breadth of outlook, a smoothing, so to speak, of the curves of history, that should give unique opportunity to perceive the major trends of man's career.

I say "should" advisedly, for just as the historian or the student of art or literature risks over-preoccupation with the

lives and achievements of persons, so can the archaeologist all too easily immerse himself in the details of his profession: with the puzzles of stratigraphy, the decipherment of glyphs, the fascinating problems encountered in every excavation.

So it is good for an archaeologist to be forced to take stock, to survey his field, to attempt to show what bearing his delvings into the past may have upon our judgment of present day life; and what service, if any, he renders the community beyond filling the cases of museums and supplying material for the roto-gravure sections of the Sunday papers. And this might seem a particularly difficult task for one who, like myself, is concerned with the prehistory of the American Indians, a vanishing race which played no part in laying the foundations of present-day world culture.

But, as I hope to show, knowledge of the history of these first occupants of the New World can aid in throwing light upon fundamental and vitally important problems.

Perhaps the most striking phenomenon of man's career has been the strange series of ups and downs which have marked his slow climb toward the heights. From the dawn of the Stone Age to the present day, we have been striving for a more comfortable existence, for better relationships with our fellows, and for artistic and spiritual improvement. And progress has been ever upward. But that progress has been the gain of the race as a whole rather than of any single group. For while many peoples have played outstanding rôles in the long drama, no one of them has held more than brief preeminence. A succession of brilliant nations have contributed their bit to the total, but every one of them has ultimately fallen.

If one plots graphically the course of human development and maps the migration of leadership from one part of the world to another, it is seen that while there has been a comforting general rise in the line of civilization, its mounting course has been interrupted by drops proportionate in violence to the height of each preceding peak; and the map shows that once a people has reached a position at the top of the heap it invariably has fallen, and leadership has been taken over by dwellers in some other land. All of which would seem to mean that our present order is due for a terrific smash and that the next advance will be made by races other than those of Western Europe and the United States.

Of course it is pleasant to feel that there will be another rise and that civilization itself is not necessarily doomed, and perhaps the people who are going to be the next overlords will run the world more intelligently than we do. Nevertheless, it is disquieting to consider even the temporary break-up of our culture or the passing of our race. What can we do about it? How can we smooth the curve, how eliminate the perhaps not inevitable drop, how keep ourselves in the cultural running?

What has happened in the past? It is of course the business of the archaeologists and the historians to find out. But they have not done so. At least not convincingly. And we do not yet know why former civilizations have withered, nor do we know why their seeds, finding lodgement in new racial soil, have almost always produced stronger cultural offspring. A thousand explanations have been offered. The geneticist attributes slumps to bad genes and recoveries to happy combinations of good ones, the nutritionist sees things in terms of vitamins, the medical man in terms of diseases, the sociologist perceives faults or virtues in this or that aspect of social organization. The theologian blames heresies. And if all else fail, we can always appeal to climatic change or economic determinism.

But, as we hope to show, the world's worst troubles have in the past been, are now, and will continue to be, due to a single underlying cause, and that is to man's inability to cope at certain critical periods with the cultural machine he has built. That is the fundamental difficulty, the hidden disease of which depressions, class clashes, wars, are the outward and visible symptoms. To explain.

Few people realize just what has happened to us humans during the million or so years of our career as *Homo sapiens*. Throughout those uncounted millenia we have been surrendering our inherited abilities, one by one, to that something, outside of ourselves, which we call culture, and which, in its more highly developed form, is known as civilization.

Every animal, save only man, comes into the world endowed with all, or very nearly all, the knowledge required to carry on its individual existence. An oriole does not need to be taught to weave its nest; a deer does not learn from its parents how to circle and watch its back-track to see if an enemy is in pursuit. Such knowledges and skills are inherited. We, on the other

hand, are born intellectually naked. Nothing is transmitted to us from former generations beyond our physical make-up and a receptive but empty mind. We have become this way by sloughing off, through the ages, one innate attribute after another. Natural hardihood we have exchanged for fire and houses and clothing; brute strength for weapons. We have at last, and this is an appalling thought, even abandoned most of our instincts, save the urge to exercise such purely organic functions as feeding and reproducing ourselves. We have entrusted everything to our culture, a pooled sum of common experience, which is now completely non-biological, outside of ourselves, which is not inherited, and which can only be passed from one individual to another by the teaching-learning process. Everything must be learned. Take away our culture, place, for example, a hundred male and female children, utterly untaught, and reared only just to the point of being able to feed themselves, upon an island well supplied with natural foods. They would probably continue to live. They might, though it is doubtful, bring children into the world and raise them. But they would have no speech, no knowledge of fire, no conception of the use of any tool. Would their descendants, in the unlikely event of their survival, recreate a culture; and if so, would it take the countless mullenia that were required for man to work through the first steps of his ascent from a purely animal life? These questions may seem academic, but they bear on the very fundamentals of man's present nature, and of that civilization to which, as has been said, we have entrusted so much. Civilization, being the creation of exceptional brains, and always being carried forward by a relatively small number of unusual individuals, is never really common human property. And constantly being added to, it tends to outgrow the ability of the average man, or indeed the capacity of any single man, to comprehend in its entirety or to make use of it in the most effective manner. Culture, too, is unrestrained by the slow-working laws of biological evolution. Thus it moves onward at such a rate that the human brain cannot develop fast enough to keep pace with it. Furthermore, and this is the really serious aspect of the matter, it expands unevenly and its material side always tends to outstrip its spiritual and ethical attributes. Hence culture can, and history teaches us that it repeatedly has, become so over-

whelmingly complex and so materialistically overweighted as to bring confusion and ruin to those very peoples who have been most active in its creation. So, although our culture, our civilization, is our greatest triumph, it is also our most dangerous possession. Of this truth, it is perhaps unnecessary to state, the most striking example is offered by the perilous stresses now being brought about by recent advances in the physical and biological sciences.

On the physical side, we all know what the development of labor saving machinery has caused in the way of social and economic dislocations. The biological sciences together with biochemistry are permitting medicine vastly to reduce infant mortality and thus allowing to grow to maturity, and to reproduce themselves, countless thousands of weaklings who would not otherwise have survived, to the certain deterioration of the race. Upon what modern science has done for warfare it is, at the present moment, quite unnecessary to enlarge.

I said, a moment ago, that while our civilization is our greatest triumph, it is also our most dangerous possession. It is dangerous, to repeat, because it has always tended to become overweighted, specialized, on the material side. Particularly has this been the case during the past two centuries. And over-specialization inevitably carries the seeds of extinction. Consider the animal kingdom—take, for example, the dinosaurs. They specialized to adapt themselves to certain definite environments—their frames were moulded to perform certain special functions. Each species of dinosaur became so perfectly fitted in size, or proportions, or the shape of its teeth to do one thing and one thing only, that when conditions changed, even very slightly, it was physically unable to meet those changes and passed away.

So it has ever been. That is why the largest and the most strangely shaped beasts, the sabre-toothed tigers, and the mastodons, have followed the dinosaurs; why the dodo and the great auk and the buffalo perished; why the giraffe, and other oversized or over-specialized creatures of today are going. While the little, adaptable animals, the squirrels, foxes, rats and mice can face even the competition of man.

And, with us, any sudden change, any catastrophe, falls heaviest upon those who have committed themselves most

thoroughly to particular ways of life. The man who could only shoe horses was without a job on the coming of the motor car. Think what happened at the time of the hurricane of September, 1938, to those of us in New England who lived in the most specialized of modern houses. The electric current failed and we had no heat, no light, no water, no ice. But the farmer continued happily to sit by his stove, and read by his lamp, and pump from his well. New York, supreme example of urban specialization, could not exist a week without coal and gasoline.

Thus it is, in a larger sense, with the civilization of which we are so proud. It has specialized so strongly on the mechanical side that the monkey wrenches of war, or pestilence, or crop failure can bring its delicate mechanism to crashing ruin.

We cannot hope for success in driving this terrific cultural machine we have built, until we know much more about it. Not only must we know what it is today, but what it has been in the past. What is its history? What is the relationship between man and his culture? Given proper opportunity, will all men take certain cultural steps? Are some races more capable than others of building culture? Are others unable to cope with it? And, most important of all, is culture—which has no physical existence, because it is carried solely in the minds of men—is culture subject to laws of development comparable to those which we know control the material world?

These questions bring us at last back to the American Indians, and to the bearing of their history upon the problems under discussion.

Man first came into the New World many thousand years ago. The earliest Indians, and apparently all other groups which later drifted across from Asia, arrived as nomadic hunters. And as savages, or little better, they spread themselves from the Atlantic to the Pacific, from Alaska to Patagonia. For a very long time no significant progress was made, but eventually there came the discovery of agriculture; and with it increase of population and rapid development of higher culture.

This culture, based on maize, got its start well before the time of Christ, somewhere in Middle America or in Western South America, and from there diffused itself over a large part of the New World. A great outward push apparently took place during the first millenium of our era. And toward or about the

year 1,000 there was reached a general peak of achievement and a maximum geographical expansion.

Then came a recession. It was not everywhere equally severe, nor were its manifestations simultaneous. But there can be little doubt of its reality. The cities of the Maya Old Empire were abandoned. The temples and pyramids of Teotihuacan in Central Mexico fell into ruin. Something similar was going on in South America, as is evidenced by the break-up of the great early cultures of Peru and Bolivia. It was a sort of American Middle Ages.

New World progress, however, was not permanently halted. For although some groups had definitely shot their bolt, other people, previously unimportant, came to the fore. In South America the Incas, building on the ruins of older and lesser states, welded a vast and powerful empire. The Aztecs were gradually spreading their rule over Mexico. The new orders seem to have resulted in economic, political and military, rather than in intellectual achievement—just as Rome surpassed Greece in material and administrative matters, and trailed or imitated her in arts and letters. But the Inca had already brought peace and order to large parts of the Andean region, the Aztecs would doubtless have done the same in Middle America. Under such conditions, even greater heights might well have been attained.

As to this we shall never know. For the white man came with fire and sword; the flimsy, immature political structures of the Incas and Aztecs fell instantly apart. Native American civilization, slowly and painfully built up through the centuries, was brought abruptly and definitely to an end.

This, for the anthropologist, is one of the great tragedies of history. We should so greatly like to know what would have happened if the Indians had been allowed time to work out their problems in their own way. Would the Inca and Aztec empires have continued to expand and flourish? Would they have met and clashed? Would they eventually have been poisoned by the social toxins that, in Europe and Asia, have brought empires, one after another, to ruin? And if the Inca and Aztec had fallen, as Rome fell, would the torch of civilization have passed to others, perhaps in the more temperate zones of North or South America? That was what happened in the case of our

own civilization, which was born in the eastern Mediterranean, flowered in Greece, was carried on by Rome, languished through the Middle Ages, and burst forth again in northern Europe.

Would the Indians have invented the wheel? Gunpowder? Would iron have come into use? Would steam have been harnessed? Electricity?

One can believe that these things, or most of them, would have come to pass. There would, of course, have been differences. History, to reverse the proverb, never repeats itself. The contours of the continents, the lay of mountains and plains, forests and deserts, would have played their inevitable rôles in shaping the development of the Indian, and the drifts of his culture.

But nevertheless one has the feeling that, by and large, American developments would have continued to parallel those of Eurasia. To make this clear, the analogies between the history of pre-Columbian America and that of the Old World must again be stressed.

In both hemispheres man started from cultural scratch, as a nomadic hunter, a user of stone tools, a palaeolithic savage. In both he spread over great continents and shaped his life to cope with every sort of environment. Then, in both hemispheres, wild plants were brought under cultivation; population increased; concentrations of people brought elaboration of social groupings and rapid progress in the arts. Pottery came into use, fibres and wools were woven into cloth, animals were domesticated, metal working began—first in gold and copper, then in the harder alloy, bronze. Systems of writing were evolved.

Not only in material things do the parallels hold. In the New World as well as in the Old, priesthoods grew and, allying themselves with temporal powers, or becoming rulers in their own right, reared to their gods vast temples adorned with painting and sculpture. The priests and chiefs provided for themselves elaborate tombs richly stocked for the future life. In political history it is the same. In both hemispheres group joined with group to form tribes; coalitions and conquests brought preeminence; empires grew and assumed the paraphernalia of glory.

These are astonishing similarities. And if we believe, as most modern students do, that the Indians' achievement was made independently, and their progress was not stimulated from overseas, then we reach a very significant conclusion. We can infer that human beings possess an innate urge to take certain definite steps toward what we call civilization. And that men also possess the innate ability, given proper environmental conditions, to put that urge into effect. In other words, we must consider that civilization is an inevitable response to laws governing the growth of culture and controlling the man-culture relationship.

If we go that far, if we can admit that there are such fundamental laws, we can assure ourselves that our fate is not entirely controlled by blind, brute accident; that we may, with greater knowledge, some day be able to steer the cultural machine.

So the problem of whether or not Indian development was, so to speak, a home product is much more than a mere archaeological puzzle.

Fuller knowledge of the Indian's history can bring us nearer to understanding the laws of cultural evolution. Such knowledge is also needed for approach to another most difficult, but most important question; this concerns the innate mental equipment of different peoples. Do all races possess the same general sort of brain? Are some racial groups fundamentally more intelligent than others? Have certain races special abilities? Or special defects?

We need to know this because modern transportation has to all intents and purposes, abolished space. And we are busily engaged in spreading a veneer of western European civilization over the entire face of the globe. The process, like the growth of modern mechanical science, cannot be stopped. What will be the result? Are all peoples equally well fitted to assimilate that civilization? Will it destroy the less able? Or, if not destroyed, must they be ruled by overlords of better mentality? What will be the effect upon the human race of the inevitable mixing of blood?

These are problems of a contracting world. They are so pressing, and the need is so great for real understanding of the races and cultures being brought so violently into contact with each other, that the anthropologist looks wistfully at the six-

million-dollar telescope now being mounted in California to probe an expanding universe. It is doubtful if that much money has been spent in the last fifty years for pure research in any one of the social or humanistic sciences. What one regrets, however, is not the increase in our knowledge of astronomy or of the other exact sciences, but our failure to gain sufficient insight into what makes the human wheels go 'round.

No one can fail to be grateful for what physics and chemistry have done in giving us control of our environment, or for the alleviation of suffering made possible by advances in medicine and surgery. The anthropologist's plea is not for less research along those lines, but for more research upon the less tangible but, in the long run, even more important aspects of human life. Only with far more accurate knowledge of man's innate abilities, of his relations to his fellow men, and to his culture, can we hope for safe issue from the troubles brought upon us by our blind and stupid misuse of our newly acquired, and still growing, command of the material world.

The reason, of course, for our slowness in gaining that knowledge is that man is so complex a creature. Lack of progress in the social sciences is due to their inherent difficulty. A plant is harder to understand than any rock, because to its basic chemical structure are added the mysterious forces of life. Any animal presents more baffling problems than any plant—for to life is added the even more mysterious factor of consciousness. And topping the pyramid comes man—a chemical compound like the rocks and the plants and the animals, but to whose life and whose consciousness are superadded the hitherto ungoverned but all-controlling powers of culture.

It is small wonder that we have made so little headway toward the understanding of man. But the sooner we realize what we are up against the better off we shall be. Only by facing situations can they effectively be grappled with. And the relation of man to his culture, to which we keep harking back, seems to be the one problem with which we must first come to grips.

The human race has surrendered nearly everything to culture. Culture, not man's brain, has come to carry our accumulated store from generation to generation. Culture has grown, evolved. But what of the human brain? We know that in cer-

tain ways it has lost, just as our bodies have lost their primitive strength and hardihood. For at birth our brain holds less than that of any other mammal. Far more, however, than any mammal are we capable of learning and of profiting from individual experience. In that way our brains have evolved, enormously. But, and this it seems is the great but, is that evolution still going on? Are we equipped with better mental tools, are we innately more intelligent than we were one, or five or fifty thousand years ago? Or have we come to be so thoroughly dependent upon culture that we have left it to do our evolving for us? If so, God help us, for our culture, like Frankenstein's monster, is a soulless thing.

The anthropologist, student of man, perceives trends and tendencies which fill him with alarm. But, on the other hand, only the anthropologist can readily appreciate the extraordinary toughness, and persistence, and resourcefulness of the human race. Most of us take our present state for granted—we do not bother to think how hardly it was won. We seldom realize the overwhelming difficulties man has undergone during his long upward struggle, how our naked arboreal ancestors fought their way through the perils of a savage and hostile world; how they added to their power through development of speech, and by the taming of fire, how they put stone and bone and wood to their service as tools. These things the anthropologist realizes probably more clearly than any one else. He sees the human race triumphing, little by little, in the face of countless defeats, over every obstacle; he knows that after each setback the rise has been a little higher. And seeing all these things so clearly he cannot but feel that our present troubles will also be conquered. But, and of this he is sure, salvation can only be attained through greater knowledge of man, not only modern man, but of man throughout the ages, and in both the Old and the New Worlds. In other words, what we really need is more and better archaeologists.

MR. KEPPEL The third speaker is rather a hard person to classify. He is an author whose books on the culture of our cities have been very welcome to many of us, but he is not just an author. I think there are two reasons why he is so hard to classify. One is that he has a most insatiable curiosity. He has been up more trails than anybody I happen to know. And in the second place, his independence has enabled him to avoid with extraordinary success the academic and other labels with which most of us have been plastered at one time or another.

But the main point and the reason for his being here today is that he is not too young to dream dreams, nor is he too old to see visions, and his name is Lewis Mumford.

MR. LEWIS MUMFORD Mr. Chairman, members of the American Philosophical Society, ladies and gentlemen, my topic is the future of American culture. A rather inconsecutive future to the two papers that were given previously, and yet I think it joins up with both of them.

LOOKING FORWARD

LEWIS MUMFORD

Author

(Read April 19, 1940, in Symposium on Characteristics of American Culture and Its Place in General Culture)

AMERICAN culture is the outcome of a movement that began among European peoples in the fifteenth century and reached its height throughout the world in the nineteenth. Almost every trait that we call American, whether in approval or in disparagement, had its origin in Europe. We Americans are largely transplanted Europeans or Africans. Our culture is part of a world culture that now embraces people who live in every part of the planet. If in the course of three hundred years we have developed special traits and aptitudes and institutions, and undoubtedly we have, the same fact is true of other regions and peoples. This culture is universal but not uniform.

The impulses that have characterized modern Western culture no longer have the strength and pressure they showed during the last five centuries. This is partly the result of our very achievements in exploiting natural forces, inventing machines, and spreading over the planet; and it is partly due to the weaknesses and disintegrations that were inherent in this culture itself; for it sacrificed the fullness and wholeness of life to the cult of power. We are now probably living through the last great crisis in this power civilization, which will either ruin Western society, or permit it to establish itself upon a broader human basis than the conquistador, the militarist, and the industrialist of the past thought wise or necessary. No one can make a sound prediction about the future of American culture who does not understand both the original forces of expansion, and the worldwide conditions that are now bringing this phase of our development to a halt.

The first thing to note is that the period of land expansion throughout the planet has come to an end. Since the 1890's, when Professor W. J. Turner first called our attention to the

fact in his now famous essay, the closing of the frontier has been a familiar theme to intelligent Americans. With this occupation of our political domain the pioneering phase of our culture is over its restlessness, its derring-do, its quick improvisations, its general shiftiness and uncertainty and speculative enterprise, its covert contempt for stable ways and for the funded stock of human experience, are all qualities that belong to a rapidly fading past. The process of colonization gives place to that of settlement. Instead of seeking new territory, we must make the most of what we have, and instead of seeking wealth by expansion, we must seek it by intensive cultivation.

The closing of the frontier has always had an ominous sound to Americans, but the fact is that the closing of the physical frontier may mean an opening up of spiritual frontiers. During the first era of colonization, the chief demands upon us were material ones: we had to master a strange environment and develop the means of surviving in it. Such borrowing and intermixture as there has been, throughout the planet, has taken place chiefly in the domain of natural resources and artifacts, like the rubber culture that the white man took over from the Brazilian aborigines—a gift that fully equals any that the white men brought.

The transferal of interest from the material conditions of survival to an understanding of the arts, the ideas, the values, and the natural environment of the conquered territories is a recent change. In many parts of the world, not least in the United States, this love and sympathy and understanding have come belatedly, only after colossal damage has been done; yet finally it has arrived. Already, the sports and dances of the Polynesians, and the music of the negroes, have made their contribution to the local culture of America, and through us once more to the rest of the world. Similarly, the esthetic traditions of the Japanese and the Chinese have during the last century made powerful modifications in our conception of the arts; a long succession of American scholars, beginning with John La Farge and Ernest Fennollosa, continuing in the architectural initiatives of Frank Lloyd Wright and the modern Hawaiian school, have done much to open the doors of our minds to the East.

The notion that modern culture is a one-sided spread of Western ideas and methods is a superstitious one, based upon the parochial egotism of the first missionaries and traders. On the contrary, the mechanical conquest of our planet, by means of railroad, steamship, cable, and radio may be interpreted with equal verity as a means whereby the mechanically more primitive cultures may in turn influence and civilize their European conquerors. They may restore to them some of that deep organic sense of unity with the environment, some of that sensuous enrichment and playful enjoyment that Western man has so often forfeited in his aggressive conquest of the environment and in his ascetic efforts, not only to invent machines, but to turn himself into a machine, worshipping idolatrously the creature he has conceived.

At all events, the period of land expansion is over. The open lands have been occupied; the whole tide of movement and migration, except under the pressure of savage conquest, such as we have just witnessed in Finland and Denmark, is at an end, region by region, we have to work out a stable pattern of occupation.

In the domain of the machine, the very same process is taking place; a slowing down of the tempo of mechanical conquest and the creation of conditions that call for balance and organic interrelationship, rather than for lonely pioneering enterprise.¹ Everyone used to assume that the expansion of industrial enterprise, which took place on such a colossal scale in the nineteenth century, was bound to accelerate rather than to decrease. This assumption was singularly unimaginative. For a good part of the mechanical activity of the nineteenth century consisted either in building new machines and utilities in areas where they had not existed before, or in the transformation of handicraft industry into machine industry. Neither of these processes could go on forever. Today a large part of our industrial plant is on a replacement basis; this holds true likewise for its products, and even the raw materials with which industry works, like the supply of metals and rubber, by now form part of a revolving fund.

Our industrial organization, which from the beginning was

¹ For evidence see Mumford, Lewis: "Technics and Civilization" New York 1934

keyed to expansion, now faces just the opposite conditions: stabilization and balance. The notion that the machine would expand because there is no limit to human wants was based on a double fallacy: the assumption that the machine was devised mainly to serve human wants, and that industry could, purely on a mechanical basis, satisfy them. But the fact is that those who put their faith in mechanical inventions have failed to see that only a modicum of our needs as human beings are encompassed by the machine, or are included in the territory it has conquered. Love and parentage, the arts that intensify the activities of eye and ear and touch, the arts that discipline the body and enrich the mind, the arts of communication and communion and social cooperation are only quite incidentally involved in the mechanical conquest and exploitation of the environment, or in the subjection of life to a rigorous mechanical discipline. In part, human needs are hostile to our ritual of economic enterprise. Beyond what is needed to provide what Aristotle called the material basis of the good life, our absorption in the machine deprives us of leisure, of the means of cultivating the arts and sciences for our personal illumination and enjoyment.

Hence the promised slowing down in the tempo of production, so far from being hostile to the real values of civilization, will on the contrary provide the first opportunity since the Middle Ages to advance our culture as a whole, instead of letting a single fragment, the will-to-power, the will-to-conquest, take precedence over the whole fabric of human aims and desires. The increase of human wants on a material level might mean only the increase of gadgets and publicity and mechanical clatter. Our problem rather is to create norms of consumption which will raise the level of human living for the whole population. In short, the stabilization and diversification of human wants, the acknowledgment of biological and social needs as well as mechanical needs, is an essential feature of modern culture, as it advances beyond the crudities of the early utilitarian period. From the standpoint of profit and power this may mean a curtailment of opportunity: but in terms of human living it means an enrichment.

Finally, the most critical change of all has been taking place in Western culture since about 1870, when the effects of contraception first became visible in the population graphs. For

the basis of our expansionist culture has been the mass production of human beings. This development went hand in hand with the opening up of new areas for agriculture, with improvements in agricultural technique, with the addition of new food crops and higher dietary standards. In the United States, between 1955 and 1970, if present tendencies continue, the population will for the first time be on a replacement basis. We are within sight of our last frontier—the population frontier. Here again our problem is no longer quantitative but qualitative: we must make the most of what we have, instead of wasting human material recklessly in a one-sided effort to pile up material advantages, whose burdens are out of all proportion to their benefits. Once more every life becomes precious: not merely at the moment of birth and through infancy, but at every subsequent phase of life, not to be used frivolously, not to be deprived of its great heritage.

With the end of this period of expansion, the conditions of life in the United States are once more potentially favorable for human culture—as favorable, if we use our opportunities, as they were in New England in that brief period between 1810 and 1860 that saw its flowering.

Now the future of American culture is bound up with our reaction to the conditions that we must now confront. If we continue to hold in fantasy to the conditions that governed our past, if we attempt to relive a stale dream of expansion, material conquest, purely mechanical progress, and if we fail to incorporate into our social myths and motives a new sense of what life may hold for us when we abandon conquest for cultivation, and one-sided pragmatic activities for many-sided efforts to achieve a balanced human personality—if we attempt to live in our dead past, worshipping our own dead selves, we will go the way of those many other cultures whose disintegration Mr. A. J. Toynbee has so masterfully described. Our very safety lies in realizing that the slowing down on one side of our needed activities brings with it the possibility of catching up on other sides: so that, in the long run, agriculture and industry, education and religion, the means and the ends of living, will work in harmony toward a common good: man's development and perfection as a social being, and society's development and perfection as a vehicle for personality.

The future of American culture is bound up with the creation of a new pattern of cultural activity, which shall be neither national nor parochial; but more intimate than the first and more open to worldwide forces and impulses and ideas than the second. We must construct an intimate regional framework for a balanced social and personal life, in harmony with the underlying possibilities of landscapes and regional resources and people; and we must achieve this local balance within the larger framework of the world as a whole. If the world as a whole disintegrates and submits to the rule of barbarous men, bent on predatory exploit and military conquest, we will be involved in this process, already we are involved in it and threatened by it. Our health and our safety consist in living more deeply as members of a regional community, and living more widely, as members of a world community. Unless our thinking and feeling tend in both directions, the conditions necessary for cultural development will be lacking.

Here I would stress the fact that American culture has always had this double polarity, regional and worldwide, whenever it functioned positively as a culture. Throughout the country today, as in New England during Emerson's and Hawthorne's youth, we are in the act of discovering our diverse regional roots and affiliations. This discovery is fundamentally a healthy one; and it has been proceeding actively in all the arts, as well as in various domains of scholarship, history, literature, geography. It has been aided not alone by the rise of regional universities, but by the grand work that has been done by the various federal agencies that have been helping, especially since 1933, to reawaken regional life. Above all, by the arts projects, music, drama, painting, sculpture, literature, of the WPA, and the splendid series of state and regional guidebooks that have been produced. All these efforts, along with the truly notable and scientifically imaginative work of the National Resources Committee, have helped us to discover those regional individualities which constitute the very backbone of our political life. For if we are to think and act in terms of a larger world, we must first see, know, understand, admire, and love some intimate part of the world. All our wider knowledge must be based on first-hand experience and self-knowledge, or it is only a vain phantasm, and not real knowledge at all.

But we must not forget the worldwide connections of all these activities. The regionalist movement is now flourishing in America partly because it has been helped by the example of a worldwide regional revival that was begun, in the ancient region of Provence, in 1854, a movement that has long challenged the mechanical and political uniformitarianism which the nineteenth century boasted, even as it now challenges the barbarous totalitarianism that has, one hopes only temporarily, taken their place. So, too, our interest in public art dates from the great upsurge of social and esthetic effort that took place in Mexico following their great revolution. Cultural advances usually work in this fashion, by cross-fertilization. We know how the Vedic scriptures profoundly affected the thought of Henry Thoreau and how, through his essay on Civil Disobedience, American civilization returned the debt to India via Gandhi. So, too, in architecture: all our separate advances in steel cage construction were first worked out in England and France before they were applied in America; but it was here, thanks precisely to our native tradition of the light-framed cottage building, itself conditioned by our difficulties in getting skilled labor in pioneer communities, that we repaid the debt by working out in detail the simplest fireproof form of this type of construction. Finally we must remember that our greatest poets and philosophers, our Emersons, Longfellows, Thoreaus, Whitmans and Melvilles, like our great scientists, have always been spiritually members of a worldwide community as well as a regional one.

The problem for American culture in future is to keep a balance between the local and the universal elements in our culture. In our original mechanical conquest of the planet, we lost our connection with regional roots, and in the very act of perfecting the instruments of material domination we became hollow, empty of values, strong on means and weak on ends. In this state, there is always a danger, a danger which Rome under similar circumstances succumbed to, of being the prey to too many stimuli, and of becoming confused and unsteady, unable to resist what is new, whether it is good or bad, or to preserve the old, whether it is alive or dead. Plainly, the tempo and the extent of intercourse between cultures must be subject to an inner control. It is idiocy, to use the word in its literal Greek sense, to wall oneself off against the outside world; and

it is equally folly to take too much in; for both in art and life, it is possible to have too much wealth, too much talent, too many opportunities.

As a community, we must have a definite center; we must have a sound and unassailable core. We find this core in the region: the region as an object of knowledge and love, as a field of activity, as a focus for community responsibilities and interests. The re-differentiation of our country into real regions, and their transformation into true cultural units, is one of the necessary aims for the future development of American culture. Goethe said of the individual creator: "In der Beschränkung erzeugt sich erst der Meister", "it is by his restrictions," by his self-imposed limitations that is to say, "that the Master first discloses himself." One can say the same thing of a community. It is by resolute attention to its individual idiom and properties that it first displays its own mastery over its conditions of life.

But one of the reasons why this firm sense of individuality is so important today is that it enables the regionalist to meet, on terms of equality, without any sense of inferiority or impotence, the products and peoples of other regions; and to take from them what is necessary for further growth, for higher creative effort. Without this challenge from the outside, and without the kind of aristocratic pilfering and borrowing that takes place openly between equals, always of course in discreet amounts, there is no true basis for development. We must deepen our regional roots in future precisely so that we may, without danger, spread our branches wider and cast a shadow over the whole world. Our best regional products are always a contribution to humanity's common store; and our culture in America will live or die by reason of the skill with which it establishes local conditions that can be universalized throughout the planet, and creates universal conditions that aid, instead of hampering and discouraging, local life.

This whole argument as to the future of American culture can be summed up in a single word: balance. We must achieve a balanced satisfaction of human needs, the social, the esthetic, the religious, the personal, no less than the mechanical and utilitarian needs; and we must achieve another kind of communal balance between the intimate, the regional environment, and those universal forces and that wider environment which in-

clude the planet as a whole, with all its great diversity of places and peoples and traditions. There is no hope for us, no health for us in any doctrine of cultural isolationism. We have sprung from and incorporated every nationality, every culture; we have our roots everywhere and draw our nourishment, not merely from our own land, but from the wide world. We have in our own constitution broken loose from an arrogant parochialism; and we forfeit our very birthright unless we regard every other land as an essential and precious part of our America. In that sense, the future of American culture is being decided today in the bombed cities and harrowed fields of Europe and China. If barbarism prevails in other areas of the world, that part of civilization which we maintain will also perish. If the Dark Ages return in Europe and Asia—and they are much closer than most of our fellow citizens, in their complacent optimism, realize—there will be no dawn in America. These sobering thoughts must govern all our hopes, all our plans, for the future.

MR. KEPPEL We have heard all our formal speakers, and we have plenty of time for discussion. We have the great privilege of having our fellow member, Mr. Van Wyck Brooks, to lead the discussion.

DISCUSSION

VAN WYCK BROOKS

Author and Literary Historian

(Read April 19, 1940, in Symposium on Characteristics of American Culture and Its Place in General Culture)

It has been suggested that I should open a discussion of the very interesting papers to which we have listened, and perhaps you will forgive me if I attempt to do so with a few notes in my hand. Our subject is American culture, and Mr. Kidder has told us how important in a people's life is a certain crystallization of culture. This word has various meanings, but our principal emphasis here is not upon culture conceived as a system of folk-ways, but rather on culture as expression, as self-understanding, as a means of having life, intellectually, esthetically, spiritually, and of having it more abundantly. In different ways, Mr. Allen and Mr. Mumford have sounded hopeful notes in regard to our culture. Mr. Allen has drawn a lively picture of the great new popular instrumentalities of culture, but Mr. Mumford has suggested that it lies under the shadow of swords and that anything hopeful in the world at present is a gamble. Until we have somehow scotched the will-to-power that is playing ducks and drakes with modern life we cannot expect to realize any of our hopes. But it is a striking fact that we remain endemically optimistic. Eminent minds from other countries still assert, in numbers, as they asserted in 1776, that America is the hope of the world. I have heard this phrase within a week, and surely no other country could be so described at present. But, while we once accepted these affirmations with complacency, we now regard them as a challenge.

Now, certainly nothing is more involved in this hope which we have for the world than the state of our literature and art. Are they in a position to grow and flourish? I agree with Mr. Mumford and Mr. Allen that, for a number of reasons, they seem to be so, and I shall mention two or three. Mr. Francis Taylor, the new director of the Metropolitan Museum in New

York, will speak to you this afternoon on the promise, as he conceives it, of American art, and it has been suggested that I should speak of literature and what I see of promise in this field. I should begin by saying with Mr. Mumford that the closing of the frontier has determined this promise, for it is obvious that, as long as this frontier was open, writers did not wish to live on it. Mark Twain, Howells, Bret Harte, the most important Western writers, withdrew from this frontier on which they had grown and which lost much of the benefit of their interpretation and criticism during the years in which it remained a chaos. They sought the stable conditions that favored their growth, in spite of the narrow conventions to which they submitted; for the frontier, with its restlessness and its improvisations, constricted still more their freedom of thought. And the first and most evident result of the closed frontier is that the Western writers have ceased to migrate. They are more and more willing to live on what was once the frontier and what is now becoming a spiritual frontier. In this they are bearing out Mr. Mumford's statement that "instead of seeking new territory, we must make the most of what we have, and instead of seeking wealth by expansion we must seek it by intensive cultivation."

This is a remarkable fact of the moment, as anyone can see who has lived in or near New York. Thirty years ago, when I was just beginning to write, we heard much of Greenwich Village. Greenwich Village still exists. It has become our permanent Latin Quarter, with its history and its settled conventions and intellectual habits. But at that time it was peopled with restless souls from the South and the West whose only thought was to escape from home. Anywhere, anywhere out of Ohio, out of Kansas, Michigan, Arkansas, Alabama, anything to escape from the town or the region, from the God-forsaken place where one was born! This was the desperate impulse of all young writers, whether they came from Boston, which was certainly dying, from Philadelphia, which was dead and buried, from Chicago, that vast Spoon River, from the swamps of the South or the plains of the West. Every writer in the country repeated the words of Leopardi, who was condemned to live in his native town. "As to Recanati, I answer that I will leave it, escape from it, hurry away from it as soon as ever I can. Be

assured that my intention is not to stay here, where I see no one beyond our household, and where I should die of frenzy, of life-weariness, of hypochondria, if one could die of these ills. It is all very well to say, Plutarch and Alfieri loved Chaeronia and Asti. Loved them and left them. In this fashion I also will love my native place when I am far away from it. Here literature is a word unknown " So all our writers spoke, and I remember that, as a publisher's reader, I read a fresh novel every morning in which the writer's birthplace was bitterly described as a sink of Philistinism and a trap for genius. What a change has taken place in this state of mind! Five years ago in Washington I knew well a young couple who had grown up in Montana Both wished to write and both have since written interesting novels. The husband was partly French, partly Irish and partly Blackfoot Indian; the wife was the daughter of a Norwegian farmer. Both were familiar with many of the world-ideas of the moment, and twenty years ago their only thought would have been to escape to Greenwich Village or Paris Both had only one thought, to return to Montana, to live there in shanties, in lean-tos, in the loft of the barn, just so that they might be able to write of Montana, the most interesting, the most thrilling spot in the world. This, in my recent experience, is a commonplace incident. Every few days I hear of some young writer who wishes to remain in Oregon, in Iowa, or Nevada and who feels that he can dig best where he stands. All the states now have their writers who feel that they possess something unique, or that they can learn to possess it; and our most interesting books now come from little towns in Mississippi, Idaho, Louisiana, where no writer twenty years ago would have felt that he had the slightest chance of survival. There lies the promise of intensive cultivation that seems bound to yield fruits in the future. And this seems the more likely in that two frontiers have been closed at the moment, the frontier of Europe as well as our own frontier I believe that among our younger people world-ideas have sufficiently spread so that the closing of this frontier of Europe will mean no separation from European thought. On the contrary, European thinkers, and many of the best, have come to this country in such numbers that we cannot escape it if we would. These European minds are scattered in our remotest corners, and they militate against provincialism

in dozens of regional centres that formerly knew nothing of the world. Having nowhere else to go and finding their regions full of interest, the new writers, I think, are sure to make bread of what they once regarded as the stones of their pastures.

This re-discovery of America, on the part of American writers, is to me the most inspiring fact of our time. We are discovering that we have roots on every side that only require care, sunlight and water to grow into flourishing plants and trees. The strange thing is that we were so long in becoming aware of these roots, even in the parts of our country where tradition was strongest. Is this not because our patriotic forbears loved their country, indeed, but in a way were not greatly interested in it. When John Fiske began to lecture about our history, he found whole towns indifferent to it that would listen gladly to stories about the Tudors and the Borgias. Our forbears associated the interesting with a journey to Europe, and nobody ever thought that American guide-books could rival Baedeker in interest until the American Guide Series put us to shame. I have heard a friend of mine, whose family has been connected with American history ever since the French and Indian wars, express the utmost astonishment over the fact that Roger Williams had such bright ideas. Not till Parrington wrote his history had he ever discovered this fact; and, while there is much of ancestor-worship in our older American circles, how many know how rich our history is in intellectual adventure and esthetic interest? My own case is typical as one who grew up in the unloved state of New Jersey, that once proud state where the Boudinots lived, where the Bonapartes also lived and where Cooper was born. What a revelation to me was a letter of Fenimore Cooper's father, regarding the education of one of his children. He did not wish to send this child to school in New England, fearing that he would acquire the debased and inferior manner of speech of the Yankees. No, for pure English undefiled he proposed to send this child to school at Newark. That was in the days when the poet Philip Freneau had his fine printing-press near Morristown. For the first time I understood why John Woolman loved my state and why the "American Vasari," William Dunlap, longed for the lovely meadows of his native Perth Amboy. In those days Weehawken was an earthly paradise and there were noble dreamers in the Hoboken dells, most of whom had their

portraits painted, for it only required the price of a stage-ticket between New York and Philadelphia to pay for a portrait by Dunlap or Samuel Morse. But we all knew that New Jersey had an intellectual history, even as Philadelphia more strikingly had one. What we are only coming to know is that the Middle Western regions are rich in intellectual history also. Miss Constance Rourke of Michigan is engaged in a statesmanly project. This author of the lives of Audubon and David Crockett is canvassing the folk-culture of the Middle West and the popular arts of that region a century ago. She is digging up astonishing facts about unknown musicians and painters who flourished, or at least existed, in a part of the country whose only known past hitherto has been the past of politics and battles. She is finding roots everywhere just under the ground. This knowledge that we have hidden roots in what once seemed barren soil is certain to foster our growth in times to come.

There is another circumstance that seems to me to promise much in regard to our literature. This originated on the spot where we stand; for I believe it was Franklin's intention in founding this society to bring the "ingenuous minds" of the colonies together. In this purpose Franklin succeeded astonishingly well. He created what might have been called an intellectual public opinion out of which the nation rose from its scattered outposts. He knew the value of what Henry Adams called a "school," an association of minds for the furtherance of all. "What we need," wrote Adams in 1862, "what we need is a school. We want a national set of young men like ourselves or better, to start new influences, not only in politics, but in literature, in law, in society, and throughout the whole social organism of the country,—a national school of our own generation. And that is what America has no power to create. In England the universities centralize ability and London gives a field. So in France; Paris encourages and combines these influences. But with us, we should need at least six perfect geniuses placed, or rather, spotted over the country, and all working together; whereas our generation as yet has not produced one nor the promise of one. It's all random, insulated work, for special and temporary and personal purposes, and we have no means, power or hope of combined action for any unselfish end." Thus Adams in 1862, and yet Franklin had largely

succeeded in forming this school, which had so much to do with forming the country. In the chaotic years in which Adams lived all hopes of this kind went by the board. There is a letter of the poet Woodberry expressing the feeling of that time, "Somehow the men of my generation never seemed to get acquainted with one another." What a wistful echo of the days of the open frontier!—when so many writers lived underground lives, wholly out of touch with one another, nursing their disillusionments and their pessimism, in utter despair of the republic. It is the good fortune of our day that the "ingenious minds" have found one another, with some of the results that Franklin anticipated, and that, vast as the country now is, the writers have a common will and purpose. Living in far corners, they correspond with one another; they are united in organizations and leagues, and the world-threat of totalitarianism has given them an extraordinary unity of democratic feeling. A network of communication has been set up among these writers, to the farthest corners of the West, the North and the South; and they feel that they are members of a guild, a community with vital ideas in common, which they share with a world-community of liberal minds. All of them gain confidence and strength from this. We do not forget that Franklin was the first of American writers to see the value of this community of minds, and how good it is that on this spot we writers should be brought into closer relations with workers in all the other intellectual realms.

MR. KEPPEL Have we overlooked the change that has come over our culture in the rapidly increasing proportion of responsibility for that culture that we males are turning over to the women? Does anybody want to talk about that?

MR. HARRISON S. MORRIS I suppose I ought to say what I want to say about that in regard to fine arts

The fine art of today called modernism was in a large degree promoted and patronized by one of the leading ladies of the country and one of the very wealthiest. And the exhibition of Picasso alluded to by Mr. Allen was one of the fruits of her labors

On the other hand Mrs. Logan of Chicago, a lady of standing, wealth and influence, connected with the Chicago Art Institute, was very much disturbed over the exhibition of Picasso which was held in their Institute, and she has, by the way, inaugurated a society for sanity in art. There are a number of branches of it being formed in various cities in the West, one is beginning here

She sent me the article which she wrote at the request of a Chicago paper, and I'll read you what she wrote "I have been requested by the *Chicago Daily News* to write my impression of the exhibition for publication in this paper. I shall give the artist's ideas of his own work. In the introduction of a catalog (this is, of course, Picasso's introduction) is an account of an interview with Picasso. This is what he says

" 'We all know that art is not truth. Art is a lie that makes us realize truth, at least the truth that is given us to understand. The artist must know the manner whereby to convince others of the truthfulness of his lies '

"As a contradictory comment on the above, he says 'One cannot go against nature. It is stronger than the strongest man. It is pretty good to be on good terms with it. Certainly painting has its conventions, and it is essential to conform with them. Indeed, you can't do anything else. And you ought always to keep an eye on real life. Academic training in beauty is a sham. We have been deceived, but so well deceived that we can scarcely get back into the shades of truth. The beauties of the Parthenon of Pheidias and various others are so many lies. Art is not the application of a pattern of beauty, but what the instinct can conceive beyond nature. The Parthenon is really a farmyard over which someone put a roof ' ' There you have Picasso

MR. KEPPEL As you will notice, the papers of this afternoon will be somewhat more special than these general ones. This is our time for discussion of the challenging statements made by our speakers this morning.

MR. C. E. K. MEES In the last speech, Mr. Mumford stated that the frontiers had closed, and he continually repeated the statement that our material expansion had ceased. As one of those who is responsible for that material expansion, I want to enter a definite protest and denial of that statement. Our material expansion is based on the technological application of science. It will cease only when the acquisition of new

knowledge and science ceases. So far from that diminishing or slowing, it is increasing with ever-increasing velocity.

The problem of the increase of scientific knowledge and its application in technology in this country as in the rest of the world—but above all in this country—is that which the chemist calls an autocatalytic reaction, one which increases in velocity by its own product. Today chemistry in this country is advancing with a speed which is utterly unprecedented in the history of the world. It is giving us a command over material resources that we could never have anticipated or dreamed of. The mere limitation of the land areas is a matter of no importance whatsoever in technical advancement. In material progress, we are dependent, not on the land areas of the world, but on the knowledge of man.

MR EDWARD V HUNTINGTON. May I call attention in connection with what has just been said to a new book, of which many of you may not know. It is by Carl Snyder and is entitled, "Capitalization, the Creator." He is the only writer that I know of who has the courage to mention the word *laissez faire* with some respect.

I was struck with the modernism of all the speakers this morning in totally ignoring two phases of culture which would have undoubtedly received a large share of attention a hundred years ago. In the first place, as I have already mentioned, nobody mentioned Carl Snyder's book, which is regarded by some as one of the most revolutionary books in fifty years. Also, nobody mentioned any form of theology.

MR W F G SWANN. I think we have one great problem to face, in addition to those mentioned. When all this has been made rosy as far as utilitarianism is concerned, how are we to realize the future of culture? There is a limit to the amount of satisfaction which one can get by simply being aware of the cultural creations of others.

Therefore, I think one of the things that will become necessary in this age will be to provide man a means by which he can find out what he must do in order that he may be happy. And the next thing to realize is that man can never get any permanent satisfaction out of anything for which he does not have to work. The mere observation of culture in others will pass in a very short time.

MR H N RUSSELL. I want to raise one question on which Mr. Allen touched. And I must say frankly that it didn't satisfy me. Namely, the question of how in the culture of which he is painting the most cheerful picture I have heard for some time—the culture of the future—we are going to maintain and raise quality?

Granting, at least for the sake of argument—but I hope for more—what Mr. Allen said about the higher general level of appreciation, I have something to add to what Mr. Swann said on the assimilation of happiness.

Human cultural levels are by no means fixed. In the past what has raised the general level has come down from individuals above that level. How is this future—this future of widely disseminated interests—this future of the three million circulation rather than the small circulation—

of the *Saturday Evening Post* rather than the *Atlantic*—how is that future going to influence the higher levels which are definitely above the average—the little group on the top of the frequency curve of capacity—the five per cent who differ among themselves more than the difference between anything except the five per cent at the lower end? How shall we meet this problem to the greatest advantage?

I have no answers to these questions except to say one thing. That in science we have one enormous advantage in that we are continually facing a nature which compels us to forcible readjustment of our ideas and expansion of our imagination.

MR. KEPPEL. Would it be agreeable to the company if we gave some of the speakers a chance in rebuttal? Let Mr. Allen answer or comment on what Mr. Russell has said.

MR. ALLEN. I said that I would prefer not to attempt to measure the altitudes of the surrounding summits, because I saw that we would let ourselves in for a heated discussion. I refuse to get into a heated discussion of the surrounding summits. I can only say that my guess would be that the summits are of pretty good altitude now. If we start discussing individual cases, we are going to be throwing things. I think we can rely on the quality of the upper five per cent. We can rely pretty well on the native intelligence of the people and on the opportunity for a large sample—a large group—of the people getting into the early stages of the arts. I refuse to be put in the position of taking the magazines for the three millions as my idea of the choice of the cultured. I was simply suggesting that those magazines do very well, considering the enormous crowds that they reach. After all, I work on *Harper's Magazine*, where we try to strike a somewhat higher level. If we fail, it isn't because we aren't trying, and I think that would apply to a great many of the rest of us who operate in these cultural areas.

MR. MUMFORD. I regret to say that so far as Mr. Mees' thesis was concerned, it is based on an optimistic interpretation of the present conditions of life due to a residue which has been held over from the past and which is very common. I am not impressed by this feverish activity in science or in technology, for the reason that we observe, for example, in Alexander's day—the second century B.C.—from the surviving manuscripts we have, that there was an enormous amount of scientific and mathematical activity taking place, just at the beginning of the disintegration of all culture. This disintegration was proceeding more and more rapidly from that time on, in spite of the enormous amount of road building, the great aqueducts, the mass control of the utilities of life, until finally the culture stopped. Why and for what reasons? Well, one of them was that there was a transfer of interests from this kind of activity to other fields.

Why again? Because, although there were many elements of real civilization, real advance, which have been preserved, the civilization as a whole which had produced the scientific culture of Alexander's day, did not support human life. And human beings rebelled, and the cul-

ture, itself lacking human support, disappeared. It took six or eight centuries for it to disappear, but it began to disappear just at this moment of great feverish activity and of enormous advance—enormous scientific advance. And what I would suggest today is that we are now living in a period which, in spite of these enormous advances which have been made during the last three hundred years and which are continuing, show signs of very deep disintegration, and this is due, among many other things, to a lack of balance between the various aspects of our culture. We have overweighted the physical content of civilization to such an extent that we have neglected the culture of man—the making of the civilized human.

We have the machines—the problem is how to control them and what is to control them. That is a problem that cannot be answered in our laboratories. It isn't answered in our factories. It isn't answered in our studies. And it must be answered if our civilization is to survive.

MR. MEES I should like Mr. Mumford to clarify his reply to me. I understand from what he says—and I should like him to correct me if I am wrong—that what he mentioned is that he foresees, that our material progress will cease or diminish—but that at the present time there is no evidence founded on facts for such diminution?

MR. MUMFORD The answer is that you were speaking about the spiritual culture frontiers which are still open, and the question is whether a disintegration will proceed from within which will ruin everything, or whether we can achieve deliberately a state of balance which will pull it back into harmony with the other factors in our civilization—those fields which are pressing knowledge down and also our power to assimilate it and to intelligently direct it.

FRIDAY AFTERNOON, APRIL 19, 1940, 2 O'CLOCK

MR. KEPPEL · We will go on from where we left off. The first speaker this afternoon is Philadelphia-born but within a few days will become a New Yorker. Whether that is a step up or a step down I will leave the audience to decide. In any case, that is not nearly so much the point as is the fact that the job which is bringing him to New York is that of the Directorship of the Metropolitan Museum of Art. And for that reason and others, I am very glad to have him as our guest today and to have him speak, in this general symposium, on the Fine Arts. Mr. Francis H. Taylor

THE FINE ARTS

FRANCIS H. TAYLOR

Director, Metropolitan Museum of Art

(Read April 18, 1940, Symposium on Characteristics of American Culture and Its Place in General Culture)

WE have been listening this morning to a discussion of the characteristics of American culture. Mr. Allen has given the contemporary picture. He has explained that there are no longer seven arts but seventy. He has shown that the creative impulse is keeping abreast of technological discovery and is manifesting itself daily in new media of expression which the world has never before known—sound movies, the radio, television, jazz bands and what for lack of a better word might be called “swing architecture,” a functional application of new engineering principles beneath a veneer of synthetic and very often untried (and sometimes totally inappropriate) building materials.

Mr. Kidder has approached the subject with the long view. As an archaeologist and ethnologist of a too long neglected continent he has shown the rise and fall of a civilization and, more important still, what happens in a world where the race survives but the civilization does not—a very sinister lesson for us when we pause to read the morning newspapers. Mr. Mumford has had the invidious task of “Looking Forward,” of taking the cultural evidences of today and casting them for the stage of tomorrow.

Throughout this morning's symposium there has been an undercurrent of hope and belief in the vitality of American cul-

ture. This belief I share implicitly, but I don't want to use either the term "Naissance" or the term "Renaissance" in speaking of those extraordinary forces now at work which will undoubtedly burst into flower in this country unless we are overwhelmed by political and military disaster.

I realize that a person in my position at a symposium of this kind is expected to do one of two things—make an apology for contemporary art or proclaim the advent of the American Renaissance. In every decade of our history some one has arisen to maintain bravely that the Renaissance is knocking at our door. But the American Renaissance will never arrive like a Golden Wedding as a reward for constancy. If it arrives at all we may not recognize it. Indeed it may have already begun two hundred years ago in this Hall of Philosophy. Or yet again it may not be evident a century hence to the historians of the future.

No more will I proceed to bore you with an appraisal of current American painting or sculpture. I will not single out any Michelangelo of the present nor attempt to justify this or that movement. Any quarrel between modernism and the Academy is completely foreign to this discussion, for such a controversy always degenerates into the question of whether one prefers spinach served with vinegar or with hard-boiled egg. There is after all only one art, a wholesome organic plant that grows irrepressibly like spinach, and the choice between the vinegar of modernism or the grated egg of the Academy is merely a matter of individual taste.

My purpose, therefore, in the few moments allotted to me on this program is to analyze certain of the social, economic and psychological factors in American life which are stimulating to the creative processes, and to show historically how the conflict between the Calvinistic conscience on the one hand and an independent, highly emotional temperament on the other have conspired to form this strange but easily recognizable phenomenon known as the American taste.

If we delve into our comparatively short past the question naturally arises: *Have we ever had an American art?* This has been for generations a bloody battleground. But the question is really unimportant, for while we may admit to this or that foreign influence or importation, we nevertheless are con-

scious of an American approach, an instinctive pathway of interpretation which stamps the portraits and furniture of our eighteenth century with an entirely different character from those of the Mother Country. Although in our nineteenth-century literature we recognize the written words as English, their selection and emphasis have produced a unique and thoroughly American idiom that expresses our national convictions. So it is with our architecture and our art, and for this very reason we cannot boast of a Renaissance. We are not as in Florence of the 1420's reviving an ancient and forgotten culture, nor are we on the threshold of a totally new experience. We are simply approaching as rapidly as social and economic circumstances will permit the plenitude of our intellectual maturity as a nation. When that point shall have been reached there will be no need to inquire whether or not we have an American art, for that art will be already a commonplace.

Possibly the growth of the arts in American history can best be examined by the study of a single phase, that of the museum which in its present form is largely the invention of eighteenth-century liberal thought. And I, therefore, beg your indulgence a moment in tracing not only the development of the museum itself but of its contents.

The earliest settlers brought with them to the Massachusetts Bay Colony very few works of art. What wealth these poor settlers possessed was largely in plate and a few sturdy household goods. Even books, save the necessary sources for theology and the law, were at a premium, and this past year has seen the tercentenary of our first printing press. But into the wilderness the Puritans brought two pearls of great price. One was the courage and will to succeed against any odds; the other was a Calvinistic conscience, disciplined by a few years' sojourn in Amsterdam, which recognized the importance of the intellect and the authority of the Augustan Age. That the arts were included in the program of the classicist was the happy accident which gave rise to the style of Early American architecture and its dependent branches. Thus from the very beginning the worthiness of art was always accepted by the fathers of our country however suspect may have been its practise.

As the later colonist prospered he turned to the avocations of the English gentleman after whom he patterned his existence.

The curio cabinet in which were brought together little oddments of art, numismatics and natural history became more and more frequent. Family portraits gave way to the "conversation piece" and the taste for "classic landscip." The eldest son went to London, occasionally to Oxford, and made the grand tour. He finished his education at Naples, where Pompeii and Herculaneum were being uncovered and he returned to his slaves and his plantation to read Gibbon and Jean-Jacques Rousseau.

Although there was a vigorous folk art which never came within his ken, and a few portrait painters like West and Stuart who abandoned their country for the Royal Academy, the esthetic significance of a new art emerging on a new continent never entered his imagination. Art was and has remained, unfortunately for many of our compatriots, a manifestation of intellectual respectability, confined to the wealthy and made at times graciously accessible for the enlightenment of the poor.

The American museum was founded in 1785 not two hundred yards from this spot by the Philadelphia painter Charles Willson Peale in the west wing of Independence Hall and from 1795 to 1812 occupied rooms in this very Hall. At Charleston and at Salem there had already been started societies for the purpose of establishing scientific institutions, but it was probably the tradition of Dr. Franklin which gave Peale's Museum its real impetus. Here he combined the *Wunderkammer* of the German nobleman (filled as it was with works of art as well as fossils and stuffed animals) and the formal gallery of the Italian Renaissance prince. There is a charming portrait of Peale in his museum at the Pennsylvania Academy of the Fine Arts. This was the formal genesis of the contemporary museum.

During the first half of the nineteenth century many abortive attempts were made towards establishing other museums. They were largely after the same pattern, the enlargement of the gentleman's curio cabinet, and were rarely open to the public. But during this period American painting and sculpture made valiant and conscious efforts to free themselves from the shackles of the Royal Academy and to become established on their own merits. Contacts with the continent, particularly through the group of expatriates in Florence and Rome who hobnobbed with the English refugees from the industrial revolution, brought about a realization that the facilities for training

the artist in America were slim indeed. James Jackson Jarves and Bryan of the New York Historical Society, as well as many others, set about to remedy this state of affairs. Public opinion clamored more and more for the establishment of museums where popular instruction in the virtues of the arts might be given. Throughout the sixties and the seventies, when the museums of New York, Boston and Philadelphia were coming into being, the importance of art rather than beauty or esthetic pleasure was the burgeoning theme.

Thus the museum became associated in the public mind with education as well as art. Prior to this time neither here nor abroad had there been any such conception. The art gallery was a collection of pictures pure and simple. The public, if they were admitted at all, could take them or leave them and were, in fact, defied to enjoy them. Fortunately at that time there existed a strange and now almost obsolete institution known as the liberal arts college. Here in the years immediately preceding our Civil War it was possible for a gentleman to acquire a bowing acquaintance with the major branches of science and the humanities. He was able to enter the great world with something of a synthetic picture of the past in his mind's eye, and he was ready to meet the future with an open and humanitarian tolerance which the age of specialization so singularly lacks. In the last three generations intellectual disproportion and inequality of judgment have probably done more to weaken the vitality of democratic thinking than any other single factor. We have become the victims in many instances rather than the beneficiaries of someone else's specialized thought.

It is at this point, I believe, that the Calvinistic concept of the museum being a factor for intellectual and moral good in the community began to take deep and lasting root. William Morris and John Ruskin in England had already protested against the encroachment of the industrial revolution, and the careful thinkers of the latter half of the nineteenth century saw ahead of them the necessity for some organized countermovement to technological progress and overspecialization. There were but two types of public institution developed over the centuries which were capable of recapturing the past on a large scale and of interpreting the present to the masses. They were the public library and the art museum.

In a brilliant essay on museum education Mr. William M. Ivins, Jr., stated that "in a certain way the great museum and the great library constitute the two halves of the community's memory of the past." This I believe to be the fundamental yet, for the most part, totally unconscious reason why the public tolerate museums, let alone support them. Their job is two-fold to reconstruct the past and give the opportunity for that refinement of taste which like "muscle can be developed only through exercise."

The American museum like the library has become the free university of the common man. It requires nothing of the student. There are no qualifications for admission; he can take away from it anything and everything that he is capable of acquiring. He continues to come, not merely out of curiosity but out of a determination to improve himself. And with it he derives from his visit a certain flicker of esthetic pleasure which at first he scarcely thought possible and which increases with each succeeding acquaintance with the arts. Here then is that marriage between the Puritan conscience and emotional life which has given such strength to American art at the present time. The proof of the pudding is in the eating. For, in the seventy years that have elapsed since the founding of the Metropolitan Museum of Art in New York, nearly eighteen hundred institutions, museums devoted to art, science and history, have been admitted to the American Association of Museums—eighteen hundred museums with a total annual attendance of nearly twenty million persons and an invested capital in land, buildings, collections and endowments of approximately four billion dollars.

The staggering proportions to which these institutional interests have arrived is the direct result of this Calvinistic purpose of which I speak. The public at large has learned to respect art and science, even history, and not altogether without loving it. One cannot think of their myriad manifestations as Mr. Allen has outlined them this morning without realizing how deeply they have penetrated the fabric of our daily lives.

Somewhere in his "Journal," which was published towards the middle of the last century, Delacroix mentions that there were 3,500 artists practising their craft in Paris. I have been told that in the census of 1930 some 60,000 persons in New York

City alone stated their occupation to be that of "artist." However apocryphal may be these actual statistics, the moral is nonetheless clear. The missionary zeal of one hundred and fifty years has borne fruit. Art is no longer the polite concern of the landed gentry but has become one of the major interests of a very large segment of our total population.

It is the size and power of this avalanche of art which make me believe in the vitality and progress of our culture. The problem of immediate quality is relatively unimportant, for, according to the law of averages, genius must occur every so often if the underlying base of mediocrity be sufficiently wide. For this reason it makes no difference whether or not you or I like what is being done today. Our duty is to see to it that American art has every opportunity of developing as a popular expression so that it may not be made the servant of a doctrinal minority as it is being forced to be in Europe. If democracy is to survive on this hemisphere it will be only through the preservation of intellectual freedom and tolerance.

One of the most thrilling experiences I have ever had was directing for a few months the Federal Art Projects in the New England States. I learned for the first time what it means for an artist to be free and how deeply concerned he is with his own time and with the world in which he lives. Coming at a moment in the depression when patronage of any kind, either public or private, had completely stopped, the intervention of the Government filled the artists of this country with a new hope and a new dignity. If there were abuses and agitations in the larger cities, the taxpayers reaped their reward in the nation at large. The death grip of the Fifth Avenue dealers was finally broken. The artist no longer was required to imitate the last picture reportedly sold to Mr. X for two thousand dollars. He was no longer dependent upon the whims of an editorial writer to make or break him. He became instead a simple workman who could take his dinner pail to work in the morning and be accepted by the plasterers, the masons and the carpenters as an ordinary and regular member of society. He is now no longer a pariah and outcast with long hair and short morals. He has entered once again into the life of the people from whom he came and with whom he has been happiest from time immemorial. True, there is still a noisy group, many of them on the

Government payroll, who feel that they are God's gift to the social sciences; but now with the Retreat from Moscow in full cry they are of less and less importance in the contemporary scene.

A "highbrow" has been defined as a person educated beyond his intelligence, and the American artist six years ago before the Government relieved him of his intellectual burden came pretty close to being a highbrow. Today he can return to being an artist, painting what he sees with candor and freshness. For this reason we are developing in the more remote parts of the United States an art of deep sincerity and intimate faith, in which are depicted the life, manners and inner convictions of the various highly disparate sections of the country. For the first time in a century the painting from the South looks Southern; the extraordinary work of the Southwest has a definite character of its own that does not need cowboys or red Indians for its identity; and in the painting of New England there lives again the strength and often parsimonious understatement of its origin. This, then, is what the Government in six short years has done for the artist. It has saved him from himself, from the horrors of introspection and European ferment. No longer dominated by the intellectuals of Greenwich Village he is, taken by and large, content to let the economist and politician fit together the jagged pieces of a broken and disordered world.

American art is probably the only art in the western world today that serves no ulterior purpose of propaganda or defeatism. More than that we have developed technologically a palette of such intensity, and so high in key, that we are able to say things which no previous generation ever knew how to say. These things are being said by simple individuals throughout the length and breadth of the land. They are not necessarily the pictures that you see in annual exhibitions, but they will be found by the critics and art historians of the future and pointed to as the documentary record of the emotions and convictions of a people in the moment of their expanding vigor.

Perhaps the one thing which characterized the liberalism of the eighteenth century was its awareness of its own time. This was equally true of art, for no period ever patronized its own artists more assiduously or was so candid in its criticism. The sinews of art, like the sinews of war, are flesh and blood. It

cannot flourish in a vacuum or on starvation wages Our problem is mainly one of awareness and distribution We have the art, we have the artists and we have the appetite Once we find its solution we will no longer need to sit around as we have today in a symposium wondering when the Renaissance will knock upon our door.

MR KEPPEL I wonder how many of you have observed that thus far this program has got along entirely without professors? I fancy we have created some kind of a record, though I don't know exactly what it is But we have no prejudices against professors In fact, the speaker I am about to introduce is himself a professor He is head of the Department of Music at Bennington College, and his name is Otto Luening He will speak to us about music

MR OTTO LUENING Mr Chairman, members of the Society, ladies and gentlemen, I'm sorry, but I must say at the beginning that we have no titles at Bennington College, and I am not a professor So the record stays clean

MUSIC

OTTO LUENING

Department of Music, Bennington College

(Read April 19, 1940, in Symposium on Characteristics of American Culture and Its Place in General Culture)

THE American musician until very recently has been a second-class citizen. In recounting the changes in our musical life it should be kept in mind that his aim has been to gain equal rights rather than to start a national movement, in the narrow sense of the word.

In describing the significant changes in American musical life during the past twenty years, I shall first present a brief catalog of events and then, with the help of piano and phonograph, give you a chance to hear at least a few examples of old and new American music. So active is our musical life right now that within my time limits many important names and happenings must be omitted.

The popularization of music in America began on a large scale with the silent movies. Through the movie orchestra, music selected from what was loosely known as the classical, semi-classical, popular and modern repertory was woven into a tonal crazy quilt to fit the picture on the screen. The orchestras were quite good—millions of people heard them every day.

With the coming of the talkies came the Hollywood composers, the men who wrote special scores for each picture. These, for the most part anonymous craftsmen, have succeeded in finding new sound effects and new orchestral devices. They occasionally write scores of real dramatic power, this even though much

of their music is imitative. Their compositions are heard by millions of people each day, although perhaps not consciously.

Through radio, music of all types and periods is made available to millions of listeners every day. The range of offerings goes from the NBC Super-super Orchestra—the “greatest orchestra in the world,” conducted by the “greatest conductor in the world” (Toscanini of course), playing the greatest music in the world—up to the Tennessee Mountaineer, singing a home-made tune to home-made words, on a home-made fiddle. Through commissions, prices and broadcasts, the radio has helped American composers to find their audience. It has shown us that some of the greatest music wears out if heard too often. Beethoven’s Fifth Symphony, for instance, has been so overplayed that it is rarely broadcast now. The radio has also shown us that unfamiliar idioms, if heard often, can be assimilated. Provincial audiences now listen to contemporary works with interest and some understanding. Phonograph recordings have served the same ends.

The Eastman School, the Curtis Institute and the Juilliard School began operating in the middle twenties. At about the same time the foundations, notably the Guggenheim and Carnegie, began making grants to composers, distributing record collections and generally assisting worthy musical enterprises, ever widening their influence and scope.

With this stimulus, music in the primary and secondary schools has by now developed to a point where our high school orchestras, bands and choruses compare favorably with those in any country.

In colleges and universities music is gradually becoming an integral part of the curriculum. Most American music students have been receiving their entire training in this country for the past eight or ten years. Formerly many of them went abroad.

Through the WPA Music Project we have had our first taste of state-supported music on a large scale—long since an accepted practice in Germany, Russia, Italy and France.

In the past twenty-five years the provincial symphony orchestra has developed. There are now more than two hundred of these.

American composition began coming into its own after the World War. Jazz, swing, Broadway musical comedies, revues

and ballads have exerted a telling influence on other types of musical composition at home and abroad. During the twenties the composers of serious music organized their own small performing and publication societies and later a recording society. At first considered experimenters, they have gained so much prestige during the past ten years that almost every musical organization has some American works in its repertory, even though frequency of performance, publication, recording and distribution have been lagging. Most American works are still in manuscript and not until this year has a music center been opened in New York to distribute published and recorded music by Americans.

New instruments have appeared—the Theremin, the Rhythmicon, the Ptolemy (it is an untempered harmonium), the Hammond Organ, other electrical instruments and stereophonic recording. To comfort those who are worried, Carl Sandburg, John Lomax, some musicologists and musicians and those who sing the songs, have discovered an American folk music of great variety and interest.

The big-time concert field, with its strange managerial setup, remains about the same as in the days when P. T. Barnum imported Jenny Lind as one of his "attractions." It concentrates mostly on music and musicians far distant from the source in space and time.

The music critics often spend much time discovering Brahms' First Symphony and Toscanini's genius as a conductor.

American conductors have not yet been fully accepted, probably because of the present managerial practices.

Grand Opera has remained about the same, but in the collaboration of modern dance, drama, and music the possibility of a new form has been suggested.

In sifting these events in a search for significant changes and contributions, the evidence suggests the following:

1. American popular music has made its influence felt here and abroad.

2. American trained singers and instrumentalists are developing a style of their own.

3. The integration of music into the school and college curriculum is producing not only a musically more literate and

sensitive audience, but also musicians who see their art in its relationship to their own time and environment.

4. American folk song has taken on an identity of its own. Even though many of the songs came originally from other countries, our time and environment have transformed them.

5. American composers, as a group, have arrived; their works compare favorably with new works from abroad. This has been said repeatedly by foreign conductors.

6. The picture is hopeful; there is much work left to be done, but time will probably show that we have with us now important creators and some works which will endure.

7. The democratization of music has really progressed.

8. American musicologists have helped us to discover early American music. I shall play a few examples and then one or two contemporary records.

Examples: Early American hymns by William Billings and Oliver Holden. Recordings by John Alden Carpenter ("Skyscrapers") and Robert McBride ("Fugato on a Well Known Theme")

MR KEPPEL Ladies and gentlemen, my introductions are over It would be sheer impertinence on my part to present Mr Arthur Compton to this audience or the audience to Mr Compton

MR ARTHUR H COMPTON Mr Chairman, fellow members of the American Philosophical Society, and guests I confess that it is with some trepidation that I approach the subject of what science does in shaping American culture or in adding to American culture, for I see about me two groups which represent the hopes and fears that American civilization holds in science The one group fears the tragedy that comes with misapplied science, another group sees with hope the possibilities and sees with courage the strength that has already been gained from science I fear that I shall not be able to do justice to either point of view, but I shall do what I can

SCIENCE SHAPING AMERICAN CULTURE

ARTHUR H COMPTON

Professor of Physics, University of Chicago

(Read April 19, 1940, Symposium on Characteristics of American Culture and Its Place in General Culture)

IN no other part of the world and at no previous time in history has life been so greatly influenced by science as in the United States today. This influence extends not only to the supplying of the means of living, but likewise to our thought, our amusements, our art, and our religion.

American civilization is based upon science and technology That civilization includes great cities, which need for their very existence mechanical transportation, steel rails and girders, electric elevators, refrigeration systems to preserve food, careful control of sanitation, and means of preventing the spread of communicable disease It embraces great areas of thinly populated but highly productive farm land. Here farmers live relatively complete lives, and supply the nation with an unparalleled abundance and variety of food, because of the agricultural knowledge and tools and convenient communication and transportation that science has supplied. With the help of science, labor and capital are efficient, the government coordinates the activities of a widely spread people, and our continent has become a national community.

American thinking is strongly influenced by science. Whereas at Oxford it remains doubtful whether science has yet earned

a true place in education, at Chicago three of the four main divisions of the University are called sciences. Of the older learned professions, the minister needs to pay close attention to science if he would retain the respect of his congregation; the lawyer who would deal with patents, or corporations, or even crime must acquaint himself with the rudiments of science, and as for the doctor, the more science the better. Most of the newer professions, such as engineering and architecture, are based upon science. A survey of current literature can leave no doubt but that in American society most of our creative thinking is in the field of science.

It is typical of contemporary American cultural life that good reproduction of the best paintings, and radio programs of the best music are available to nearly everyone. Here is an opportunity for widespread vicarious enjoyment of fine art and music. Yet the soul of art is in its individual expression. While the widespread use of color printing may seem to have discouraged the amateur painter, his place is perhaps taken by the amateur photographer, and the recent rapid growth of school orchestras and bands seems to be ascribable to the growing familiarity with orchestral music as heard over the radio. It is not impossible that use of the radio may mark the birth of a new era in American musical expression.

On the credit side of the ledger we can certainly count the introduction of new techniques in music and art. Among these may be mentioned the electric, "organ," which affords rich new tone possibilities, and photography and motion pictures. Though the possibilities in these directions are only beginning to be explored, it is already clear that in both still and moving pictures there are new fields opening for both the professional and the amateur. In particular, the possibility of adding action and sound to pictures is comparable in importance with the discovery of representing a third dimension in perspective drawing.

In our recreation we may try to live a primitive life. Having motored hundreds of miles over hard highways, we arrive at the cabin in the wild-wood, cook Chicago bacon on a stove using oil from Texas refined in New Jersey, and go fishing with an out-board motor made in Michigan. Or it may be that we go so completely native as to canoe down the river, relying only on our Pittsburgh steel axe and matches made in Ohio from Louisiana

sulphur to light our fires, fruit canned in California for our food, and mosquito netting woven in New England to keep off the pests. Though we want to be free from the ring of the telephone and to use the sun as our clock, we must take care that the milk we drink is pasteurized. Thus the American frees himself from technology!

Science makes men human In his recent book, "Science and the New Humanism," George Sarton shows how throughout history man's cultural growth has followed the gradual growth of his scientific knowledge. In art, except for new pigments, tools, and photographic technique, the American certainly does not excel the Greek nor hardly even the prehistoric European who painted lifelike animals on the walls of his cave. In music the Russian peasants and the natives of Hawaii give us lessons. It is Sarton's contention that those aspects of our culture which have been developing owe their growth primarily to the advance of scientific knowledge. Thus by learning more and more about the world in which he lives, man has distinguished himself from his animal cousins. If this claim is valid, it means that the primary responsibility for humanizing man lies with science, and that the society in which scientific knowledge is most rapidly growing is the spear-point of man's advancing culture.

Let us then examine Sarton's argument more closely. He points out that each stage has been ushered in as some inquirer, more persistent or more fortunate than his predecessors, and building on the foundation of their techniques, has learned new facts regarding the properties of matter, the chemistry of metals, or the laws of mechanics. Thus when we speak of the stone age, the bronze age, the iron age and the machine age, we are summarizing the growth of man in terms of the tools with which he does his work. Not that mechanical inventions are the only ones. Language and writing are among the most significant inventions of all, giving as they do means of thinking more clearly, of communicating ideas, and of remembering ideas with definiteness. When the invention of printing, telegraphy, the telephone, moving pictures, and the radio are added, it becomes possible for people to share thoughts widely, to become quickly aware of what is happening to all mankind, and to "remember" what has happened to men in the past. A great change thus comes in men's attitudes toward each other. The

world becomes almost a conscious unit, very similar to a living organism. Thus even the non-mechanical inventions have found their most effective application through the aid of scientific developments.

Hand in hand with this development of invention has gone the increase in our knowledge of nature. Skilfully made lenses made possible a telescope, and Jupiter was found to be a miniature solar system. As high vacuum pumps were developed, x-rays were discovered, giving new knowledge of the structure of matter, with resulting advances in metallurgy. "If I saw farther, 'twas because I stood on giant shoulders," is the statement ascribed to Isaac Newton, who clearly recognized the way in which one advance makes possible another.

The knowledge of nature, which from the beginning had been man's gradually but accidentally increasing heritage, at length became the conscious objective of alert minds. Three centuries ago the hobby of a few amateurs, there are now in the United States nearly two thousand research laboratories, equipped with refined apparatus, where men of the highest training are striving to enlarge our understanding of the world. As a result, our life differs from that of two generations ago more than American life of that day differed from the civilized life at the dawn of written history.

The growing rate of this increase in knowledge and of the resulting social changes may be strikingly presented by using the historian's device of compressing the time scale until the whole growth of man through a million years is concentrated within the lifetime of a middle-aged man of fifty. It was then as a child that our man was learning how to use certain odd-shaped sticks and stones as tools. The meaning of sounds became definite as he learned to talk. By the time he was forty he had developed the art of skilfully shaping stones to fit his needs. Man soon became an artist, and by half a year ago had learned to use simplified pictures as symbolic writing. Some six weeks ago the Phoenicians introduced the alphabet, and within a fortnight came the brilliant art and science of ancient Greece. Then came the fall of Rome, hiding for some weeks the values of civilized life. Less than a week ago, as the report has it, Galileo dropped the heavy and the light cannon balls from the Leaning Tower of Pisa, refuting a proposition of Aristotle and starting

the period of modern science. Three or four days ago the first practical steam engine was built and it was at about this time that the United States came into being. Day before yesterday the laws of electromagnetism became known, which by yesterday had given us the telegraph, the telephone and incandescent electric light. Only last night x-rays were discovered, followed quickly by radium and wireless telegraphy. It was this morning that automobiles came into general use. Air mail began to be carried only at noon today. Popular short wave broadcasts, practical color photography, and fluorescent lighting have been with us for only an hour. It is clear that our American scene is staged in the midst of a period of unparalleled advance in science and rapidity of social change.

American culture is that of a changing society. Even before the outbreak of the present wars, America had become the leader in most fields of scientific endeavor. The tradition of the pioneer has made it relatively easy for the American to alter his habits as required by the introduction of new techniques, with the result that in this country social changes have gone ahead with a speed not found elsewhere. Our culture is thus that of a new community, with our customs and ideas only partly adapted to the rapidly changing conditions of life.

For a week I have been living in an apartment on a corner by which a street car clangs its noisy course. When first installed, these cars gave the rapid transportation that made the city possible. Now the demand is insistent that the street cars be replaced by quieter buses that will permit conversation by day and sleep by night. Thus the first application of technology was to meet the primary need of transportation; but eventually the refinements come that add to life's enjoyment.

Our older habits no longer fit the new conditions of life, and we have not yet learned how best to use the new possibilities placed at our disposal. Nor as long as such rapid changes in our social life continue can we hope to make a completely satisfactory adaptation of our mode of life. For as one aspect of the problem becomes solved, changes will lead to maladjustment somewhere else. It would for this reason be futile to hope to attain within the next generation an art of living in a technological world that can compare in refinement with the classic culture initiated by the Greeks and developed through centuries.

of such tradition as that carried on by European and English society. In course of time, though it may require centuries, we may expect the development of science to approach a new plateau of knowledge and invention. Then we may hope again to refine our mode of living to fit precisely the conditions of our greater world.

Does this prospect of generations of incomplete adaptation, with resultant discontent and hardship seem discouraging? One is reminded of the legend in which the people complain to Daedalus that the steel sword he has given to King Minas will bring not happiness but strife. Daedalus replies, "I do not care to make man happy, but to make him great." For those who have courage, the new powers thus given by science present a challenge to shape man's life on a more heroic scale. Here is a vision of a new world which only the brave may enter.

Yet we can thus appreciate the dread felt by those who have followed the tradition of classic culture as the life they have loved and whose values they have cherished is threatened by the advance of technology. They see science replacing the human interests present in literature, art and music with technological developments in which the human factor becomes less and less significant. The most fundamental values of morality and religion are ruthlessly shaken, with the implication that their value is negligible. It is just because so many scientific men seem blind to these human difficulties that one feels the greater concern lest in following science mankind may lose its soul.

There is a passage in Plato's *Phaedo* in which Socrates describes his early interest in physics and how he had found that physics fails to account for the important things in life. Thus, he explained, Anaxagoras would say that Socrates sat on his cot waiting to drink the hemlock because of certain tensions of tendons acting on his bones. The true reason was rather because he had been condemned by the people of Athens, and as a man of honor he could not creep stealthily away. Such moral forces as honor were not to be explained by science; yet it is these forces that shape men's acts. Since it did not meet their human needs, the followers of Socrates and Plato abandoned science, and the study of the truths of nature was forgotten for a thousand years.

We have now once more come to fear the unhuman implications and the inhuman abuses of science. Yet science has enriched our lives and has helped us catch a vision of a new and better world. Shall we then again give up science and with it the tools by means of which that better world may be attained?

The truth is that we cannot cast away science even if we would. In a time of intense social strife the knowledge of the world that we call science is a source of tremendous strength. Nothing is so clear as that a nation which abandons science must soon become weakened. The world's leadership must go to those who are served by science and technology. That we shall live with science is thus decreed by the immutable laws of evolution.

The Human Meaning of Science.—For those who know science, its inhumanness is a fiction. It serves to satisfy the human hunger for a better understanding of man's place in his world. In this age when men throughout the world are trying to formulate a philosophy by which they can live, it is to science that they are turning with confidence in its truth. But perhaps of greatest importance is the fact that science is making man develop into a social being.

One of the most striking of biological phenomena is the change of man in a short thousand generations from an individualistic to a social animal. As has been indicated above, this change is due largely to the development of science and technology. If we would assess the cultural significance of science, it is thus important to consider what the more specific directions may be along which this social evolution will proceed. It is clear that we may expect those modifications in our mode of life to survive which give strength to the social group. Among these strengthening factors three may be emphasized. These are: knowledge, cooperation, and a common objective.

In science and technology lies our approach to the laws of the world of nature and the application of these laws. Enough has been said regarding the strength that comes through such knowledge. In a highly competitive, war-like world, that society cannot long survive which neglects the truths of science.

Without cooperation, knowledge cannot be made effective. If men divide into antagonistic groups, it becomes terribly destructive. Experience as well as theory has shown the superior

strength of those social groups which work together. The evolutionist thus sees as inevitable the growth of social cooperation.

Just as the automobile demands sobriety, or congested life makes necessary careful sanitation, so the mutual dependence of a technological civilization implies consideration of the rights of others. Breasted has shown how the growth of community life along the Nile stimulated among the Egyptians the "dawn of conscience." Cheyney, in his retiring presidential address before the American Historical Association, lists prominently among his "laws" of history the trend toward a greater consideration of one's fellows as society grows more complex. Thus in the technological society of which American culture is a supreme example, science and industry are emphasizing as never before the need of the will toward cooperation, that is, of the love of our neighbors. Perhaps the urgency of the universal acceptance of this central doctrine of Christianity is not generally recognized. This is merely because the social implications of our increasingly complex life have not yet become evident within the brief decades of the world's growing social unity.

Most significant of the factors that give strength to man is, however, the vision of a goal which he recognizes as worthy of his supreme effort. If we would truly live, we need a purpose. To many of its followers, science gives a basis for the appreciation of man's place in the universe. It helps him to see himself as he is, a creature with animal limitations, but with godlike powers, sharing with his Creator the responsibility for making this world a fit place for life. The man of science may not feel qualified to choose for others that which gives life dignity and worth, but he can at least supply the data on which that choice must be made. How can we correctly orient ourselves without learning the facts about the world and dispassionately considering their implications. It is, I believe, in just this direction that science must ultimately make its greatest human contribution. Science must clarify the vision of the seers who would point out to us the goal of life.

It is noteworthy that these things which give strength to society are likewise those that make life worthwhile, the understanding of man and nature, the love of one's neighbor with the acceptance of responsibility for his welfare, the finding of a goal

worthy of our best efforts. Though American technological civilization may lack the refinements and nice adjustments which perfected the classic culture, its growth is toward the greater social development of man. In this sense it is truly humanistic.

The rôle of science in American culture is thus threefold. First, it supplies more adequate means of life, giving men longer life, better health and a richer variety of experience. Second, it stimulates man's social growth by rewarding more abundantly cooperative effort and punishing more severely his antagonisms. Third, science serves as a direct means of expression of the human spirit.

It was the greater variety of life that was the great reward of science seen by Francis Bacon as he wrote in his "New Atlantis":

"The end of our society is the knowledge of causes, and the secret motions of things, and the enlarging of the bounds of human empire to the effecting of all things possible "

After three and a half centuries of experience with modern science this aim has been so realized that the president of one of our leading technical institutes can say,

"In the last fifty years physics has exerted a more powerful beneficial influence on the intellectual, economic, and social life of the world than has been exerted in a comparable time by any other agency in history "

It is its responsibility for man's social evolution which leads Sarton to describe the growth of science as the central thread along which may be traced the biography of mankind.

To the man of science himself, however, it is as an effective method of developing the human spirit that he values his science. His study affords exercise of imagination and broadening of perspective. Whereas to Plotinus it appears that:

"It is through intuition rather than through reason that we may approach our highest aspirations,"

the scientist finds that in the discipline of unprejudiced search for truth lies the beginning of wisdom. Thus, in the words of Thomas Huxley:

"Science seems to me to teach in the highest and strongest manner the great truth which is embodied in the Christian conception of entire surrender to the will of God. ' Sit down before a fact as a little child, be

prepared to give up every pre-conceived notion, follow humbly wherever and to whatever abysses nature leads, or you shall learn nothing "

This is the aspect of science recognized by the Greek philosophers, who would seek "of what and how the world is made" in order that they might find a better way of life. To a certain degree this humanizing aspect of science is esoteric, since it can be fully appreciated only by those who have themselves submitted to the discipline required to share in the effort to widen the horizons of knowledge. Certain aspects of science, notably astronomy, have been more effective than others in opening the way for many amateurs to take part in their enterprise. As in art and literature, here in advancing human understanding is an opportunity for enriching life. With finding new knowledge comes the satisfaction of knowing that one has not only made a permanent addition to man's heritage, but that the new knowledge is a seed that will grow from more to more. With Democritus the scientist can truly say, "I would rather learn the true cause of one fact than become King of the Persians "

MR KEPPEL. Mr Brooks, do you want to stand up here as the leader of the discussion? Would you like to continue your functions, not as a man of letters, but as a leader of discussion?

MR BROOKS. Well, so many things have been discussed that it's very hard to make a synopsis of it. I wonder if we could have some questions? Mr Chinard, I think, has a question.

MR GILBERT CHINARD. I have a question, yes. While I was listening to Mr Compton, I thought of the many regions over which science extends today, regions which in the past I had thought practically immune to the invasion of science. Then I thought that the only thing I had to do was to hand over the whole universe and the whole realm of knowledge to science.

Quite seriously, though, I don't like to seem inquisitive, but while I was listening to his speech, there was a constant query in my mind. We heard about science and then about technology. Sincerely, I would like very much to know whether the two words are practically synonymous. I have always had the highest respect for what in my youth was called "science." But I don't profess to say the same for all the branches of technology.

MR COMPTON. The scientist has no desire to disown his child—technology. Certainly, the two are closely related, but I suppose the true relationship between science and the technology associated with science is clear in that technology refers to the application of science toward practical ends. So that if we think of the social implications or the cultural implications of science, we find them twofold. One, the intellectual implications of science itself, and the second, the social implications of the applications of science. If one is speaking for science, one cannot neglect the very important implications of the applications that have been made of scientific ideas. It is well known, of course, that there is a distinction between the attitude of the early Greek scientist and the modern scientist. To the Greek scientist the applications—which we now would call "technology"—played a very minor part. For example, to the early philosophers up to the fifth and fourth centuries B.C., it took practically no part at all. That would be at the time of Archimedes. Archimedes did apply his science, but the thing he was proud of was the intellectual aspects of his science. And the records that we have of applications of science—of technology—are all secondhand. And we find that throughout the other developments of the Greek and Roman period the scientific men and the philosophers have no desire to claim credit for technological applications, but rather consider themselves as philosophers whose objective it is to study the values of life without considering the applications of their science. But the idea is that the present applications of science with all our technology are the natural outgrowth of the objectives that the early scientific men had as they introduced the modern period of science.

The quotation that I gave from Francis Bacon, when he speaks of the science that is to be taught in his "New Atlantis," is really representa-

tive of the spirit of the age. He says that he looks to this new science to enlarge the bounds of the human empire, and he goes on to describe what that means. He speaks of the new types of agricultural development and of mechanical development and so on, that will come as a result.

And it is only too clear the way in which those technical applications have arisen. There are certain aspects of science, nevertheless, which still remain essentially of the old Greek pattern—astronomy would be one of those. Astronomy still has its primary reason for existence in the broadening of man's horizons. And many of us feel that other aspects of science—physics, biology, chemistry, and the rest—have their greatest interest in their technical implications and their philosophic significance. In that regard, we might treat our science as philosophy, but it is not by any means its only application to the present human culture. These implications of technology are very real, and it is the technological implications that have been primarily responsible for this social development—the force toward making man more of a social being. We owe that really, I should suppose, to the technological implications of science rather than the philosophical implications.

MR. CHINARD. I am not satisfied yet, but I am less discouraged, because I begin to see I have an idea of the essential difference.

MR. KEPPEL. You've lived under two flags, and in my discussions with Frenchmen two words almost always come into the discussion at one point or another, and those two words are *la culture générale*.

We've said a good deal about American culture in spots—all sorts of interesting spots—now would you as a Frenchman venture an opinion as to where we are getting?

MR. CHINARD. I don't think I would. I would rather engage in a private fight with Mr. Compton. But I was just about to say, Mr. Chairman, that I still believe there is some confusion among our scientific friends and that you are assuming a responsibility which is not theirs.

To take a specific instance. I don't doubt for a minute that the radio is due to science and all the developments of the radio are due to science, but the man who bears the responsibility for the use of the radio is the man who makes up the programs, not the scientist who builds the tubes. They are two entirely different realms, and Mr. Compton as a scientist who invented the radio has no social responsibility whatsoever. Scientists have put some tools at the disposal of the people who are using them for good or for evil, but theirs is not the responsibility.

MR. E. P. CHEYNEY. I was just going to state one other reaction. During Mr. Compton's speech I couldn't help wondering why in a scientific age, where science and technology have been dominant, it seems to have made such a poor job of it. We look on the world with dismay at the present time. We have unemployment, and we have the horrors of war, and we have along with these, other evils that one might mention, and yet they come after a generation of the dominance of science. In the scientific spirit, I have the greatest sympathy with technology.

It has led us to live in great cities, but it has also created the great cities and all the problems that lie within them

I should like to see that as one side of a debate Mr Compton showed the affirmative side for science, but there has been a long list of evils that have accompanied this period that needs to be accounted for

MR KARL COMPTON While this discussion has been going on, Mr Chairman, I couldn't help thinking of a story I was told by Professor Frank Fetter I believe Mr. Fetter is here so it might be dangerous to tell it, but here it is anyway.

Some years ago, there was a discussion in the faculty meeting at Princeton as to who was responsible for accomplishing a certain thing, and Professor Fetter got up and told this story That as a boy he used to pump the organ at the church, and there was always a great dispute between him and the organist as to which one of them made the music This dispute went on for some time until one day it occurred to him that he could prove his point So Mr. Fetter stopped pumping the organ He proved his point, but it lost him his job. And I think this discussion here as to who is responsible for the radio may be held in somewhat the same light.

In regard to the features that have been brought in by technology, of course we all realize that there are many of them I think sometimes the perspective becomes a little warped, because we are more likely to be struck by the things that we are concerned about and that worry us than we are about the things that are on the other side of the ledger We are more likely to take those for granted

A few years ago, I was sitting at a dinner at which the national awards were being made to nineteen men in this country for having made very significant inventions during the past twenty-five years In that group there was only one whose invention, so far as I could see in looking over the list, had anything to do with anything that could be criticized by a social scientist, and that was Orville Wright, one of the inventors of the airplane Such things as the method of chlorination of water, the development of the x-ray tube, and the incandescent lamp, I could see no particular harm in.

The only thing that I would say, Mr Chairman, is that I think sometimes in our concern over these troubles we somehow lose perspective. I would say one thing more, though, and I think that my brother brought out this It may be that this very magnitude of the problems of science will be a thing that will make for security

I am thinking now of the matter of war and peace. As long as nations were completely isolated from one another, it made no difference what went on in one or the other But the greater the solidarity between nations, the more important it becomes to find means of settling aggressions And I think it is possible—I think many people believe it would be possible right now—if people could only be brought together by the seriousness of the present situation, to set up a scheme for the abolishment of war, that would probably come far closer to doing away with war than anything that has been done in the past.

MR. KLEPPEL If Mr MacLeish, as a poet and not as a Penrose Memorial Lecturer, would help us out in this dilemma, we would appreciate it

MR. MACLEISH I find it very hard to answer As a Penrose Memorial Lecturer, I want to talk about this very much We have talked a little about mutual responsibilities The humanist has felt that some of the responsibilities that ought to be his have been taken by Mr Compton But, friends, I think the question concerns the allocation of the irresponsibilities That's what I would like to talk about tonight, so I'd rather not talk about it now

MR. W. J. HUMPHREYS I would like to butt into this discussion I have been very much impressed in listening to the delightful talks this morning and this afternoon by the ephemeral outlooks of the speakers All of them, except Mr Compton, seemed to take for granted that the present conditions are of indefinite duration Our industrial civilization lives on capital, not on income Everything that comes out of the ground—not things from living creatures, a living plant or a living animal—but everything that is taken from the mines or the quarries or the oil wells is lost—irreplaceably lost And the time is bound to come—it isn't a matter of opinion—it's certainty—when our metals, minerals and fuels will be exhausted We shall have no coal and no oil We are like a body of excursionists floating on the Niagara River, and the cataract is ahead of us And it behooves us to prepare for that time when the boat reaches the edge of the abyss We must go ashore and learn to change our whole outlook, our whole mode of life It will take a long time but that doesn't bother us.

You remember the story of the old lady who heard that in thirty thousand years Niagara Falls would have disappeared under the waters of Lake Erie, and she said, "Well, then I must go and see it!"

It is something like that so far as we personally and our immediate descendants are concerned There is nothing to be afraid of now But the history of this country, industrially speaking, has been one of ghastly waste. The amount of exploitation of foolish waste of resources is perfectly heartbreaking, although it won't make much difference in the long run The automobile is dependent absolutely on a few rare metals like vanadium and selenium, because the alloys of these metals make possible a vehicle that shall be sufficiently strong without being too heavy And yet look at the millions of cars that are turned out to rust and waste every year Now that kind of thing can't keep up forever It behooves us to look out for the time when we've got to go ashore or else go over the cataract

MR. MEES I was going to make a much less apoplectic statement. I was merely going to support Mr. Chinard's view that a scientist is not entirely responsible for the use that is made of the tools he has provided for society You see I have a practical interest in it My business is making film, and I should hate to be responsible for what they do with it.

MR. HUNTINGTON As a resident of Massachusetts, I should like to add just one footnote to Mr. Luening's address on the subject of music

The efforts that are being made to start an institute for music in the Berkshire Hills in the summer seems to me especially striking and successful. And I should like to say a word in appreciation to everyone in every state of the Union who welcomes what is being done. The institute is going into its third or fourth year now and is filled up this year.

MR H S HETTINGER I wonder if I may add a question to the discussion—one that has been bothering me. I have been wondering (I have been watching some of the arts rather intimately recently) whether or not we have been fully aware, not only of the close interdependence which must exist between science on the one hand and the fine arts on the other, but also between the social sciences and the arts.

Let us take music as an example. We have had in this country a most remarkable growth of music in recent years. That growth, to take one phase, has produced between two and three hundred symphony orchestras of varying qualities, but all of them rather interesting. You find all kinds of people playing in them. You will find a local factory man playing beside the local hardware salesman, these two playing beside the local lawyer and some of the high school students, and so on, and doing a rather competent and highly interesting job, providing the community with something they wouldn't have otherwise. And when you look at the country as a whole, you become impressed, and probably if you look at it too closely, appalled to what an extent this development may depend in its ultimate success, socially and culturally, upon the economic and social organization which supports the artistic development.

For instance, if you really want to run a small orchestra in a business-like manner—and I am not thinking of balancing the budget, but merely of planning its concerts, its activities, its repertoire and making it interesting to the community, getting its support in a way that will bring it to realize the potential value of that orchestra,—at the present time there is no body of knowledge, no particular source of advice for the people of those communities to go to for planning their work in the best possible manner.

We might call this lack of social economic development to keep abreast with the times, but this lack may have certain effects upon the development of music, and on some other things it may have the opposite effect.

One could go out of the field of music and into the field of arts, which, if we are going to have any large number of practicing artists, means that there must be a broad base which must be supported somehow. And that immediately gets us into the economic question of selling the products of those artists in sufficient quantity so that they can support themselves and develop for themselves other higher skills. Either that, or we must look to the amateur side of most of our arts as the principal place from which these higher skills may be derived. I am not sure what the answer is to this but it is a very fair question.

MR. KEPPEL And I think you may say this. That little by little and step by step, various agencies or a kind of clearing house, a freemasonry—I think it is already very true with the museums today—will arise. No museum is too high and powerful to lend a hand to another. Consider the public library movement. The economics of a public library can be learned and applied to almost any city. And my guess is that instrumental music will probably find its way; maybe not its perfect way, but it will be far more advanced in ten years.

It was great fun to watch the way the museum followed the public library—one after another—about a half a generation behind. And the other cultural agencies, I am sure, will be able to help themselves and one another more than they know how to do at present.

Mr. Brooks, are there now any spots that you'd like to have cleared up?

MR. BROOKS I think not.

PRESIDENT MORRIS Before you adjourn, I would like to have an opportunity to say a word about our program. I think you have been a perfect audience here today, and I know I have been greatly rewarded by this discussion. Again I want to thank those who spoke and the organizer of this symposium who presided, for giving us this opportunity to have a discussion of this kind.

We have been here, first at a business meeting and then at this discussion, for many hours, and I am sure we are all going away with a touch of inspiration and a deeper realization of some of the problems of our own national life than we have had before. I like to hope for the future and for the attitude of our own people throughout the United States, in spite of the bewildering and dreadful conditions that exist in so many parts of the world.

GROWTH IN *AESHNA TUBERCULIFERA* (ODONATA)

ELSIE LINCOLN

(Communicated by Philip P. Calvert)

ABSTRACT

Four larvae which hatched, on the same day, from eggs of one female *Aeshna tuberculifera* E. M. Walker, were reared side by side under similar temperature, light and food conditions. One individual transformed in 492 days after 13 moults, one in 499 days after 15 moults, two individuals died in the fifteenth instar, one after 441 days and the other after 482 days. Measurements were made of 86 parts of each exuvia of each individual and the growth rates were computed. Those parts to be functional in the adult (wings, cercoids, and gonapophyses) demonstrated higher rates. Results indicate (1) there is a marked variation in the number and length of instars in different individuals, (2) no constant growth rate for this species was observed (it was found to vary as much as 1.04 to 2.80 in one moult); (3) some gain in length occurs between moults, (4) in spite of the heterogonic growth demonstrated by these individuals, there is a similar pattern or rhythm of growth unique for the individual and discernible in all parts.

This study was undertaken in an attempt to determine the exact number and length of the instars and the amount of growth accomplished between successive ecdyses of larvae of *Aeshna tuberculifera* E. M. Walker, reared from the egg under similar food, temperature and light conditions.

The original adults and their eggs were collected and all the larvae reared by Dr. Philip P. Calvert, who very kindly turned over to me the exuviae and rearing notes. I am deeply grateful to him for suggesting the problem, for the material, and for all his many helpful suggestions.

MATERIALS AND METHODS

Aeshna tuberculifera is described by Walker (1914) as a rare species which, however, is common at Lonely Lake, Vancouver Island, British Columbia. Never before reported further south than northern New York State, ten males and two females were taken between September 8 and 27, 1935, near a small pond (which is often drained during the winter months) in Cheyney, Pennsylvania. One male and one female were taken pairing on September 15 and the female was induced to lay eggs in the leaf of a cattail (*Typha*) the following day. The leaf was kept in water at room temperature throughout the winter. Eighty-five larvae hatched from these eggs between March 13 and April 5, 1936. Of the 33

which were kept for rearing, 21 died in the first instar, 5 in the second, 1 in the fourth (No. 32), 2 in the fifth (No. 31, No. 30), 1 died in the fifteenth (No. 9), 1 died while transforming in the fifteenth (No. 17), and 2 transformed (No. 3, No. 15). The two which matured and the two which died in the fifteenth instar all hatched from the egg on March 22, 1936.

The larvae which hatched were placed in pond water in caster cups, transferred to larger containers at approximately the sixth instar and again changed to battery jars in the last stages. Aquatic plants, such as *Lemna* or *Elodea*, were always present in the water. An adequate amount of food was constantly supplied.

The exuviae were removed as soon after ecdysis as they were noted and preserved in 70 per cent alcohol. The labia of all stages and the entire early exuviae were cleared, stained, and mounted in balsam on depression slides. The later stages were measured in alcohol in a Syracuse watch glass. Measurements were made with an ocular micrometer in a binocular (ocular 8 \times , objectives 2 \times and 4 \times) and in a Leitz compound microscope (ocular 4 \times , objectives 3 \times and 7 \times) for the finer details.

Measurements were made of each exuvia of each of the four larvae No. 3, No. 9, No. 15, and No. 17, of the following parts, total length, lengths of head, fore margin of the hind wing, cerci, cercoids, superior appendage, gonapophyses, ovipositor, tarsal joints of all three pairs of legs, hind femur, labium, labial palp, labial spine, primary ligular cleft, maxilla, mandible, antero-posterior diameter of the eye, antennal segments, greatest width (7th abdominal segment), widths of head, labium (mentum)—base and maximum width, labial spine, labrum, mandible. From these measurements the growth rates or progression factors for each part were computed after the method of Brooks, by dividing the length (or width) of a given part at one stage by the length (or width) of the preceding stage. These measurements are set down in detail in four tables designated as numbers 2-5, included in a paper bearing the title *Some Observations on Growth in Aeshna tuberculifera*, deposited in the library of the University of Pennsylvania, where it will be accessible to investigators.

OBSERVATIONS

Larva No. 3 transformed in 499 days after having made 15 moults and after having passed the winter months, from September 17 to April 14-18, in the eleventh instar. Larva No. 15, which

moulted 13 times, transformed in 492 days after having passed the winter months, from September 29 to April 18-21, in the tenth instar. The winter, from September 14 to March 14-18, was passed by the twelfth instar of larva No. 9 which died after 441 days in its fifteenth instar. Larva No. 17, which also died while attempting to transform in its fifteenth instar (after 482 days), spent the winter months from August 25 to March 7-10, in its eleventh instar.

The irregularity in duration and number of instars is readily observed. The third and fourth instars were the shortest, except in larva No. 15 where each was at least twice as long as the fifth stage. Both No. 3 and No. 15 transformed, but the latter went through 13 instars whereas the former moulted 15 times. None of the larvae moulted from September 29 to March 7-10. It is interesting to note that the winter rest was longer in the two individuals which transformed.

All these larvae were raised side by side at a north window in a moderately heated room. The fairly constant temperature which prevailed, is, of course, not the normal condition. However, these larvae, which hatched at the same time and were raised under similar food, temperature and light conditions, demonstrate a wide variation in the number of instars and the length of each instar (Fig. 1).

There is likewise much variation in the dimensions in the same instar in the four individuals. For example, the total length in the ninth instar is 18.75 mm. (No. 3), 14.38 (No. 9), 22.24 (No. 15), and 17.44 (No. 17). The amount of growth from moult to moult differs in the same individual and in different parts in the same instar, so that for example, the growth rate between the seventh and eighth moults of No. 15, exclusive of the wings, cercoids and gonapophyses, varies from 1.04 to 1.44. The three parts just mentioned, however, have higher rates ranging from 1.25 to 2.80.

Due to the variability in the different individuals, it is difficult to state in which instar a morphologic change took place. For example, whereas the fifth antennal segment developed in the sixth instar in No. 3, No. 9, and probably No. 17, it was differentiated in the fourth instar in No. 15. The only change which seems to have developed in the same instar in all four individuals is the curve of the mentum of the labium.

The spine on the sixth abdominal segment, which is a generic character for *Aeshna*, is present in the first instar. There is only one tooth on the inner lateral margin of the labial palp and from 11

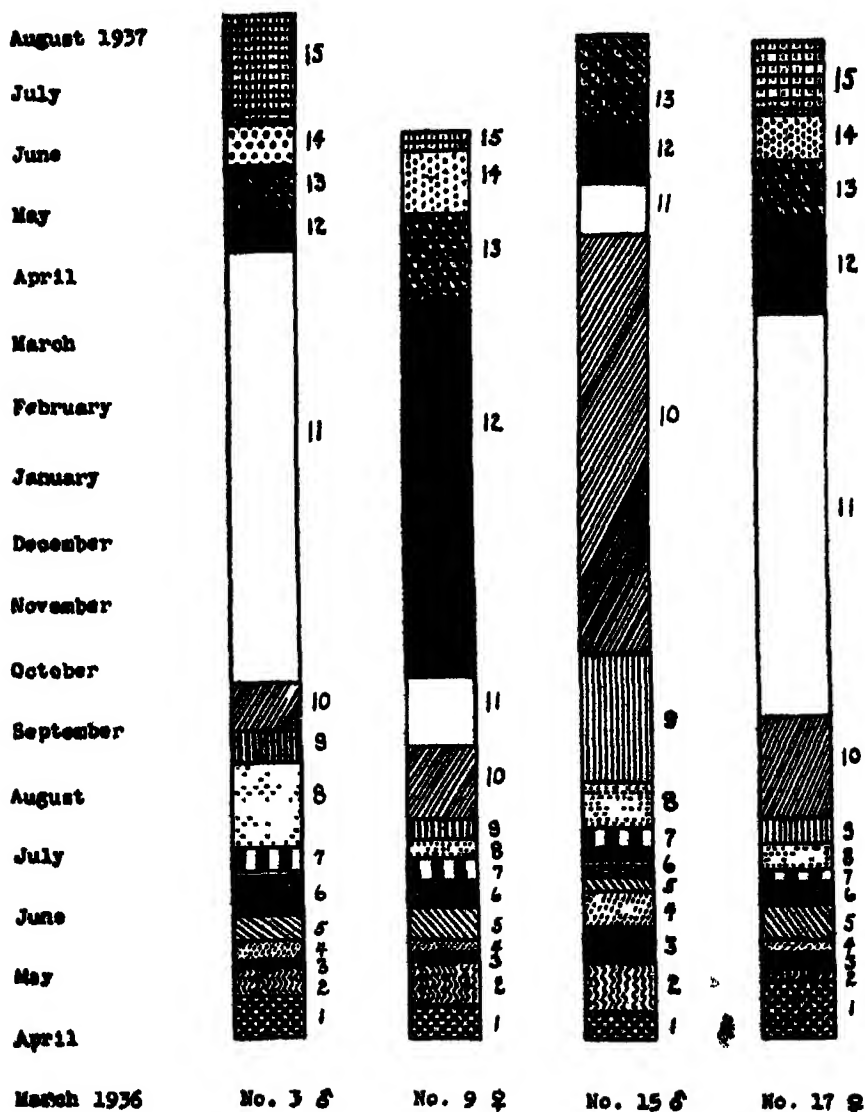


FIG 1 This figure illustrates the number of instars and the length of each instar of larvae No. 3, No. 9, No. 15, and No. 17. The winter months were spent by No. 3 and No. 17 in the eleventh instar, by No. 9 in the twelfth and by No. 15 in the tenth. The dates of the shedding of the exuviae terminating each instar of each larva are given in Table 2.

to 14 on the apical margin, in the first instar. The number gradually increases on the lateral margin so that in the last instar there are from 18 to 21, whereas on the apical margin there are still from 11 to 14.

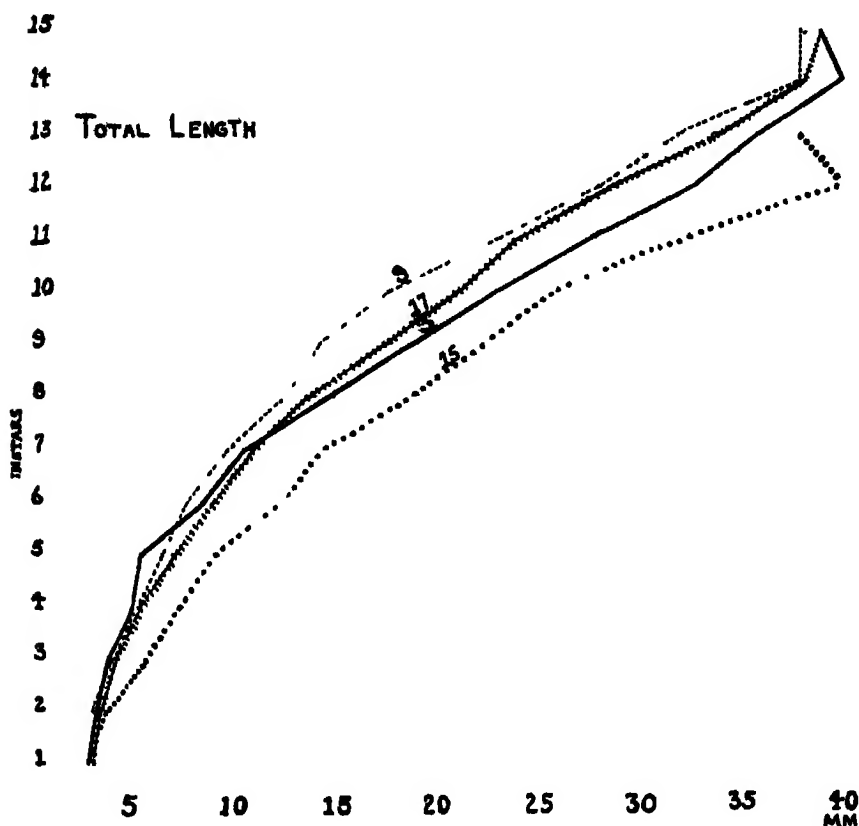


FIG 2

FIGS 2 TO 4 Figure 2 illustrates the total length growth curve for the four larvae; Fig 3 demonstrates the growth curve for the width of the head and Fig 4 the curve for the length of the labium. A comparison of these three curves indicates a fundamental rhythm of growth in each part of the same individual (with certain variations)

Nine graphs illustrating the growth in total length, in the width of the head, length of the fore margin of the hind wings, length of the cercoids, cerci, hind femur, labium, primary ligular cleft, and the width of the labium of the four individuals are also included in the paper above mentioned as deposited at the University of Pennsylvania. Three of these are reproduced herewith as Figs. 2-4.

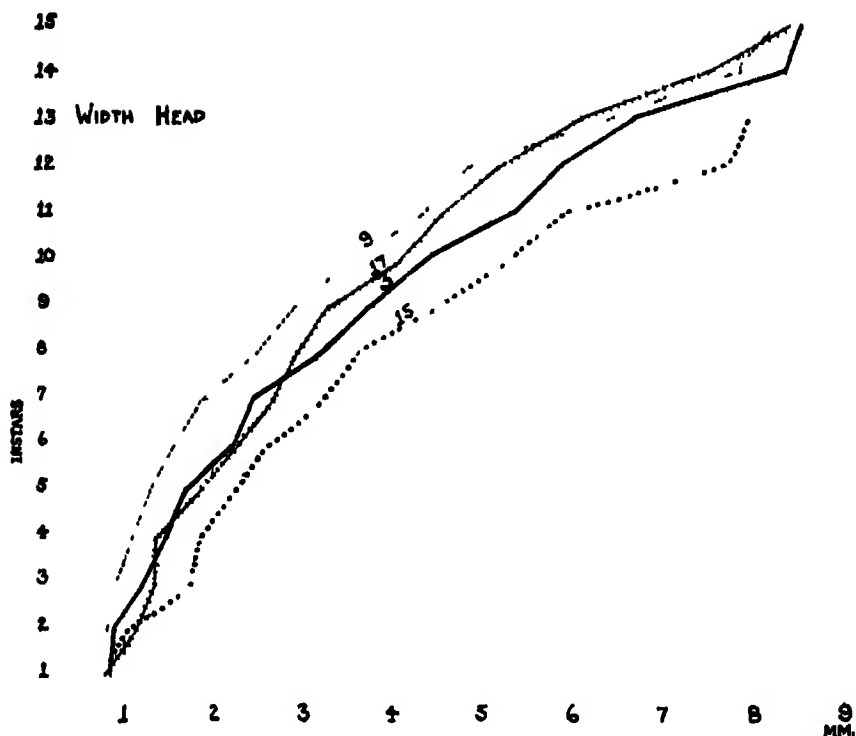


FIG 3

A comparison of these three charts illustrates (1) that the amount of growth accomplished in a given instar is variable, (2) that each larva demonstrates a unique rhythm of growth, (3) that the rhythm of growth for the whole is perceptible in the pattern of growth of each part (with certain variations).

DISCUSSION

1. Principle of Przibram and Megusar

From their work on the exuviae of *Sphodromantis bioculata*, the Egyptian preying mantis, Przibram and Megusar (1914) found a successive doubling in weight of exuviae and of the insect, an increase in length on the average of $\sqrt[3]{2}$ or 1.26 and an increase in areas of $\sqrt{2}$ or 1.41. This sequence of the biological stages after the fashion of a geometric progression of which the exponent is 2 was believed by these authors to correspond to a division of the cells; there is, however, considerable variation in their data.

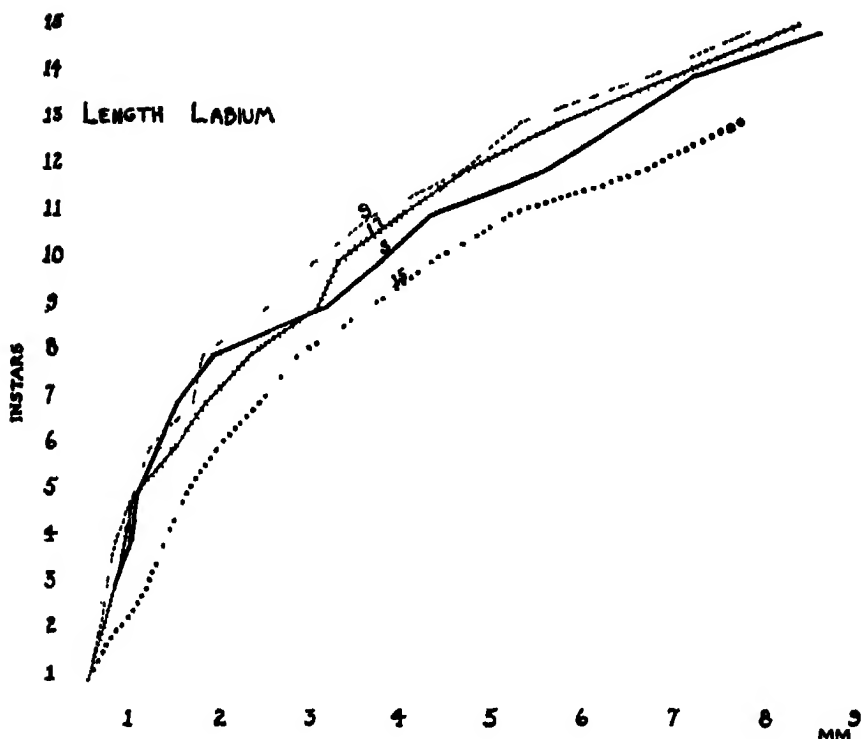


FIG 4

Many others have discussed this Przibram principle. Ludwig (1932, 1934) stated that his work on the Japanese beetle did not substantiate Przibram's principle. Ludwig's results have been further corroborated by Abercrombie (1936) who made measurements and weighings in conjunction with cell counts and found that "the progression in weight and length cannot be correlated with an increase in cell number . . . The increase in size of the larva is largely due to an increase in cell size".

In this study of *Aeshna tuberculifera*, although many of the growth rates hover about the theoretical ratio 1.26, the great variation from instar to instar and of different parts within the same instar force us to conclude that this theory of Przibram and Megusar cannot be substantiated by our results and to agree with Teissier that although this progression factor has a statistical value, it does not explain the method of growth.

2. High Percentile Rate of Growth in Youth

Many investigators have described a higher percentile rate of growth in the earlier stages of post-embryonic development. This has been done for animals in general by deBeer (1924) who stated, "The most striking thing about the curve (of the percentage additions in weight during a given duration time, plotted against time) is that the organism grows fastest when it is very young, and that the percentage additions become less and less as it grows older". Brody (1927) and Child (1915) also indicate this principle for animals in general. Brody divides growth into two phases, an early "self accelerating" phase and a later "self inhibitory" one. Child claims "The rate of growth (in animals generally) also shows a decrease from early stages of development onward, although in many cases periodic or occasional increases in rate of greater or less magnitude occur".

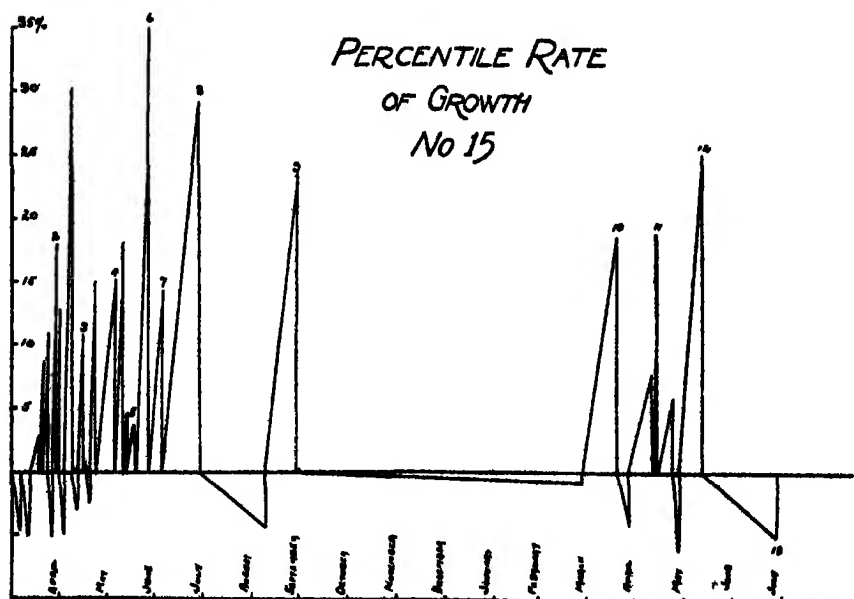


FIG. 5 This graph illustrates the percentage rate of growth of *Aeshna tuberculifera* No 15 at and between successive ecdyses.

The earlier higher percentage rate has also been demonstrated by Nelson, Sturtevant and Lineburg (1924) in the honey bee, by Titschack (1924) in *Carausius morosus* and the clothes moth caterpillar, and by Ludwig (1934) and Abercrombie (1936) in the Japanese beetle in which it was found that there is a progressive

decrease in the progression factor with age. Calvert (1929), however, found in *Anax junius* the highest rate of growth at the ninth moult after 30 weeks of post-embryonic development, and high rates at the third, fifth, sixth, seventh and eighth moults. If the percentage gain in length for *Aeshna tuberculifera* is plotted in a manner similar to Calvert's Fig. 4 (1929) for *Anax*, comparable results are obtained (Fig. 5). This is in keeping with the discontinuous growth exhibited by *Anax* and *Aeshna*, which will be discussed in detail later. If, however, we conceive of the *Aeshna* larva as a growing organism within a non-living box, and compute

TABLE 1

PERCENTILE RATE OF GROWTH IF REGARDED AS CONTINUOUS, *Aeshna tuberculifera*

Instars	Number 8	Number 9	Number 15	Number 17
1-2			11	} 22
2-3	96	56	28	
3-4	81	43	13	
4-5	66	18	34	48
5-6	27	15	28	17
6-7	20	02	12	
7-8		32	10	18
8-9		13	02	23
9-10	09	06	008	04
10-11	009	08	09	006
11-12	04	009	035	04
12-13	05	03		06
13-14		06		03

the gain in length only from the points at which expansion takes place, we do find higher rates in the early stages. Table 1 illustrates this. It has been obtained by the following formula

$$\frac{L2 - L1}{\text{No. days}}, \quad \begin{array}{l} L2\text{—length at a given instar,} \\ L1\text{—length at preceding instar.} \end{array}$$

It must be remembered that we are assuming here a constant growth process within a confining shell, although we know that this is not strictly the case, as we have definite evidence of increase in length between moults as is indicated in Table 2.

3. Heterogonic Growth

Pezard (1918) employed the term heterogony to describe the type of growth of an organ growing at a different rate from that of the body as a whole, isogonic being that type in which the rate of

TABLE 2

INCREASE IN LENGTH OF LARVAE, *Aeshna tuberculifera* (LENGTHS IN MM.)

Larva Number 3 ♂			Larva Number 9 ♀		
Date	Larva	Exuvia	Date	Larva	Exuvia
Apr 5	2 78		Apr 5	2 54	
8	2 94		8		(not measured)(1)
12	2 96	3 02(1)	19	2 94	
19	3 11		29	3 68	3 30(2)
26	3 27	3 02(2)	May 3	4 50	4 05(3)
29	3 93	3 90(3)	6	4 58	
May 6	4 34		9	5 32	5 10(4)
9	4 66	5 10(4)	12	5 89	
12	4 41		16	5 97	
16	4 58		20	5 24	
20	4 99		24	5 73	
22	5 24	5 55(5)	25	6 46	6 60(5)
24	5 56		June 7		7 95(6)
June 11		8 55(6)	11	8 51	
12	8 67		18	9 48	9 80(7)
18	8 92		27		12 70(8)
23		10 66(7)	28	12 92	
Aug 6		(in fragments)(8)	July 7		14 38(9)
16	15 00		Aug 11		17 75(10)
25		18 75(9)	19	17 50	
Sep 11	19 50		Sep 11	19 00	
17		23 00(10)	14		23 00(11)
Mar 24	24 00		Mar 14-18		28 00(12)
Apr 14-18	27 00	27 64(11)	28	26 00	
May 5-9		32 90(12)	Apr 18	29 00	
26-30	35 00	36 00(13)	25-28	34 00	32 00(13)
June 14-17	38 00	40 00(14)	May 9	32 50	
July 4	39 00		11	32 00	
Aug 3	39 00	39 00(15)	23	32 00	
			30	37 50	38 00(14)
			June 4	37 00	
			7	36 00	(dead)
Larva Number 15 ♂			Larva Number 17 ♀		
Date	Larva	Exuvia	Date	Larva	Exuvia
Apr 5	2 86	3 00(1)	Apr 8	2 86	
12	2 78		19		3 16(1)
18	2 86		24	2 86	
19	3 11		26	3 00	3 26(2)
24	3 44		29	3 76	
26	3 27		May 4		4 15(3)
29	4 34	4 85(2)	6	4 42	
May 8	4 09		9	5 24	5 40(4)
9	5 32		12	5 48	
12	5 15		20	5 32	
16	5 72	5 70(3)	24	5 89	
20	5 66		26	6 38	7 00(5)
24	6 38		June 6		(incomplete)(6)
June 6		7 35(4)	June 12	10 31	10 95(7)
11	8 67		18	11 08	
13		9 10(5)	25	14 24	13 60(8)
18	9 43		July 7		17 44(9)
July 7		12 70(6)	Aug 25		21 22(10)
29		14 50(7)	Sep 11	17 50	
Sep 11	18 00	18 80(8)	Mar 7-10		23 85(11)
29		22 20(9)	28	23 00	
Mar 28	22 00	26 00(10)	Apr 21-25	26 00	28 66(12)
Apr 18-21			May 9	28 00	
25	25 00		11	27 50	
28	25 00		20-23	30 00	34 17(13)
May 11	27 00	32 00(11)	May 26	34 00	
13-16	32 00		June 11-13	36 50	38 15(14)
23	34 00		July 11	39 00	
26	32 00		17	41 00	(dead)
June 11-13	38 00	40 00(12)			
July 27		38 00(13)			

the part is the same as that of the whole. Champy (1924) has referred to heterogonic growth as disharmonic. Many evidences of this type of growth are reviewed by Huxley in Problems of

Relative Growth (1932). An example of heterogonic growth in dragonflies is given by Calvert (1934) from his *Anax junius* material. This author states, "The rate of growth is higher on the whole in those parts which are to be functional in the adult than in those the significance of which is larval rather than imaginal". The same results were obtained in *Aeshna tuberculifera*, the cercoids, wings and gonapophyses having a generally higher rate than the other parts

4. Growth without Moulting

Although the growth process has been divided into two phases by Brody, it is generally recognized in most animals as a continuous process. In arthropods, although growth in weight is a continuous process (Olmstead and Baumberger 1923, Eidmann 1924, and Teissier 1928) growth in length appears as a series of jumps. There are, however, considerable data presented by Calvert (1929) indicating that there may also be continuous growth in length—that is growth between the moults of Lepidopterous, Coleopterous, Heteropterous and Dipterous larvae, while insects with more rigid exoskeletons exhibit the typical discontinuous type of growth. Among the Odonata, larvae of *Nannothemis bella* (Calvert 1929) which have a more flexible exoskeleton, showed distinct increases in length between moults, while for those of *Anax junius* the changes other than at ecdysis are negligible.

In *Aeshna tuberculifera* there is some slight evidence (Table 2) that increases in length do occur between moults, in spite of the fact that differences in the amount of contraction or extension of the living larva will considerably alter the measurements for example, in the fifth instar of No. 15 and in the second instar of the same larva. Radio reports increases in length between moults in *Sinea diadema* (1924) and in *Corizus hyalinus* (1928). Abercrombie (1936) reported continuous growth in weight and length in the Japanese beetle, but discontinuous growth in length and breadth of the head capsule (a more chitinated part).

5. Changes in the Rate of Moulting

Although normally there is a wide variation in the rate of moulting, changes in the environment have been shown to vary the number and frequency of moults still further. Starvation in the larva *Trogoderma tarsale* according to Wodsdalek (1912) and to Riley (1883) increases the number of moults. The latter author

believed that not only the number of moults but also the frequency of moulting was increased by lack of food. Wodsdalek, however, found that the rate decreased by one-half under such conditions. Increase in the number of moults with starvation was also reported by Chapman (1920) in *Tribolium confusum*, Singh Pruthi (1925) and Teissier (1928) in *Tenebrio molitor* and by Titschak (1926) in *Tineola bisellhella*. Quaintance and Brues (1905) found in larvae of the cotton boll-worm "The cause of this variation in the number of moults in regard to the 10 per cent thus varying could always be traced to abnormal conditions . . . Moults in excess of the normal were caused by neglect and insufficient or disagreeable food".

The data on the effect of temperature on moulting seem to indicate that a low temperature decreases the number of moults while a high temperature increases the number (Severins 1913, the walking-stick, *Diapheromera femorata*, and Ludwig 1928, the Japanese Beetle). This decrease in the number of moults by exposure to lower temperatures may be comparable to the shortening of the diapause in Orthoptera by low temperatures.

The results of the effect of humidity on moulting are confusing. Ripley (1924) found some species of Lepidoptera in which a 100 per cent humidity reduced the number of moults by one and another species in which the number was increased by one.

These *Ae. tuberculifera* larvae raised side by side under identical temperature, humidity and food conditions show a variation in the number and frequency of moults as we have indicated (Fig. 1). It is evident from this study, just as it was in that made by Calvert (1929) on *Nannothemis bella*, that a specific moult cannot be used as an indicator of biologic age.

6. Increase in Length Immediately after Moulting

Shafer (1923) has shown that an abrupt increase in weight occurs in *Aeshna* immediately following moulting, due to the fact that the larva swallows a large quantity of water. Although increases in weights were not determined in this study, there is evidence as is seen in Table 2 that increases in length do occur after ecdysis in *Ae. tuberculifera*. This can also be observed in Calvert's Fig. 1 (1929), particularly after the eighth, ninth and tenth moults of *Anax junius*. This phenomenon is even more striking in the growth curves of the blood-sucking bugs *Cimex* and *Rhodnius*, which take only one large meal during each instar. Wigglesworth

(1934) points out that the weight curve of these insects show a series of acute peaks gradually rising to a higher level.

7 Moulting an Excretory Phenomenon

Titschack (1926), from studies of the walking-stick, *Carausius morosus*, concluded that moulting is not caused by growth pressure, is not related to periodic phenomena, but is a metabolic process which aids in the elimination of excretory elements. Such an explanation might fit in with our results with *Ae. tuberculifera*. Obviously growth pressure cannot be regarded here as the sole cause of moulting. A fairly constant amount of growth in successive instars would then be expected which we find is not the case, since we have moulting with little or no growth in certain parts.

8. Food and Growth

Since starvation has been shown to increase the number of moults, it should also lengthen the larval life, the growth period, and decrease the growth rate. There are some data, however, which indicate a more direct effect of food upon growth. Cousin (1926) fed larvae of the blue bottle-fly, *Calliphora erythrocephala*, with different quantities of food. Irregular or insufficient food was found to retard larval growth, to double the duration of larval life, and to produce imagoes which were distinctly smaller. Kopec (1924), in starvation experiments upon the caterpillars of *Lymantria dispar*, states "During long-lasting starvation, organisms get accustomed to the abnormal conditions the rate of growth of the caterpillars starved every second day during their whole life becomes in time considerably greater than that of specimens analogously deprived of food since their last moult but one".

Although quantitative food records were not kept until the septultimate instar, the results illustrated in Table 3 for *Ae. tuberculifera* seem to indicate a greater consumption of food per unit time for the two individuals which transformed, the greater amount of food having been consumed by No. 15, which transformed in the shorter time, with fewer instars. Since the food was not weighed, and since some of it was undoubtedly consumed by other predators, which had also been placed in the jars as food for the *Aeshnas*, this is only a rough approximation.

TABLE 3

FOOD CONSUMED IN LAST SEVEN INSTARS BY *Aeshna tuberculifera* LARVAE *

Instar	Began	Ended	Number of Days	Mosquito Larvae	Mosquito Pupae	Mayfly Larvae	Odonate (Zygop) Larvae	Odonate (Anisop) Larvae
Number 3								
9	Aug 6	Aug 25	19	60	4	3	3	6
10	Aug 25	Sep 17	23	12	2	26	10	2
11	Sep 17	Apr 14-18	209	221	53½	29	16	
12	Apr 14-18	May 5-9	21	46	58			
13	May 5-9	May 26-30	21	60	18	6		
14	May 26-30	June 14-17	19	118	23	7		
15	June 14-17	Aug 3	50	578	22	16		
	Total		362	1095	180½	87	29	8
Number 9								
9	June 27	July 7	10	43	2	3-4		
10	July 7	Aug 11	35	82	6	11	4	2½
11	Aug 11	Sep 14	34	39	19	8	6	6
12	Sep 14	Mar 14-18	182	183	28½	42½	29	
13	Mar 14-18	Apr 25-28	42	181	26			
14	Apr 25-28	May 26-30	31	96	34			
15	May 26-30	June 6	11	79	29			
	Total		344	703	144½	64½	39	8½
Number 15								
7	June 27	July 7	10	57		4		
8	July 7	July 29	22	86	6	11	8	
9	July 29	Sep 29	62	133	22	17	14	6
10	Sep 29	Apr 18-21	201	368	52½	47	5	
11	Apr 18-21	May 13-16	25	111	12			
12	May 13-16	June 11-13	29	112	49	9½		
13	June 11-13	July 27	46	467	15	9		
	Total		395	1334	156½	97½	30	6
Number 17								
9	June 25	July 7	12	56	1	7-8		
10	July 7	Aug 25	49	179	16½	12	15	3
11	Aug 25	Mar 7-10	194	261½	20	35	24	1
12	Mar 7-10	Apr 21-25	45	202	15	3		
13	Apr 21-25	May 20-23	29	102	16			
14	May 20-23	June 11-13	22	100	35	1½		
15	June 11-13	July 17	36	250	11	15		
	Total		387	1156½	114½	83½	39	4

* In addition, the following food was consumed by the same four larvae of *Aeshna tuberculifera*. By larva No 3 in its 9th, 10th and 11th instars 1 Dytiscid, 2 Hydrophilid, 2 Notonectid, 4 Dixid and 1 Perlid larvae, 2 Chironomid pupae and 2 frog tadpoles (except their tails). By larva No 9 in its 10th, 11th and 12th instars 1 Dytiscid, 1 Hydrophilid, 7 Notonectid, 3 Chironomid, 3 Dixid and 1 Perlid larvae. By larva No 15 in its 8th, 9th and 10th instars 1 Hydrophilid, 4½ Notonectid, 1 Chironomid, 10 Dixid, 1 Perlid and 2 Trichopterous larvae. By larva No. 17 in its 10th, 11th and 15th instars 6 Notonectid, 8 Chironomid, 2 Dixid, 1 Perlid larvae and 6 frog tadpoles.

9 Growth Gradients

D'Arcy Thompson (1917) says one of the characteristics of growth is that "it varies in different parts of the body and according to various directions or axes, such variations being definitely correlated with one another and thus giving rise to the characteristic proportion of the organism and to the change in form which it undergoes in the course of its development". These growth gradients have been discussed at length by Huxley (1932). In the present material, the antennae seem to exhibit a growth gradient with the growth center located in the third segment. The growth rate of this segment is not higher but differentiation seems to take place at this point. The newly hatched larva has 3 segments, the third dividing to produce the third and fourth, the third again dividing to produce the new third and fourth, the fourth becoming the fifth. Once more the third divides to produce again a third and fourth segment, while the fourth becomes the fifth, and the fifth the sixth. The seventh segment seems to be formed by a division of the sixth segment.

10. Relative Growth

Champy (1924), Huxley (1932), and others have shown that organs increase in relative size with the absolute size of the body which bears them. To express this relationship, Huxley has employed the formula $y = bx^k$, in which y = the part, x = the whole, b = a constant (the per cent of x which y occupies when x is equal to unity) and k = the specific constant for the growth rate of y . Calvert (1934) determined the value of " k " for the wing rudiments of *Anax junius* and found that it varied from 1.25 to 10.37. The same has been done for this material of *Ae. tuberculifera*. " k " for the hind wing rudiment of No. 9 varied from 1.5 to 4.7, and for the head width from the first to the seventh instars from 2.65 to 9.47.

But in spite of this wide variation in the specific constant, if the growth of the head width, cerci length, hind femur length, etc., of all the individuals are plotted (Figs 2-4), it is observed that the curves for the growth in total length, for example, bear the same relationship to each other on the whole, as the curves representing the growth of the labia, the wings, the cercoids, even the primary ligular cleft. It appears then that whatever factor influences the growth rate of the whole or a part, at a given time, also exerts a

similar influence on all the other parts, regardless of the disharmonic growth of the organ.

CONCLUSIONS

A Measurements were made of the exuviae of individuals of *Aeshna tuberculifera* raised from the egg to, or almost, to transformation. The growth rates or progression factors were determined. Results indicate:

1. There is wide variation in the growth rate from instar to instar and in different structures of the same instar in the same individual.

2. Przibram and Megusar's theoretical growth rate of 1.26 does not seem to hold for this species.

3 Those parts to be functional in the adult, the wings, the gonapophyses and cercoids, show a more rapid rate of growth.

4 There is a definite underlying pattern or rhythm of growth which is discernible in all the parts regardless of their characteristic rates. This pattern is demonstrated by comparison of curves of the growth

5. Some gain in length occurs between moults.

B The length of each instar and the number of instars before transformation is extremely variable

C There is a slight correlation between the amount of food consumed and growth.

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DEVELOPMENTAL EFFECTS RESULTING FROM EXPOSURE TO X-RAYS. II. DEVELOPMENT OF LEOPARD FROG EGGS ACTIVATED BY BULLFROG SPERM¹

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(Communicated by Edwin G Conklin)

ABSTRACT

Following the suggestions of the previous work ('39) a separation of the two functions of the spermatozoon (activation and hereditary contribution) was attempted by irradiation of bullfrog sperm prior to inseminating leopard frog eggs. Under normal conditions this hybrid cross invariably causes the death of the embryo at about the time gastrulation would occur but when the bullfrog sperm are first irradiated, the embryonic development goes beyond gastrulation and when irradiation is carried up to 66,000 r as many as 80% of the embryos hatch and develop into tadpoles. Most of these tadpoles are obviously haploids, manifesting those characteristics normally associated with tadpoles parthenogenetically developed. Some, however, appear as normal as the controls and such chromosomal observations as were possible indicate a normal (diploid) number of chromosomes, probably entirely maternal. The stages of development reached by the embryos in relation to the x-ray exposure of the sperm suggest that the abolition of the hereditary function of an individual sperm is progressive rather than abrupt. Since bullfrog sperm exposed to 66,000 r can only activate the leopard frog eggs, parthenogenesis has been successfully produced in 97% of such cases.

In the previous paper of this series (Rugh '39) it was shown that even though chromosomes are not visible in the mature spermatozoon, its hereditary nuclear material can be damaged by x-radiation and when the chromosomes are re-formed, they are often fragmented. In some cases damage was accomplished with an exposure of as little as 25 roentgen units at 200 kv. This damage was measured by its effect on the otherwise normal development of the embryo. Evidence was presented that with increased exposure of sperm to x-radiation, there was a correlated increase in the abnormalities of the resulting embryos until at 1,000 r. no normal embryos developed. When the exposure was carried beyond to 50,000 r. however, quite normal embryos resulted from as many as 91 per cent of the eggs fertilized by the irradiated sperm. These embryos showed certain diagnostic abnormalities recognizable as haploid characters, i.e., characters normally associated with eggs stimulated by artificial parthenogenesis, and having half the normal

¹ Aided by a Grant from the Penrose Fund of the American Philosophical Society

number of chromosomes. This meant that up to 1,000 r the sperm nucleus was contributing increasingly damaged hereditary material to the egg while somewhat beyond that point the second of the two major functions of the sperm (hereditary influences) was eliminated. The rising percentage of haploid larvae above 10,000 r indicated a reciprocal decline of the deleterious effect of the hereditary elements of the damaged sperm until the sole function of activation remained

One reason for the failure of hybrids to develop is the incompatibility of their respective sets of maternal and paternal chromosomes. Numerous crosses between species of amphibia will give normal cleavage and normal blastulae but at the beginning of gastrulation, when the nucleo-cytoplasmic ratio has been restored and when the hereditary influences of the sperm begin to manifest themselves, then is the time when development usually ceases. The foreign sperm can activate the egg and can take a passive role up to gastrulation but at that point the hereditary tendencies begin their effect on development and if these tendencies are incompatible with those of the egg, development ceases.

An experiment has been devised which demonstrates these two major functions of the sperm. Bullfrog (*Rana catesbiana*) sperm will give 100 per cent fertilization if used with leopard frog (*Rana pipiens*) eggs. The rate and pattern of cleavage is typically that of leopard frog eggs fertilized by leopard frog sperm. But invariably these eggs go to pieces when gastrulation should begin. If, then, the hereditary contributions of the sperm can be separated from the activating power, and eliminated, development should go beyond gastrulation. The purpose of these experiments was to test the validity of this assumption by separating the two functions of the sperm by means of exposure to very large doses of x-rays prior to insemination of normal eggs.

MATERIALS AND METHOD

Adult male bullfrogs (*Rana catesbiana*) were secured directly from their hibernating environment. Normally this species does not breed until June or July but, as with most amphibia, male gametes are matured prior to hibernation and are maintained in this condition by Sertoli cells until liberated. Sexual activity and the liberation of gametes can be stimulated by the injection of amphibian anterior pituitary hormone but in these experiments whole testes of untreated males were used.

The testes of 5 adult bullfrogs were excised and each gonad was cut horizontally with a sharp blade into equal parts, with a minimum of manipulation. This provided 20 half gonads from five different males. Six of these half gonads, known to represent five males, were isolated in a covered celluloid dish as control material. This left 14 half gonads for irradiation, and these were placed in a similar covered celluloid dish. In each of these dishes there had been placed a thin layer of cotton moistened with 20% Holtfreter's solution. This is the usual medium for making up sperm suspensions and is therefore known to be non-toxic and was used to compensate for any possible evaporation within the closed containers. As the experimental material was exposed to successive doses of x-radiation, the cover of the celluloid dish was removed and two half gonads (known to be from different males) were removed from the dish at each level from 500 r. to 20,000 r. Beyond 20,000 r. three half gonads were removed after each interval of exposure up to 66,000 r. The control material was kept under the same physical conditions of temperature and humidity until the termination of the exposure of the experimental material, and was then used to inseminate the eggs from the common source. These precautions insured uniformity in sperm suspensions used except for the one physical variable, irradiation.

Following appropriate irradiation the testes were removed to covered finger bowls until the termination of the entire radiation period. Then each sperm mass was cut up in approximately 10 cc of 20 per cent Holtfreter's solution, taking all necessary precautions against contamination. All sperm suspensions, including the control, were then made up simultaneously using the same suspension medium, and all were allowed to stand for 20 minutes at room temperature before using for insemination of normal leopard frog eggs.

Two days previous to this irradiation work, three sexually mature and hibernating female leopard frogs (*Rana pipiens*) were each injected intraperitoneally with six female anterior pituitary glands. The frogs were then kept at 20° C. in a small amount of water. Just prior to the irradiation of bullfrog sperm, eggs were stripped from each of these leopard frogs into leopard frog sperm suspensions, in another laboratory, in order to test their viability. Normal development of all of these control eggs ensued.

Each female produces about 2,000 eggs and once they have reached the uteri they will remain in viable condition for at least

4 days at 20° C. It is common practice in this laboratory to strip 50–100 eggs from the uteri as they are needed, and since the eggs are all at the same stage of development, this has proved to be ideal experimental material. The only precaution found to be necessary was the removal of the first 10–20 eggs that emerge upon stripping because the proximity of the cloaca to the uterus sometimes causes a reduction in the fertilizability of the nearby eggs. This may be due to a premature swelling of the egg jelly permitted by cloacal or bladder fluids.

Eggs were stripped from each of the three females into each of the sperm suspensions representing irradiations from 500 r. to those which had been exposed to 66,000 r. Care was taken to see that the eggs were all flooded with the sperm suspensions and 15 minutes later the egg masses were covered with the same fluid used for sperm suspensions, i. e., 20 per cent Holtfreter's solution. Within an hour all eggs had rotated, indicating normal fertilization, and by 3 hours they were all in the 2-cell stage. At this point the egg masses were reduced so that 50 isolated eggs were placed in each finger bowl with fresh Holtfreter's solution. All finger bowls were covered with glass plates and placed on a water table where the temperature was uniform at about 15° C.

Irradiation was carried out with a Sloan ('35) x-ray apparatus, designed and built primarily for biological experimentation at the Institute of Cancer Research (now Department of Cancer Research) of Columbia University. A water-cooled tungsten target hangs 32 cm. above a 12.5 cm. diameter window in the bottom of the steel tank which serves as the x-ray tube. The window consists of 0.4 mm. aluminum and 0.25 mm. copper. The half testes in a thin celluloid container were placed 2 cm. below the center of the window. The container was covered with 1.0 mm. of aluminum to cut out the soft fluorescent radiation from the copper window. With an effective target potential of about 250 kilovolts and a biased cathode emission of 20 milliamperes the x-ray intensity acting on the testes was 380 roentgens per minute. Off-focus and scattered radiations made up about 15 per cent of this amount. The copper half value for the focal radiation was 1.1 mm. and the quarter value layer was 3.4 mm. The intensity measurements were made with a Victoreen condenser dosimeter with two recently calibrated chambers together with an auxiliary chamber of small volume. The non-uniformity of exposure within the half testis due to absorption was less than 5 per cent.

In the column listed as Normal reference is made to absolutely standard conditions while in the column listed as Abnormal tadpoles, reference is made to embryos the majority of which mani-

EXPERIMENTAL DATA

DEVELOPMENT OF LEOPARD FROG EGGS FERTILIZED BY IRRADIATED BULLFROG SPERM

Irradiation	No Eggs	Cleavage	Gastrula	Neurula	Hatching	Normal	Abnormal Tadpoles
Control (none)	250	100%	100%	100%	100%	100%	0%
500 r	300	100	64	0	0	0	
5,000 r	350	100	54 0	17 0	3 2	0	0 3
10,000 r	400	100	94 0	24 0	12 0	0	3 5
20,000 r	300	100	94 0	53 0	32 0	0	5 0
30,000 r	350	100	95 0	94 0	62 0	0	35 0
66,000 r	400	100	97 0	96 0	80 0	0	80 0

festated those characters previously associated with haploidy. No doubt most of these were haploids. At 66,000 r. many of the hatched tadpoles appeared quite as normal as the controls (Plate II) showing only slightly those characters of stunting, dorso-ventral thickening, micro-cephaly, etc., which have been demonstrated as haploid characters. Some of these tadpoles were not only normal in appearance but were actually larger than the controls (Plate II, fig. G).

In Fig 1 below, the experimental data are brought together to indicate the convergence of the hatching and the tadpole curves at the higher levels of irradiation. At 30,000 r. as many as 27 per cent of the embryos will hatch and then die, while 35 per cent will go on to tadpole stages. But at 66,000 r. all embryos that are able to hatch will develop into tadpole (swimming) stages, most of which exhibit definite haploid characteristics.

DISCUSSION

The spermatozoon is known to have two functions. Its initial function is the activation of the egg. The peripheral stimulation of the egg by the sperm brings about an instantaneous cortical alteration which results in rapid physico-chemical changes involving the entire egg. These changes are normally followed by cleavage and development of the egg, which processes terminate only with the death of the organism. Such activation can be

accomplished in the amphibian egg by means of artificial parthenogenesis, simply by pricking the egg with a needle (Bataillon '10, Brachet '11, Parmenter '20). The second major function of the

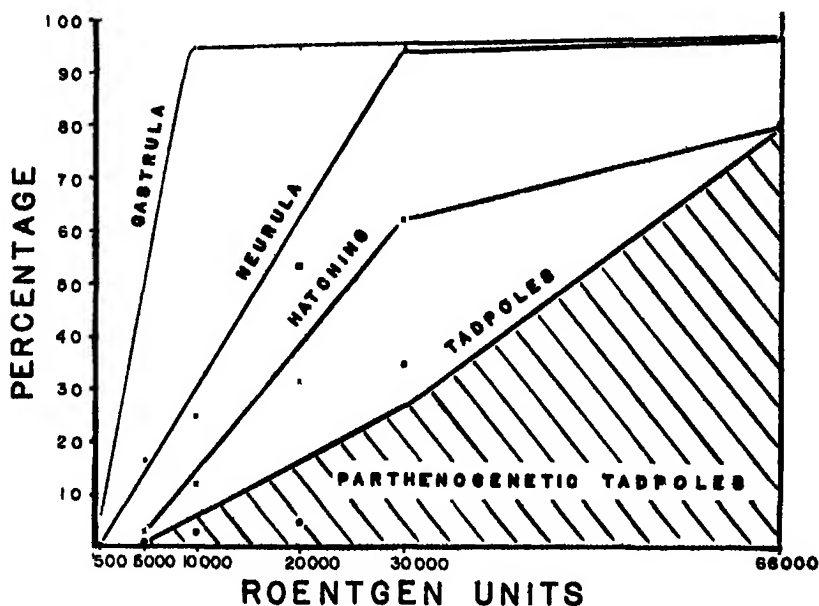


Fig 1

spermatazoon is to contribute to the egg a paternal set of hereditary tendencies (chromosomes bearing genes) restoring to the egg the normal diploid (somatic) number of chromosomes. The importance of this second function of the spermatazoon is borne out by the innumerable researches of the geneticists of the last 40 years. A third possible function is not of particular concern here but relates to the role the sperm entrance path may play in the symmetry of the future embryo.

In order to function in fertilization, spermatazoa of all animals must first reach maturity, i.e., they must have completed a definite maturation cycle. In this mature state the spermatazoon consists of very little cytoplasm and a nucleus in which the chromosomes cannot be identified. The chromosomes, then, of the mature spermatazoon cannot be seen and yet they reappear in regulation number, shape, and size, when the spermatazoon has invaded the egg of the same species and development ensues.

The early development of hybrids is known to be quite normal and where there are differences in cleavage rate or pattern, the

hybrids show the maternal influences up to gastrulation. This is borne out by these irradiation experiments (Plate I, Fig *D*) where the blastula is indistinguishable from a control blastula of *Rana pipiens* eggs fertilized by *Rana pipiens* spermatazoa. The next stage, however, shows a characteristic mottling of the entire egg surface (Plate I, Fig *F*) such as invariably occurs in hybrid crosses between these species when the sperm are not irradiated. Plate I, Fig. *H* shows the most successful attempt of such hybrids to achieve gastrulation. In contrast with this series is another (Plate I, Figs. *A*, *C*, *E*, *G*) showing *Rana pipiens* eggs which had been fertilized by *Rana catesbiana* sperm previously exposed to 66,000 r. of x-rays. This series is indistinguishable from the normal (control) series. Gastrulation is accomplished without any variation from the normal. When bullfrog sperm were used which had been exposed to much lower doses of x-rays, cases of exogastrulation resulted which were comparable to those reported in the previous paper.

At the higher exposure (66,000 r.) 97.2 per cent of the hybrids gastrulate, 80 per cent hatch, and all of these develop into swimming tadpoles. There is considerable variation in these tadpoles, all of which are assumed to have developed parthenogenetically inasmuch as the non-irradiated bullfrog sperm would not have carried development beyond gastrulation. The differences (Plate II) may be due to varying ability of different individuals to recover the diploid condition during development. Some of these individuals demonstrate clearly those characteristic parthenogenetic features while others cannot be distinguished from the controls. The obvious haploids generally died in about 12–15 days at 15° C., but some of the more normal appearing parthenogenetic tadpoles survived until killed for study, about 5 weeks. Tail tip studies in the anura are quite difficult as compared with the urodeles, but normal chromosome groups and mitoses can be demonstrated in these normal appearing parthenogenetic tadpoles (Plate II, Figs *K* and *L*) while in some of the definitely parthenogenetically (haploid) tadpoles the chromosome pictures are generally abnormal (Plate II, Figs *M* and *N*). Chromosome counts were attempted and ranged from 10 in some definitely abnormal tadpoles to a maximum of 24 in the tadpoles that lived for 5 weeks. The normal number for *Rana catesbiana* is 26, and for *R. pipiens* is 24 so that while chromosome studies were not conclusive, the maximum number never exceeded that of *Rana pipiens*.

The increasing development of embryos from sperm receiving increasing doses of x-rays, beyond a certain point, is interpreted to mean that an increasing number of gametes are so altered by the x-rays that haploid development is possible. In a single spermatozoon this change might occur all at once, in the nature of an all-or-none phenomenon, in which case one would expect that all embryos that can achieve gastrulation would go on to complete development. This is clearly contrary to the observations shown in the table. On the other hand, if the x-ray effect is cumulative and syngamy occurs in all cases, then with increasing dosage of x-rays, individual embryos should show an increasing degree of release from the detrimental effect of the foreign sperm. It is suggested that at the lower exposures (lower dosages) fewer genes are affected, and with the prolongation of the exposure more and more genes are bombarded until either the summation of the genic damage or the developmental importance of affected genes prevents the normal functioning of the male (sperm) complex of hereditary factors. At 500 r. only a few of the embryos have the machinery necessary for gastrulation unencumbered by bullfrog heredity. With 10,000 r. 94 per cent of the embryos gastrulate but only 12 per cent are sufficiently cleared of the effect of the bullfrog genes to be able to hatch, and only 3.5 per cent develop as tadpoles. With 66,000 r. 80 per cent hatch, all of which develop into tadpoles and show either the characteristics of haploidy or recovery from haploidy toward the normal, diploid condition.

Such a separation of the genetic and the activating functions of the sperm was suggested in the previous paper but in that case leopard frog sperm and eggs were used. In the work reported here it was shown that the nuclear material of the two species is incompatible and that development invariably ceases at gastrulation. The bullfrog sperm under normal conditions will so adversely affect the leopard frog nucleus that development is terminated at gastrulation, but by means of irradiation the bullfrog sperm is bereft of all of its functions save that of activation. And this is accomplished with 97 per cent effectiveness. This is essentially artificial parthenogenesis, resulting in 80 per cent developing tadpoles when the irradiation of foreign sperm was carried to 66,000 r.

SUMMARY AND CONCLUSIONS

1. Leopard frog eggs fertilized by bullfrog sperm will not develop beyond the gastrula stage.

2 The early development of leopard frog eggs fertilized by bullfrog sperm is normal in every respect, i.e., rate and pattern of cleavage are typically that of normal leopard frog eggs fertilized by leopard frog sperm.

3. Irradiation by x-rays in no way affects the activating power of bullfrog sperm. Activation of leopard frog eggs is complete in every case.

4 The two functions of the sperm are clearly defined, one being to activate the egg and the other to supply genetic tendencies. This latter function of bullfrog sperm is incompatible with the normal development of leopard frog eggs

5. With exposure above 500 r some of the sperm will allow the egg to develop to gastrulation but not beyond into the neurula stage

6. With exposure of bullfrog sperm to 5,000 r as many as 54 per cent of the eggs of the leopard frog gastrulate, 17 per cent neurulate, and 3.2 per cent develop as tadpoles, all with the characteristics of haploidy

7 With increasing exposure to irradiations there is a rise in all percentages so that at 66,000 r exposure 80 per cent of the embryos hatch and all of these develop into tadpoles. Many of these tadpoles appear as normal as the controls.

8 The above conclusions indicate that there is an increase in gynogenetic development based upon the progressive defunctioning of the genetic contributions of the sperm resulting in parthenogenetically produced haploidy. Should there have been any contamination with non-irradiated sperm, this would have been evident with the cessation of development at gastrulation.

9. This work suggests that parthenogenesis might be accomplished with mammalian material by the irradiation of sperm with high doses of x-rays prior to coition. This work is shortly to be attempted

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PLATE I

Two series in vertical groups

A, C, E, G Blastula through gastrula stages of *Rana pipiens* eggs fertilized by *Rana catesbeiana* sperm following exposure of sperm to 66,000 r x-rays. This series cannot be distinguished from controls.

B, D, F, H Blastula through gastrula stages of *Rana pipiens* eggs fertilized by normal (untreated) sperm of *Rana catesbeiana*. Mottling (*F*) is characteristic of impending degeneration of the embryo, and occasionally such embryos attempt gastrulation (*H*) but never succeed.

A, B C, D E, F G, H represent corresponding stages of development.

PLATE I

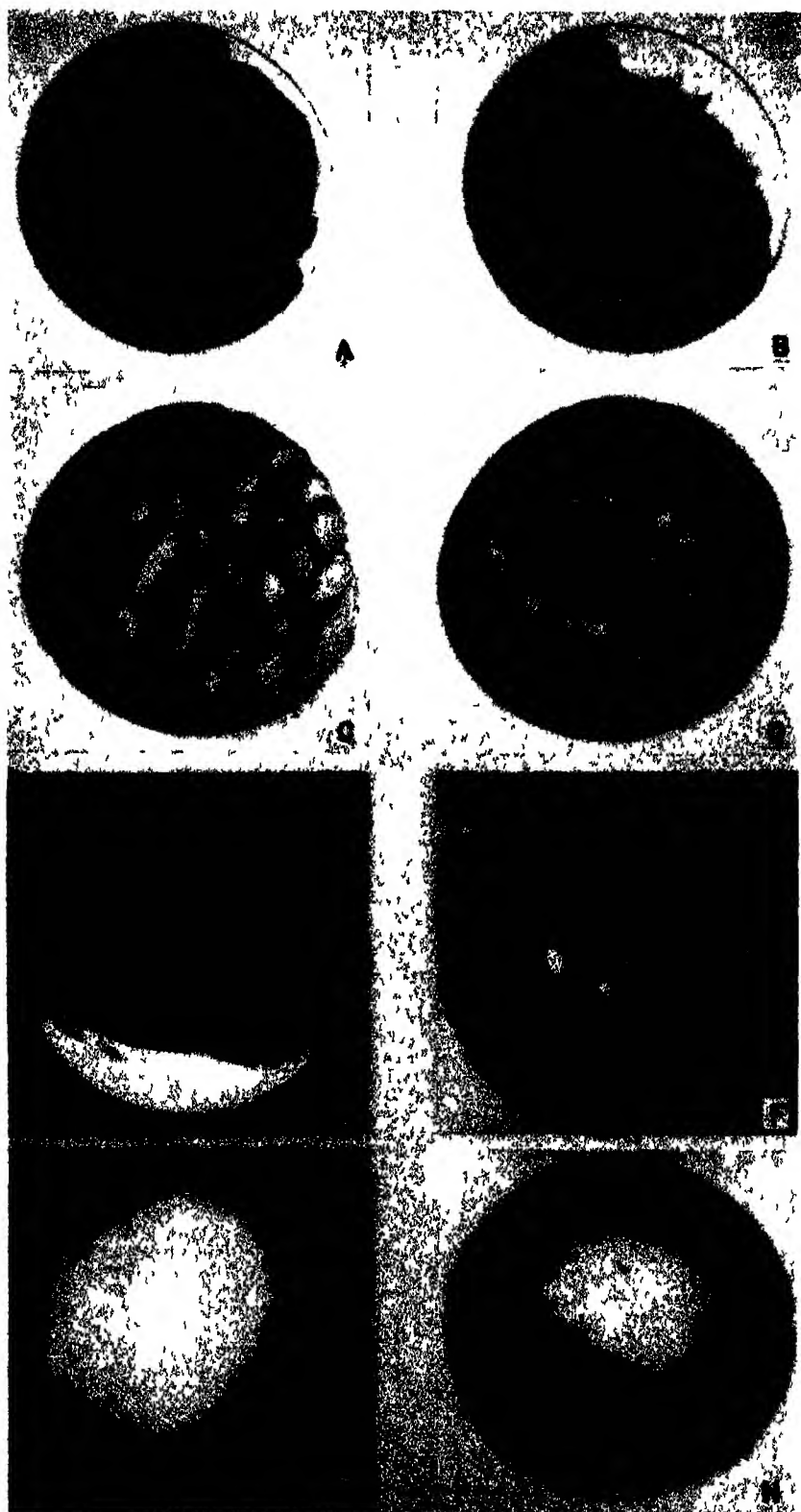


PLATE II

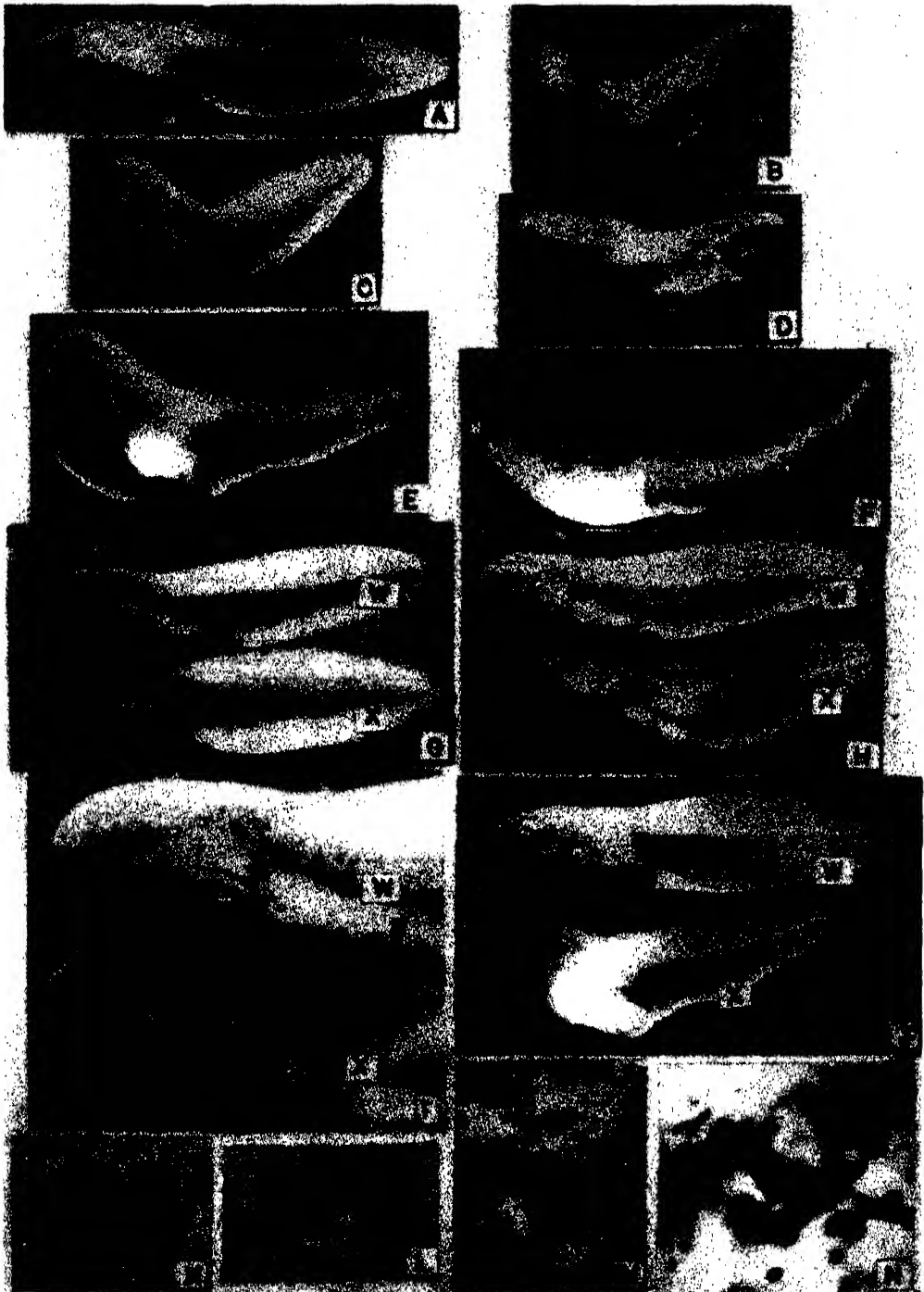
A to F Parthenogenetic tadpoles, developed from *Rana pipiens* eggs inseminated by *Rana catesbeiana* sperm after the sperm had been exposed to 60,000 r x-rays and thereby rendered useless in syngamy. Note typical haploid conditions of some and apparently normal characteristics of others.

G to J Control and Experimental tadpoles, the controls being *Rana pipiens* eggs and sperm and the Experimental (*X*) being *Rana pipiens* eggs inseminated with irradiated *Rana catesbeiana* sperm. Controls (*W*).

K and L Photographs through oil immersion lens of normal chromosome configuration and mitotic figure of normal looking experimental tadpoles (such as in Fig. *A*).

M and N Apparently fragmented and unorganized chromosomes characteristic of tadpoles which had the outward characteristics normally associated with haploidy (*E, F*). Chromosome numbers obviously reduced.

PLATE II



STUDIES ON THE PHOSPHORUS CONTENT OF THE ESTUARINE WATERS OF CHESAPEAKE BAY¹

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(Communicated by Henry B. Bigelow)

ABSTRACT

A review of the more cogent points resulting from studies on phosphorus variations in the waters of Chesapeake Bay is presented. Highest concentrations of this nutrient occur during summer when the turbidity of the water and the light intensity are greatest. At this time, the content of phosphorus is significantly higher in the heavier bottom waters which are characteristically low in oxygen throughout this season. During September, a decrease in phosphorus content occurs accompanied by an increase in oxygen and in transparency. The bottom waters of the Bay in the direction of its headwaters are progressively lower in salt content but higher in concentration of phosphorus, and in turbidity. Diurnal changes in phosphorus content are pronounced, there being an abrupt increase following sunset and a decrease following sunrise. In these instances, an increase in amount of phosphorus coincides with a decrease in amount of *light*. The possibility that these variations in natural waters may be due to metabolic processes of the plankton as suggested by the experimental results of several investigators is discussed.

STUDIES of the amount of phosphorus in the estuarine waters of Chesapeake Bay during the summer of 1938 gave evidence of pronounced variations in time not explainable on a basis of assimilation and release by ordinary processes of decay and digestion by other organisms. Data from subsequent observations have supported these findings and extended our knowledge of the characteristics of these fluctuations (Newcombe and Lang, 1939; Newcombe and Brust, 1940). Water samples for the study of diurnal variations were taken at Station 1 in the deeper waters of the Chesapeake Bay and from inshore waters near Station 6 (Fig. 1). Distributional data are based on collections taken in the Patuxent River and in that portion of the Bay south of the Severn River near Annapolis, Maryland (Fig. 4).

¹ Contribution from the Virginia Fisheries Laboratory and the Department of Biology, College of William and Mary.

Assistance in the pursuit of this study was rendered by a Research Grant from the American Philosophical Society.

There are two distinct classes of variation in the phosphorus concentration of Chesapeake waters that are considered in this discussion, namely, diurnal changes and seasonal changes

Diurnal Changes—Records of hourly samplings have shown that light and darkness apparently produce distinct effects upon the amount of phosphorus in the water. During hours of day-



FIG 1 Location of principal sampling stations (numbered points) in the Patuxent River, Md

light the phosphorus content has been found to decrease or remain relatively low while during and following the hours of sunset a pronounced increase obtains and relatively high concentrations prevail until sunrise. Although considerable variations are known to exist between the different series that have been taken, nevertheless, there seems to be a general agreement in respect to the occurrence of higher concentrations during night time. Fig. 2 shows the trends of phosphorus concentration,

tidal amplitude and chlorinity in shore waters near Station 6 during periods of 26 hours. The uniformity of the day to night trend of chlorinity shown in this figure seems to disprove any possibility that variations in phosphorus content might have been caused by upwellings of bottom water or in some way correlated with the tidal cycle. Furthermore, day and night time sampling in the shallow waters between the end of the pier and shore have indicated changes which are not considered to be greater than the normal variation obtained over this range of distance in the general region.

Seasonal Changes — Monthly trends of phosphorus concentration in the shore waters at Solomons Island during the period June 20, 1939, until January 18, 1940, are shown in Fig 3². The months of maximum phosphorus content are July and August, at which time the turbidity is greatest. During September, a pronounced decrease in phosphorus and an equally conspicuous decrease in turbidity as indicated by Secchi disc readings have been shown to occur. This phosphorus-turbidity relationship is the reverse of that characterizing the summer season and is maintained during the fall and winter periods.

It has been shown already that a clearly defined oxygen stratification exists in the Chesapeake Bay during the period from about June 1 to about October 1, with very low oxygen concentration in the bottom waters (Newcombe and Horne, 1938). An equally pronounced chlorinity stratification of opposite gradient occurs. Numerous horizontal sections taken during different seasons of the year have indicated a conspicuous increase in phosphorus (from about 0.15 to 1.7 mg atoms per m³) accompanying the decrease in chlorinity (from almost 8.5 to 4.5 per mille) and also in transparency (from a Secchi disc reading of about 1.7 to 0.5 meters) in the direction of the river headwaters, Station 21 (Fig 1). In contrast to this condition, vertical gradients studied during the summer season have shown that with increase in depth a significant increase in phosphorus accompanies the increase in chlorinity. Briefly, there are two conspicuously different strata of water which, despite local and short interval changes, maintain their identity. The boundary between the two is located at a depth of from 9 to 13 meters. It

² For assistance in preparing material for this figure, thanks are expressed to Mr. Harry F. Brust and Dr. Rodney Olson of the Chesapeake Biological Laboratory where these analyses were made.

is within this four meter distance that the more abrupt changes in the vertical trends may be expected. The upper stratum extending to a depth of about 9 meters is characterized by maximum amount of light, highest oxygen concentration and highest temperature, whereas, the bottom stratum of water possesses a minimum of light available for photosynthesis, a minimum quantity of dissolved oxygen and a maximum concentration of phosphorus. Bottom waters in summer frequently contain from 0.6 to 1.1 mg. atoms of P per m^3 in comparison with comparable surface values of about one-half of this magnitude. Thus it is seen that while an increase in the concentration of phosphorus may accompany a decrease in chlorinity or an increase in chlorinity it is in each instance positively correlated with a decrease in the amount of *light*. This correlation stresses the importance of a study of plant metabolism in order to arrive at an explanation of the fluctuations in concentration of phosphorus.

There is some information on the general conditions in the Bay that is quite suggestive in connection with the possible effects of light and darkness upon the amount of phosphorus present. From the above discussion it is evident that the period of high phosphate concentrations (summer) coincides with the period during which light penetration is poorest and the amount of dissolved oxygen is least. During September the decrease in phosphorus content is accompanied by an increase in oxygen and in transparency. Despite the fact that solar radiation is greatest in summer, the depth of the photosynthetic stratum is reduced on account of the high turbidity of the water. During other seasons when the turbidity and solar radiation are reduced, the depth of light penetration is increased, as indicated by higher concentrations of oxygen at the lower levels. The phosphorus content at these levels is below that which prevails in summer (Newcombe, Horne and Shepherd, 1939). Concentrations of phosphorus as high as 0.8 mg. atoms per m^3 were obtained off the Virginia Fisheries Laboratory pier near the mouth of the York River as late as early October. The turbidity, however, was high as indicated by a Secchi disc reading of only 87 cm.

Available evidence points to a fairly uniform concentration of phosphorus in the surface waters of the main channel of the Bay. In the deeper waters, however, an increase from south to north has been shown to occur during summer seasons, the

element in solution and slowly decline in the direction of the mouth of the Bay due to mixing with more oceanic water of the lower bay which is relatively low in phosphorus. Indirectly, the increase may occur also as a result of high turbidity in northern waters declining progressively in a southern direction. The latter factor is of a biological nature.

Considering for a moment the hydrographic properties of Chesapeake waters, it should be pointed out that two separate and distinct water movements are recognized in the Bay, namely, a northerly moving, bottom stratum of relatively clear, high chlorinity water being oceanic in origin, and also a southerly moving, surface stratum of turbid, low chlorinity water being estuarine in origin. With this in mind, it may be possible to throw some light on the character of the known phosphorus variations in surface and bottom waters. The concentration of phosphorus in the heavier bottom waters shows a progressive increase in an up-bay direction. A part of this increase may doubtless be explained on a basis of direct contribution of phosphorus by the estuarine waters, although they are considered to exert, primarily, a surface effect. Doubtless the colloidal soil particles added to the surface layers reach the bottom waters by a gradual settling process. However, it remains an open subject as to the magnitude of the effect of these contributions and of the addition of plant nutrients washed from soil on the amount of dissolved inorganic phosphorus in the waters. Another explanation may be found in a direct biological approach.

Stress has been placed on the possible significance of turbidity in reference to phosphorus utilization linked with photosynthetic activity. The turbidity is high in the upper-bay surface waters since enormous quantities of silt are contributed annually by its tributaries notably the Susquehanna River basin located at the most northerly extremity having a drainage area of 27,400 square miles with a mean rate of contribution of 41,300 second-feet (Cf. Harvey, 1928; Riley, 1937).

It is reasonable to assume that the photosynthetic stratum increases in thickness in a southerly direction as a response to the decrease in turbidity expected to occur. This brings about a more intensive utilization of phosphorus. However, the bottom water mass moving in a northerly direction is increasingly

isolated from light because of the increasing turbidity of the surface waters in a northerly direction. Consequently less and less phosphorus utilization may be expected to take place. Another possible factor tending toward an increase of free phosphorus in the bottom waters during their northern movement is

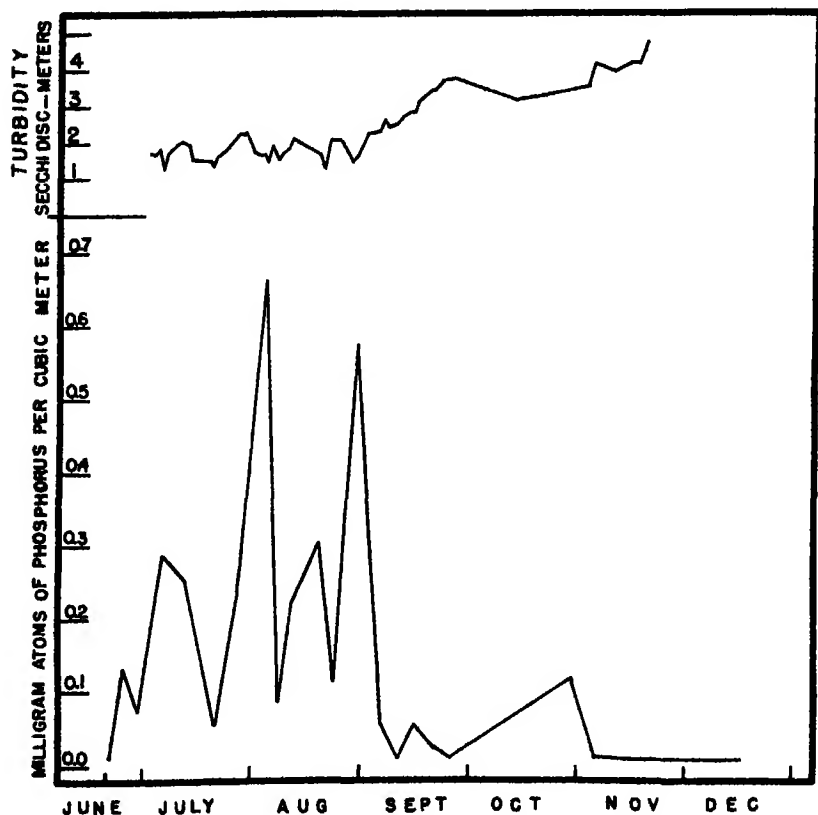


FIG. 3 Showing trends in the phosphorus concentration and in the transparency of the shore waters near Station 6 during the period June 20-Dec. 15, 1939. Samples were collected in dark green bottles and analyzed immediately.

suggested by the character of the diurnal fluctuations shown in Fig. 2. This figure provides evidence that phosphorus may be liberated during darkness by living plankton organisms (Newcombe and Brust, 1940). A similar process might be conceived to occur should the living phytoplankton reach the dark bottom waters in appreciable quantities. The rapidity and magnitude of the changes observed in Fig. 2 are not rationalized on the

usual basis of regeneration as a result of processes of decay and digestion by other organisms. That such a rapid liberation of phosphorus by these processes would occur does not seem probable since, in the ocean, this liberation is known to produce significant effects after the organisms have travelled to relatively great depths (Seiwel 1934, Iglesiasund, Robinson and Thompson, 1936). This indicates a considerable time interval. Even the fact that the summer temperatures of the Chesapeake Bay waters are high (around 25° C) in comparison with the ocean water temperatures, would not likely offset this time factor.

Evidence in support of the hypothesis that the phosphorus change is due to phytoplankton metabolism is provided by the experiments of Gardiner (1937) who studied the effect of typical North Sea plankton, comprising *Calanus finmarchius*, *Temora longicornis*, *Pseudocalanus elongatus* and *Sagitta spp*, on the amount of phosphorus in the water. He concluded that, despite the abnormally high concentrations employed in the experiments, there must be a constant replenishment of the phosphate content of natural waters through the agency of living animals. Again, Renn (1937) in discussing regeneration experiments states "if autolyzing bacterial cells liberate phosphorus as phosphate there always exists the possibility that diatoms and other phytoplankton may similarly bring about direct regeneration." The importance of light and darkness in affecting the absorption of phosphorus by diatoms is clearly shown by the experiments of Ketchum (1939 and 1939a). He worked with the marine diatom *Nitzschia Closterium* and found that these forms when kept in an absence of phosphorus develop a "phosphorus debt." The cells make up this deficiency if phosphorus is provided, a process which may occur in the dark even although the cells do not divide under this condition. It appears, according to Ketchum, that this phosphate so absorbed is united with some cellular constituent which must be formed in photosynthesis even when no phosphorus is available for absorption at that time. Since, in the presence of phosphorus, appreciable amounts are rapidly absorbed and, perhaps, loosely combined within the cell, it is possible that a mechanism may exist within a diatom which could effect a rapid release of this element. This elimination might be sufficiently large to account for the pronounced increases observed during the early evening hours. The question of adsorption of phosphate on the surface of diatoms has been

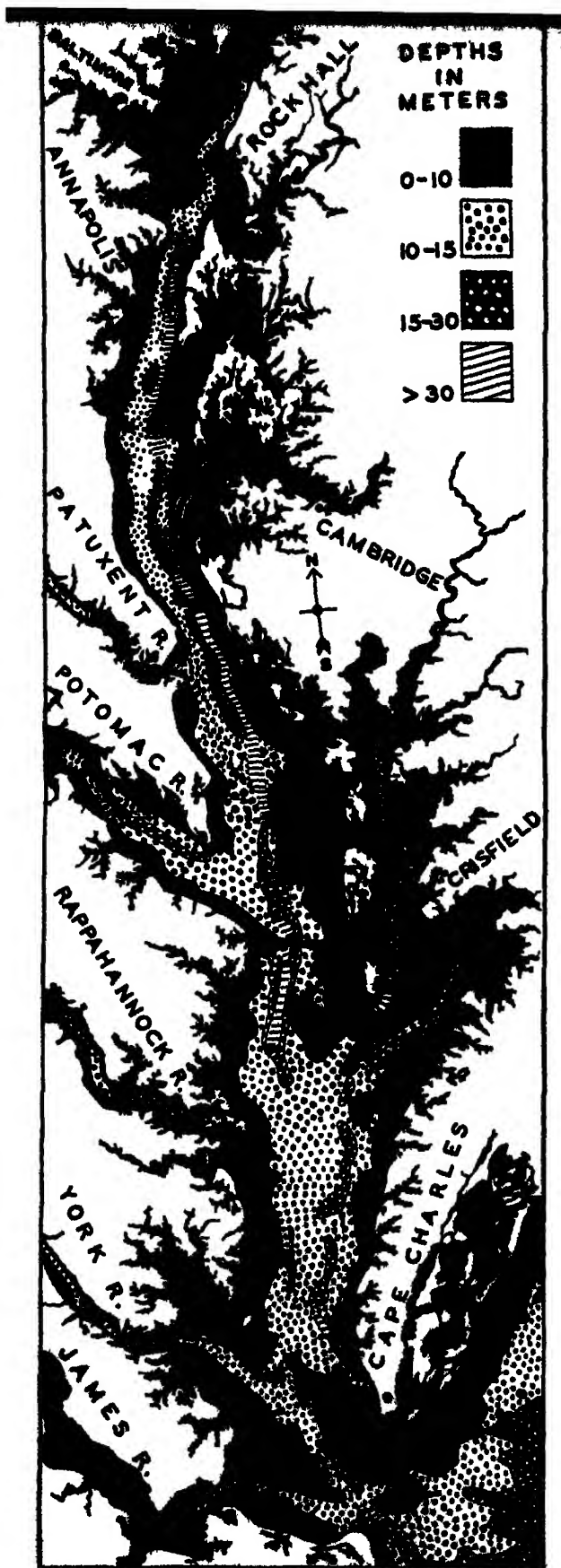


FIG. 4. Bathymetric chart of the Chesapeake Bay (Data from Coast and Geodetic Survey Map).

discussed by Harvey (1937). He points out that colloidal and larger particles of phosphate can be utilized by and support the growth of diatoms.

The suggested hypothesis that the rapid changes in phosphorus concentration pointed out above may be due to metabolic processes of the plankton requires careful experimental study. The course of the rapid phosphorus regeneration during darkness may be a consequence of processes of decomposition, or a regeneration by living phytoplankton or living zooplankton or by some combination of these alternatives. To date, experimental results by Gardiner (1937), Renn (1937), and Ketchum (1939, 1939a) together with the field observations reported here contribute to a much better appreciation of fundamental facts governing plankton metabolism and open up a wide field for investigation.

Thanks are expressed to Professor Herman DuBuy of the University of Maryland for reading the manuscript critically.

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THE COMPARATIVE BEHAVIOR OF MAMMALIAN EGGS IN VIVO AND IN VITRO. VII. FURTHER STUDIES ON THE ACTIVATION OF RABBIT EGGS * †

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(Communicated by Edwin G Conklin)

ABSTRACT

Rabbit eggs were obtained in large numbers by a technic of superovulation, and washed out of the uterine tubes, with sterile solutions. The eggs were exposed to various agents, in order to activate them parthenogenetically. These included hypotonic balanced salt solutions, hypertonic followed by hypotonic solution, exposure to cold. Ova were considered activated if they showed clear pronuclei, or cleavage chromosomes, or underwent true cellular division. Some of the eggs showed excellent mitotic figures (see illustrations) and others segmented into 2, 3 or more perfect blastomeres. The eggs subjected to cooling yielded the greatest percentage of activation.

Attempts were also made to activate the eggs *in vivo*, by exposing a Fallopian tube of an animal under anesthesia, with customary sterile surgical technic, and inserting a loop of the tube containing eggs recently ovulated by the same animal, into a metal jacket, through which cold water could be circulated. The tube and contained eggs are thus brought to the temperature of the water circulating through the metal jacket. This treatment activates eggs *in situ*. The animals were allowed to recover, and go to term. One of the does so treated gave birth to a daughter of normal appearance and behavior.

In this paper we present data on the activating effects upon unfertilized rabbit tubal ova of hypotonic solutions and cooling. Previously it was demonstrated that heat, hypertonic solutions, exposure to foreign sperm, and a certain type of culture of unfertilized rabbit ova led to ovum activation with subsequent development in a limited proportion of treated ova (Pincus 1930, 1936, 1939; Yamane 1930; Pincus and Enzmann 1936).

METHODS

Rabbit ova were obtained after superovulation by pituitary extracts (Pincus 1939, 1940). The eggs were taken from the

* Aided by grants from the American Academy of Arts and Sciences and the Penrose Fund of the American Philosophical Society. (Grant No 278)

† Presented at the annual general meeting of the American Philosophical Society, Philadelphia, Pa., April 20, 1940. Demonstrated at the summer meeting of the Genetics Society of America, August 30, 1940, Woods Hole, Mass.

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Fallopian tubes at 16 to 20 hours after an ovulating injection. All hypotonic solutions used were made so by diluting with glass distilled water. Cooling *in vitro* was accomplished by exposing the ova in rabbit serum in Carrel flasks to room temperature or to refrigerator temperatures.

All ova cultured *in vitro* were incubated at 37° C. in rabbit serum in a Carrel flask (Shapiro, 1939), and were fixed in Bouin's solution after 20 to 24 hours of culturing. Ova were sectioned and stained as previously described (Pincus 1939).

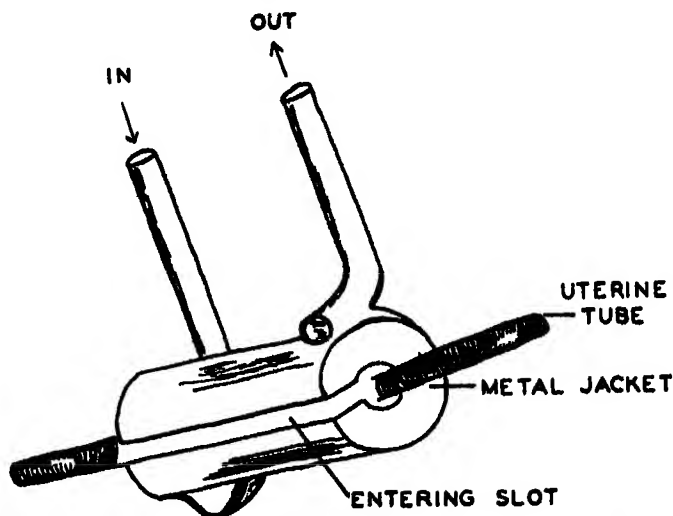


FIG 1 Diagram of the cooling jacket into which the Fallopian tube is shipped, for lowering the temperature of the eggs *in situ* in the Fallopian tube

Cooling *in vivo* was accomplished by flowing ice water through a brass jacket so designed that it would enclose a Fallopian tube over a length of some 3½ cm (Fig 1).

The details of the technic of the *in vivo* activation are as follows. By means of a course of injections of pituitary extract, the doe can be made to ovulate at a predetermined time. The eggs thus shed from the ovary pass directly into the Fallopian tube. The animal is anesthetized by injecting nembutal intravenously, through a marginal ear vein, whereupon unconsciousness supervenes within thirty seconds. After laying the animal on her side on a wooden framework in the sink, the fur is thoroughly moistened in the abdominal region with 70 per cent alcohol, and then a concentrated solution of sodium

sulphide is applied in the same place, with a wad of cotton held in a large hemostat. The hair dissolves away completely in a minute or two, and is quickly washed away with a copious stream of warm water. The washing away of the sodium sulphide is done rapidly, as soon as the hair has dissolved, in order to prevent burning of the skin, which occurs when the sulphide is allowed to remain too long in contact with it. All subsequent procedures are carried out with customary surgical aseptic technique.

After draping the animal on the operating table with sterile towels, anesthesia is completed cautiously with ether, a laparotomy is performed beginning with a dorso-lateral incision, and the Fallopian tube of that side is exposed, and gently brought into place outside the body cavity. Manipulation of the tube is facilitated, and the possibility of trauma, or rupture of delicate blood vessels reduced, by passing a loose ligature through the loop of the uterine tube just beyond the fimbriated end, and another loose ligature beyond the end of the broad portion of the tube. The threads are then used to manipulate the tube containing the eggs into the cooling jacket (Fig 1).

If the bulge containing the eggs is clearly visible, this is placed in the center of the jacket. After about the eighth hour following ovulation, the bulge disappears, as the proteinaceous mass of which it is chiefly composed, and in which the eggs lie, begins to disintegrate. In those cases where the bulge is not plainly visible, an estimate is made of the position of the eggs, and that portion of the tube is accordingly selected, although other portions of the tube may be cooled as well. Cold water is then allowed to flow through the brass jacket, thus lowering the temperature of the eggs. The temperatures of the influent and exfluent streams of cold water in the metal jacket are read and recorded. After the cooling, the Fallopian tube is carefully removed from the jacket, and replaced into the body cavity, and the muscle layers and skin incision separately sewn up with silk thread. A sterile gauze square is then placed over the site of the wound, and collodion used to seal the edges of the gauze to the skin. The doe is then allowed to recover from the anesthesia.

Tubes containing eggs were frozen *in vivo* by the application of solid CO₂, and were held under a strip of rubber sheeting on

which the CO₂ was placed directly. The return of the tissue to body temperature, and reestablishment of the circulation occurred rapidly (within a few minutes), after removal of the dry ice.

RESULTS

The protocols of individual experiments are presented in table 1, in which ova are classified in 7 categories: (1) those showing peripheral meiotic spindles or meiotic chromosomes with no visible spindle fibers (Fig. 2); (2) those showing pronuclei, *e.g.* two large nuclei or a single large nucleus (Fig. 3); (3) those showing scattered chromosomes or numbers of small subnuclei, which arise from the dispersed chromosomes (Fig.

TABLE I

PROTOCOLS OF THE VARIOUS EXPERIMENTS ON RABBIT OVUM ACTIVATION *in vitro**

Date	Treatment	Number of Eggs	Peripheral Chromosomes or Spindle	Pronuclei	Subnuclei or Scattered Chromosomes	First Cleavage Spindle or Cleavage Chromosomes	Cleaved	Uncleaved Fragmented	Cleaved Fragmented
10/ 3/38	50% Van Dyke's solution 5 mins	11	4		1		1	3	2
"	60% Van Dyke's solution 5 mins	3			3				
11/ 1/38	60% Van Dyke's solution 3 mins	4	4						
"	60% Van Dyke's solution 5 mins	15	15						
"	60% Van Dyke's solution 8 mins	6	6						
11/16/38	None	4			3			1	
"	60% Van Dyke's solution 3 mins	4		1			2		1
"	60% Van Dyke's solution 3½ mins	15	14			1			
"	60% Van Dyke's solution 5½ mins	6					4	1	1
11/25/38	None	4	4						
12/17/38	None	4	3		1				
"	60% Van Dyke's solution 3½ mins	3	1			1	1		
"	60% Van Dyke's solution 3½ mins	5	2				3		
	Transplanted recipient found dead								

* Undiluted balanced salt solution or serum is considered as 100 per cent, hypotonic solutions are therefore less (*e.g.* 50 per cent, 60 per cent), hypertonic solutions greater (*e.g.* 180 per cent).

TABLE I—Continued

Date	Treatment	Number of Eggs	Peripheral Chromosomes or Spindle	Pronuclei	Subnuclear or Scattered Chromosomes	First Cleavage Spindle or Cleavage Chromosomes	Cleaved	Uncleaved Fragmented	Cleaved Fragmented
1/17/39	60% Ringer-Locke solution 3 mins	6	2	2	1	1			
"	180% Ringer-Locke solution 3 mins	8	2	2	1	3			
"	180% Ringer-Locke solution 3 mins 60% Ringer-Locke solution 3 mins	20	7	3	3	4	1	2	
"	None	13	6		4	1	1	1	
1/25/39	50% serum 4 mins	12	3	1	4	2	1		1
"	50% Ringer-Locke solution 3 mins	5				5			
"	50% Ringer-Locke solution 3 mins then to equal parts serum and Ringer-Locke 20 mins	9	5		1	1	1	1	
"	170% Ringer-Locke solution 3 mins	16	14				2		
"	50% Ringer-Locke solution 3 mins 170% Ringer-Locke solution 3 mins	9	5		3				1
"	None	10	4		1			5	
2/ 1/39	100% serum, 30 mins	13	5		3			5	
"	100% Ringer-Locke solution 27 mins	22	3	1	12			6	
"	50% serum, 4 mins	13	2	3	1		3	1	3
"	50% serum, 6 mins	16	12			4			
"	50% serum, 8 mins	30	6	6	6	8	3	1	
"	50% serum, 8½ mins	11	5	1	4	1			
2/ 7/39	50% serum, 4½ mins.	2			1		1		
"	50% serum, 8 mins	5		2	1			2	
"	50% serum, 15½ mins	9	1	2	3			3	

TABLE I—*Continued*

Date	Treatment	Number of Eggs	Peripheral Chromosomes or Spindle	Pronuclei	Subnuclei or Scattered Chromosomes	First Cleavage Spindle or Cleavage Chromosomes	Cleaved	Uncleaved Fragmented	Cleaved Fragmented
2/25/39	50% serum, 5 mins	4				2	2		
"	50% serum, 8 mins	8			2	3	1	2	
"	50% serum, 12½ mins	3		1				2	
3/16/39	None	10	3	1	4		1	1	
"	50% serum, 8 mins	17		7	1	3	2	2	2
"	50% serum, 20 mins	25	8		5	4		5	3
"	50% serum, 51 mins	58	6	4	25	6	2	11	4
4/14/39	None	7	1		1	1		4	
"	40% serum, 10 mins	16	1	5	5			3	2
"	40% serum, 60 mins	13		4	6	2	1		
4/21/39	None	11	1		8		1	1	
"	None (not on rocker)	14	1	1	10		1	1	
"	50% serum, 15½ mins	31	12	5	8	4		2	
"	50% serum, 15½ mins (not on rocker)	32	10	4	13	3		2	
"	50% serum, 28 mins	25	7	5	5	7	1		
"	50% serum, 28 mins (not on rocker)	23	14	1	4	2		2	
5/10/39	None	4	1	3					
"	27° C for 3½ hrs	15		7	6		1	1	
6/ 6/39	None	11		1	3			5	2
"	20 2° C, 2 hrs	19		6	2		1	9	1
"	6° C, 10 mins	18	1	3	5		4	4	1
6/12/39	None	12					3	7	2
"	6° C, 30 mins.	11			1		3	5	2
"	6° C, 85 mins	13		4	1	2	1		5
6/27/39	6° C, 15 mins	4		1				3	

4); (4) those showing cleavage spindles or clear cleavage chromosomes with no visible spindle (Figs. 5 and 6); (5) those cleaved into 2 or more regular nucleated blastomeres (Figs. 7 to 10); (6) those fragmented into non-nucleated portions or obviously cytolized, *e.g.* with hyaline or highly vacuolated cytoplasm, (7) those with obvious nucleated blastomeres one or more

portions of which have fragmented (Fig. 11). This last category includes only ova which obviously cleaved normally and then proceeded to degenerate and cytolize. Ova are considered activated if they fall into categories 2, 4, 5 and 7. At ovulation the rabbit ovum contains a set of peripheral chromosomes aligned on a spindle subjacent to the first polar body (Fig. 2)

In table II we summarize the data of table I, grouping the ova into 6 classes (1) those exposed to hypotonic balanced salt solutions (*e.g.* Ringer-Locke and Van Dyke's solutions diluted with distilled water); (2) those exposed to 50 per cent serum, (3) those exposed to 170 per cent to 180 per cent Ringer-Locke solution, (4) those placed for 3 minutes in hypotonic and then for 3 minutes in hypertonic solutions, (5) those exposed to sub-normal temperatures, (6) control ova placed in culture at the same time as the experimental ones

It will be noted that in all classes except the ova treated with hypertonic solutions all 7 types of effects are noted. We were particularly struck by the fact that the control cultures showed cleaved ova. This indeed was what originally led us to the cooling experiments. These experiments were conducted in a basement laboratory which had room temperatures of 20°–27° C. A period of one to more hours might ordinarily elapse between the sacrifice of the doe and the incubation of the ova. Our control cultures do indeed show the lowest percentage of activated ova, but we are inclined to attribute much of this proportion to the cooling that must have occurred, especially in view of the high activation percentages obtained in the experimentally cooled eggs. Thus (table I), of 19 ova left in serum at 20° C for 3½ hours, 8 were activated and 1 cleaved.

As these data indicate, cooling results in activation, hence a method of approach to artificial activation *in vivo* is available. These experiments now to be described differ from the earlier parthenogenesis experiments (Pincus, 1936, 1939) in that the eggs were not removed from one female and transplanted to a host mother, after they had been activated *in vitro*. Instead, the doe which shed the eggs was laparotomized, and activation of her eggs took place in her uterine tube, as described in the section under methods, of this paper.

In one series of experiments, in which the animals were not allowed to go to term, the methods given above were employed

TABLE II

SUMMARY OF THE EFFECTS UPON UNFERTILIZED RABBIT EGGS OF HYPOTONIC SOLUTIONS, HYPERTONIC SOLUTIONS, AND EXPOSURE TO COLD

Treatment	(1) Num- ber of Eggs	(2) Peripheral Chromosomes or Spindles	(3) Pronuclei	(4) Subnuclei or Scattered Chromosomes	(5) Cleaved Chromosomes	(6) First Cleavage Spindles or Chromosomes	(7) Uncleaved Frag- mented	(8) Cleaved Frag- mented	(9)* % Activated	(10)* % Cleaved
Hypotonic balanced salt solutions	92	53	2	7	12	9	5	4	29.3	17.4
Hypotonic serum	353	87	51	94	17	51	38	15	37.9	9.1
Hypertonic solutions	24	16	2	1	2	3	—	—	29.2	8.3
Alternating hypertonic and hypo- tonic solutions	29	12	3	6	1	4	2	1	31.0	7.0
Cooling	80	1	21	15	10	2	22	9	52.5	23.8
Controls, no treatment	139	32	7	49	8	3	36	4	15.8	8.6

* The data of column nine are calculated by dividing the sum of columns 3, 5, 6, and 8 by the total number of eggs as given in column 1, the data of column 10 by dividing the sum of columns 5 and 8 by the total egg number given in column 1

to cool the right Fallopian tubes of four females. The first, killed at the second day after cooling, yielded one uncleaved ovum from the cooled side, though five were ovulated, but from the uncooled side no ova were recoverable, though it had four ovulation points. The second was killed at five days after cooling and of eight ova recovered from the right oviduct one was a morula, five uncleaved or fragmented and one a collapsed blastocyst (Fig. 12). The left oviduct (not cooled) yielded six uncleaved degenerate ova. Two others sacrificed at 20 and 21 days respectively after the operation had no eggs nor embryos, but one had a resorption site in the right uterus indicating presumably an abortive implantation.

Sixteen other animals underwent the Fallopian tube cooling operation and were allowed to go to term. The results of these experiments are given in Table III. One of the animals, whose

TABLE III
THE EFFECTS OF COOLING THE RIGHT FALLOPIAN TUBE CONTAINING FRESHLY
OVULATED OVA

Number of Rabbits	Period of Cooling (Mins)	Results
1	5	No young
2	10	No young
7	15	No young
2	20	One gave birth to one living female
4	Frozen with solid CO ₂ 2 to 10 mins	No young

tube was cooled 20 minutes, gave birth to a parthenogenetic daughter (Fig. 13). Since these rabbits should have ovulated 12 to 15 ova on the cooled side (Pincus, 1940), and this was checked in some instances by counts of ovulation points, it follows that one egg in some 200 developed into a living parthenogenetic rabbit. This is less than would have been expected if all ova that cleaved developed normally (tables I and II). Hence the majority of such eggs activated *in vivo* must ultimately undergo degeneration at some stage or other.

DISCUSSION AND CONCLUSIONS

The various methods of artificial activation employed *in vitro* lead to demonstrable parthenogenetic development in only a certain proportion of ova. Employing the method of cooling

(which gave the best results *in vitro*) *in vivo* led apparently to no increase in the number of parthenogenetic ova. The cooling method we employed *in vivo* is, however, attended by several uncertainties and difficulties. The exact placing of the cooling jacket is difficult and at times involves trauma of the Fallopian tube and adjacent tissues if the width of the slot is not correct. Furthermore, we were not always certain that we had correctly located the mass of ovulated ova owing to the separation of the protein mass from the eggs. When this occurs the "bulge" in the Fallopian tube is no longer apparent. Finally, the exact temperatures of the cooled ova are not known, though our incoming ice water ordinarily was at 3° to 6° C. and the egg temperatures must have been close to these figures, as the volume of blood flowing through the tissue must have been small compared to the amount of water circulated through the cooling jacket. Ingoing and outgoing temperatures differed by only a few degrees at most. The efficacy of the activation by cooling should be tested with still larger numbers of ova, and transplantation of ova activated by cooling *in vitro* should be attempted, as an interesting comparison. Nevertheless, despite these difficulties and uncertainties, the method has the all-important advantage of enabling activation of eggs *in vivo*.

The types of activated ova encountered in these experiments are like those previously described (Figs. 2 to 12, and Pincus, 1939). Numerous "good" mitotic cleavage spindles have been observed. The frequency of fragmented ova seen indicate either that our technic of handling and culture is still not optimal or that the activation procedures are "damaging."

Concerning the chromosomal constitution of the one female parthenogone produced by cooling the eggs *in vivo* in the Fallopian tube, nothing definite can be said at present. Cytological examinations of artificially activated rabbit eggs (Pincus, 1939) show that they may be haploid, diploid, or tetraploid. Any one of these might conceivably yield a viable organism. The production of these different types, it is evident, might arise in a number of ways, such as suppression of a polar body, or nuclear division without cytoplasmic division.

In eggs of *Rana* induced to cleave parthenogenetically by pricking (Parmenter, 1940) diploid embryos were found in at least 68 out of 91 diploids in which the eggs were delayed about

one cleavage period during the first cleavage. He states that diploidy may have been brought about during this delay.

Conklin (1938) observed that the most effective temperature for causing cytological disorientations in the early development of the marine gasteropod *Crepidula plana*, lay between -1° and 5° C. He exposed the fertilized eggs for periods of 2 to 20 hours. Numerous variations of normal cytological patterns were observed. One case of suppression of the first cleavage furrow (Fig. 13, l.c.) is illustrated, and in Fig. 16 (l.c.) the details are shown of an instance in which the first cleavage furrow was suppressed, but the nucleus divided, leaving both daughter nuclei in one macromere. At the second cleavage, each of these nuclei divided, giving rise to two micromeres which were not in contact. In our experiments, however, cold was used as an agent for initiating mitotic activity and cleavage, the greatest part of which effects must have occurred after the eggs were allowed to return to body temperature.

The first use of cold as an agent for initiating cleavage appears to have been made by Morgan (1900), in which eggs of the sea urchin *Arbacia* were made to segment by lowering the temperature of sea water to the freezing point. The segmentation induced was irregular, it appeared in only a small percentage of eggs, and the embryos did not develop very far.

In mammals, cases of rudimentary parthenogenesis (i.e., eggs capable of undergoing a certain amount of parthenogenetic development, which halts at any stage) are not uncommon. However, it has been only in the earlier discoveries of Pincus (1939), and in the experiments here described, that viable young, capable of growing to sexual maturity, and reproducing, have been reported. The findings on rudimentary parthenogenesis in mammals have been summarized by Vandel (1931).

L. Loeb (1911) described embryos developing parthenogenetically within the ovary of the guinea-pig, a previous fertilization of the ovarian ovum could be excluded. Stages as advanced as chorionic vesicle with trophoblast, and plasmodia and syncytia penetrating into the neighboring tissue, were found. These cases of parthenogenetic development were relatively frequent, appearing in about 10 per cent. of all guinea pigs below six months of age. However, it was not found possible to produce them experimentally.

In the human species, cases of what appear to be rudimentary parthenogenesis have been recorded. In women who had died of puerperal peritonitis, eight to ten days after confinement, C. Morel (1864, cited from Vandel) found in the hypertrophied Graafian follicles, segmented eggs divided into about 50 blastomeres. The eggs had undergone fatty degeneration. It is also believed that certain dermoid cysts of the ovary may arise from parthenogenetic development of eggs within the ovary.

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PLATE I

FIG 2 Rabbit ovum as ovulated. Note polar body and meiotic chromosomes.

FIG 3 Two pronuclei in egg that had been in 50 per cent rabbit serum for 28 minutes. This and all succeeding photographs represent steps of mitotic activity induced in the ovum by an activating treatment.

FIG 4 Egg with "subnuclei." Hypertonic solution 3 minutes followed by hypotonic solution 3 minutes (all Ringer Locke).

FIG 5. Polar aspect of typical mitotic cleavage spindle in egg that had been in 50 per cent rabbit serum for 8 minutes.

PLATE I



PLATE II

Fig 6 Typical 1st cleavage anaphase in egg that had been in refrigerator for 8 minutes

Fig 7 2 cell stage Treatment, $\frac{1}{2}$ hour of refrigeration

Fig 8 3 cells of a 4 cell stage Treatment, 10 minutes at 6° C

Fig 9 Typical 6 cell stage (only 5 cells seen in this section) Treatment, 28 minutes in 50 per cent rabbit serum

PLATE II



PLATE III

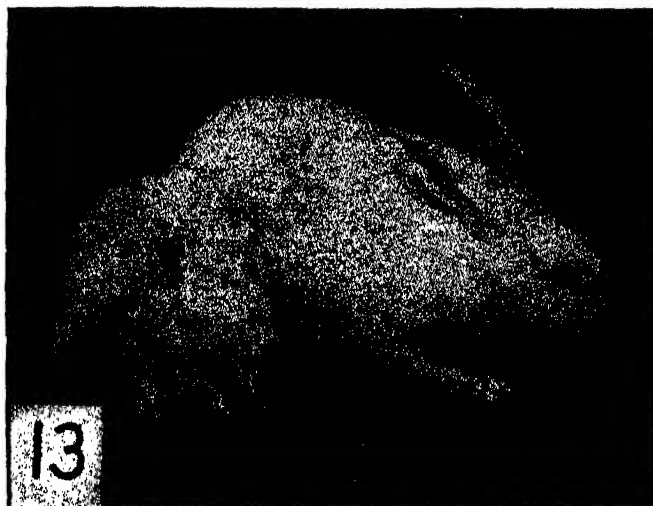
FIG 10 Typical cleavage anaphase in 1 cell of a 4-cell stage. Control (probably cooled at room temperature for 1-2 hours). Serum used had been previously refrigerated.

FIG 11 Metaphase (mitotic) in egg previously refrigerated for $\frac{1}{2}$ hour. This egg cleaved into two cells (note part of nucleus of other blastomere), and then fragmented.

FIG 12 Collapsed blastocyst removed from animal whose oviduct had been cooled 5 days previously.

FIG 13 Albino rabbit which had eggs cooled *in vivo* in uterine tube, and gave birth to parthenogenetic daughter shown at the left. Age of mother at time of photographing was 12 months, and of daughter $2\frac{1}{2}$ months.

PLATE III



REVIEW OF THE MAMMAL-BEARING TERTIARY OF SOUTH AMERICA

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ABSTRACT

A century of study has revealed a long sequence of mammal bearing strata in South America, but most of the details are still in dispute, innumerable gaps remain, and an adequate synthesis has not been made. Knowledge of earlier Tertiary mammals is almost confined to Central Patagonia. The later Tertiary is best known from elsewhere in Patagonia and other parts of the Argentine, especially the northern pre-Andine zone. The physical history revealed by these strata is summarized and a consistent nomenclature is proposed, distinguishing rock, time, and faunal units. The faunas themselves are more briefly reviewed and it is concluded that the basic fauna was probably of North American origin but, except for adventitious arrivals, developed in isolation until reunion with North America in the late Pliocene. There is no firm basis for world wide correlation, but the evidence is most consistent with an arrangement approximately intermediate between such extreme views as those of Ameghino, on one hand, and Frenguelli, on the other. Known occurrences of South American Tertiary mammals outside the Argentine are scanty and mostly from the later Tertiary but are well scattered in six countries. Their main value is in demonstrating that South America, including its northern part, was a faunal and geographic unit long before the late Pliocene.

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INTRODUCTION

It is now well over a century since mammalian paleontology in South America began with the discoveries of Darwin and of d'Orbigny. During this time, thousands of specimens have been collected, making known representative faunas of all the Tertiary epochs. Descriptions of these fossils and of their stratigraphic occurrences have been written in every learned language and are so voluminous that it now takes years simply to read the important literature. A review of this vast body of knowledge, conjecture, and theory is a task that must be approached with humility and with the realization that the result is sure to have many shortcomings.

In such a review it is impossible to trace all the essential steps in the development of ideas and no general historic summary of these studies is attempted. Even adequate summaries of all the theories and opinions currently maintained are not possible in less than the scope of a volume, for it is characteristic of this subject that controversies have constantly raged over most of its many phases and that they still do. A review such as is here presented cannot, then, be an impartial abstract and synthesis of the literature but necessarily involves much selection and the expression of personal opinion. It can, however, be useful in giving the broader outlines of what is known and a reasonable (even though not the only possibly valid) interpretation of these facts.

This review is written primarily from a stratigraphic point of view, with a minimum of biological or strictly paleontological details. It is confined to rocks in which land mammals surely or probably of pre-Pleistocene age occur, and to their mammalian faunas. This means that it is devoted almost entirely to regions now included in Argentina because, as will be shown, discoveries of Tertiary land mammals elsewhere in South America have been few and relatively unimportant. Although several North Americans and a few Europeans have worked in this field in Argentina, the great bulk of the work has been by Argentines, by birth or by adoption.

A firm and comprehensive basis was laid by the Ameghino brothers, Carlos in the field and Florentino in the office (see F. Ameghino's reprinted works, 1913-1936), whose splendid accomplishments have not always been justly appraised outside the

Argentine. Like any work completed a generation ago, this requires critical revision on various points, but it is striking how little basic progress has yet been made beyond the point reached by the Ameghinos and that much later work has fallen into errors avoided by them. Despite voluminous studies, advance in knowledge since Ameghino's stratigraphic summary of 1906 has been slow and mostly concerned with details rather than with the most important points. Any competent review must to some extent reveal how profound our ignorance still is on many essentials, including some that could quite simply have been solved with no more work than has been wasted on fruitless polemics. A reason for this unsatisfactory condition is that field studies have so often been made by geologists who were not able paleontologists, and who often paid no attention to fossils at all, and faunal studies by paleontologists who were not able field stratigraphers or had never themselves been in the field. There has been little correlation of the two essential approaches to the subject. This has been an unfortunate general rule, with some happy exceptions.

The best general work on Argentine geology is that of Windhausen (1931) and the best summary of Argentine fossil mammals is in Scott (1937). The list of references at the end of this paper and the bibliographies of papers there cited give access to most of the detailed studies.

I am indebted to Mr. Bryan Patterson of the Field Museum of Natural History, Chicago, for reading this manuscript and offering many valuable suggestions.

REGIONAL GEOLOGIC CHARACTERS OF ARGENTINA

Southern South America can be clearly divided into ten physiographic, structural regions, of which eight are well represented in Argentina (Fig 1). The Brazilian Shield, a peneplaned area of old rocks, forms much of Uruguay but is scarcely represented in far northeastern Argentina.

Northern and northeastern Argentina are mainly included in the great Chaco-Pampa region, low-lying plains mostly formed by little consolidated sediments late Miocene to Recent in age. To the west and south several isolated masses of folded, older, harder rocks rise like islands above the pampa, constituting the Pampean Sierras. The pampa and the chaco are bounded on

the west by the Andes which, in about the latitude of Buenos Aires, are divisible into three structural zones, each trending north and south, the Precordillera, in western Argentina, the Andine Cordillera, proper, along the Argentine-Chilean frontier, and the Coastal Cordillera, in Chile. In northwestern Argentina and extending northward into Bolivia and Peru the Precordillera and the Andine Cordillera diverge and between them is the broad Puna, a high region of relatively little local relief.

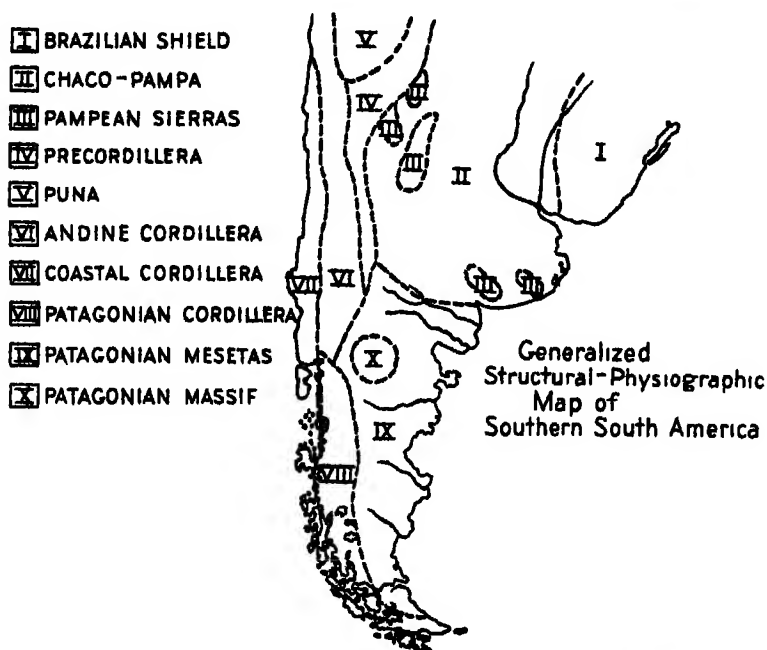


FIG 1 Sketch map of the southern end of South America, showing the major physiographic regions of the Argentine and adjacent territory

The extreme southern part of South America comprises Patagonia, an unofficial regional name sometimes taken to include southern Chile but now more often confined to the wider region in Argentina, north of the Straits of Magellan. Western Patagonia, with most of southern Chile and southern Tierra del Fuego, is formed by the Patagonian Cordillera, a southern continuation of the main Andine Cordillera but lower and somewhat different in structure. East of this is the Patagonian Meseta region, mostly of relatively flat-lying Tertiary and Mesozoic sediments and volcanic rocks, forming high table-lands and ter-

races cut by steep watercourses and with some structural basins. In the north-central part of the meseta region is a rugged nucleus of older igneous rocks sometimes called the Patagonian Massif

To the student and collector of fossil mammals this physiographic structure has direct importance because each region includes characteristic and different parts of the sequence, although the areas of different fossil mammal occurrences do not exactly correspond with the physiographic regions (Fig 2). With few exceptions, early fossil mammals, Paleocene through Oligocene, are found only in the central part of the Patagonian meseta region. The next younger stage, typified by the Santa Cruz formation with the richest, best-known of all South American Tertiary mammalian faunas, occurs mainly south of these earlier deposits, in the southern meseta region and westward to the margins of the Patagonian cordillera

Later Tertiary mammal-bearing deposits, approximately late Miocene through Pliocene, are much more widespread. The principal regions in which they occur are (a) the eastern slopes and adjacent pre-andine zone of the central and northern Patagonian cordillera, (b) similar situations in the eastern Andine cordillera and precordillera northward to Bolivia, (c) a peripheral zone along the western and southern margins of the pampa, (d) the mesopotamian zone of the chaco-pampa region, between the rivers Paraná and Uruguay, and (e) relatively unimportant occurrences in northeastern Patagonia

Pleistocene mammals occur sporadically almost everywhere but are best known from the great sedimentary sheet of the Pampean group, forming most of the pampa.

EOGENE STRATIGRAPHY

With unimportant exceptions all the known early Tertiary mammals of South America come from central Patagonia, over most of Chubut Territory and the northern part of Santa Cruz Territory. The majority are from a region averaging about 400 kilometers in length, north to south, between the Chubut and Deseado valleys, and about 250 kilometers in breadth, from the coast around the Gulf of San Jorge inland to the western part of the meseta region. The oldest rocks exposed here, in small areas north and south of the gulf, are porphyries believed

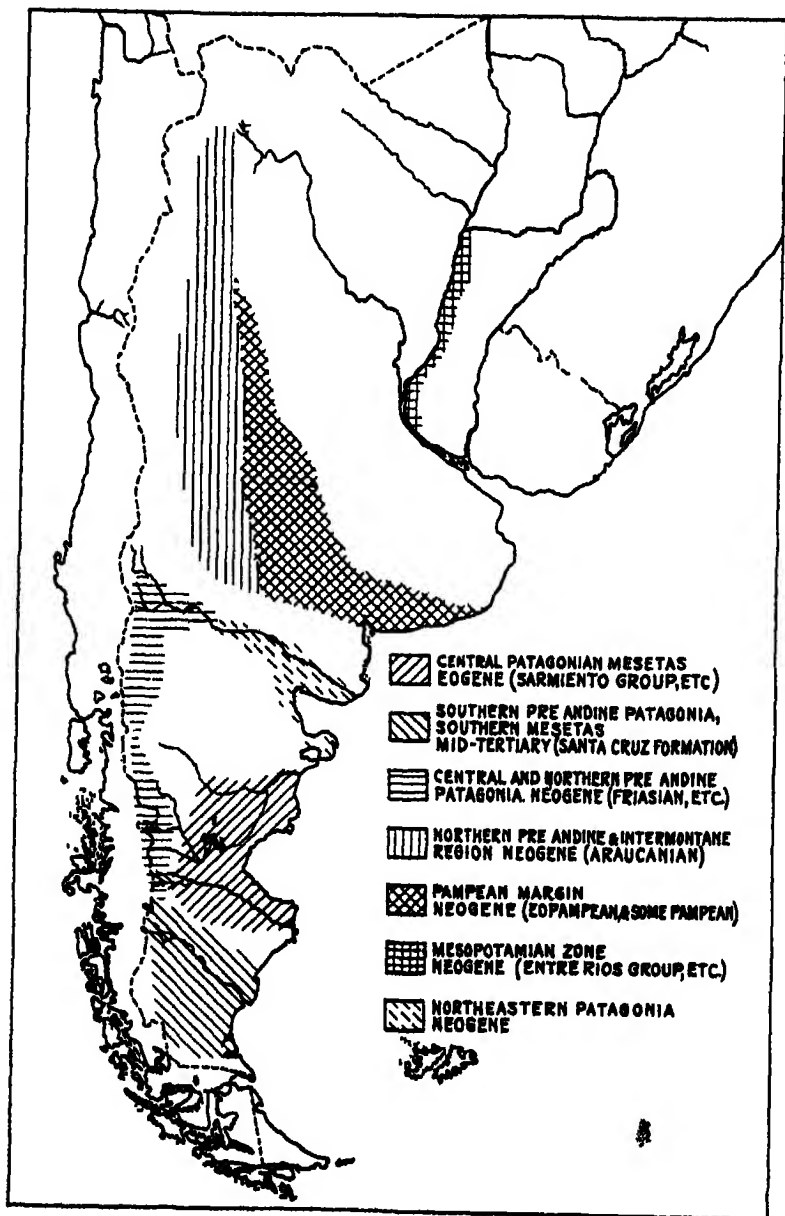


FIG 2 Sketch map of the southern end of South America, showing the principal regions of the Argentine in which fossil mammals have been found. This is not a geological map, but a map of general collecting fields, as explained in the text.

to be Triassic and Jurassic in age. On these lies a very thick and complex pile of sediments, mainly of continental deposition but with at least three marine intercalations. The older sediments, exposed in a few small folded ranges like the Sierra San Bernardo and in less disturbed zones that are relatively low topographically and high structurally, form a thick series mainly of tuffs and sandstones, brilliantly variegated in color and thence called by Ameghino the "grès bigarrés" or "areniscas abigarradas." They are now classified as the Chubut group, with various local subdivisions. They contain dinosaurs and are of Upper Cretaceous age.

Near the coast these rocks are overlain by the Salamanca formation, which is largely marine in origin. In northern Patagonia there is another marine formation in about this part of the sequence, the Roca, and it has long been disputed whether these two formations are of the same age or, if not, which is older. Recent studies (especially Feruglio, 1937) seem to establish beyond much doubt that the Salamanca is distinctly younger, Danian or possibly Montian, and so almost on the Cretaceous-Tertiary limit, a very important datum plane. I believe that the Salamanca is probably early Paleocene and the Roca late Cretaceous, perhaps late Senonian. The Salamanca has littoral facies with crocodiles and turtles, but no mammals or dinosaurs have been found in it.

Above the Salamanca where that occurs and above the Chubut where it does not, there is a group of sands and clays the relationships of which have only recently been grasped. They were long supposed to represent the terminal part of the Chubut group, as reflected in their being called "dinosaur sandstones" (e.g. Roth, 1908), "upper dinosaur beds" (e.g. Windhausen, 1924), "Pehuenche" (e.g. Feruglio, 1929), etc. The type Pehuenche (Doering, 1882) is a truly dinosaur-bearing formation in northern Patagonia, equivalent to part of the Chubut group.¹ The central Patagonian beds erroneously correlated with the Pehuenche were believed to contain dinosaurs and often regarded as contemporaneous with or, in places, as earlier than the Salamanca. It is now well established that they are somewhat later than the Salamanca, that they do not contain dino-

¹ Feruglio (1938b) continues to call these central Patagonian beds "Pehuenche" even though he now recognizes that they have nothing to do with the type Pehuenche. Most others have abandoned this misleading nomenclature.

saur, that they do contain mammals, and that they are of Tertiary age (Feruglio, 1931, Piatnitzky, 1931, Simpson, 1935b). I have named them the Río Chico formation. Their mammals are the oldest known from South America, various reports of Mesozoic mammals in the Argentine all being unreliable or incorrect (Simpson, 1932).

Above the Río Chico there is a sudden change in lithology and the following series is composed almost exclusively of tuffs and clays. These clays are all bentonitic (McCartney, 1933) and were manifestly derived from volcanic ash katamorphically, so that the material of these sediments is almost entirely pyroclastic in origin.

In some places below this tuff series there are bright-colored harder strata, the "argiles fissilaires" of Ameghino (although they are not clays and not fissile), recently named Koluel-Kaike tuffs by Feruglio (1938b). They are opalized tuffs (McCartney, 1934) and apparently are merely a metamorphic facies of the normal tuffs. I do not believe that they represent any time not elsewhere represented by normal tuffs, so that they are purely a lithologic formation, not representative of a distinct time stage. No mammals have been found in them and they are mentioned here only because some of the most readily available literature (*e.g.* Windhausen, 1924) stresses them as a terminal Cretaceous formation and claims that they underlie the Tertiary with strong angular unconformity. Since the type Koluel-Kaike is now known surely to be Tertiary, this is obviously not the period boundary. This supposed tectonic phase with folding, the presumed first phase of Andine orogeny, is a will-of-the-wisp that has never been dated, in spite of very positive statements in the literature. The dating depends on the identification of angular unconformities in the fossil-bearing series of the meseta region, and so far all the evidence for sharply angular unconformities in this part of the series has proved to be either doubtful or definitely erroneous on closer study. The very existence of this supposed tectonic phase is not surely established.

The Eocene tuff series as a whole has received various names of which "Sarmiento tuffs" (Feruglio, 1938b), although the most recent, is the only one that is unequivocal and acceptable. Despite its relatively uniform source materials and the tendency

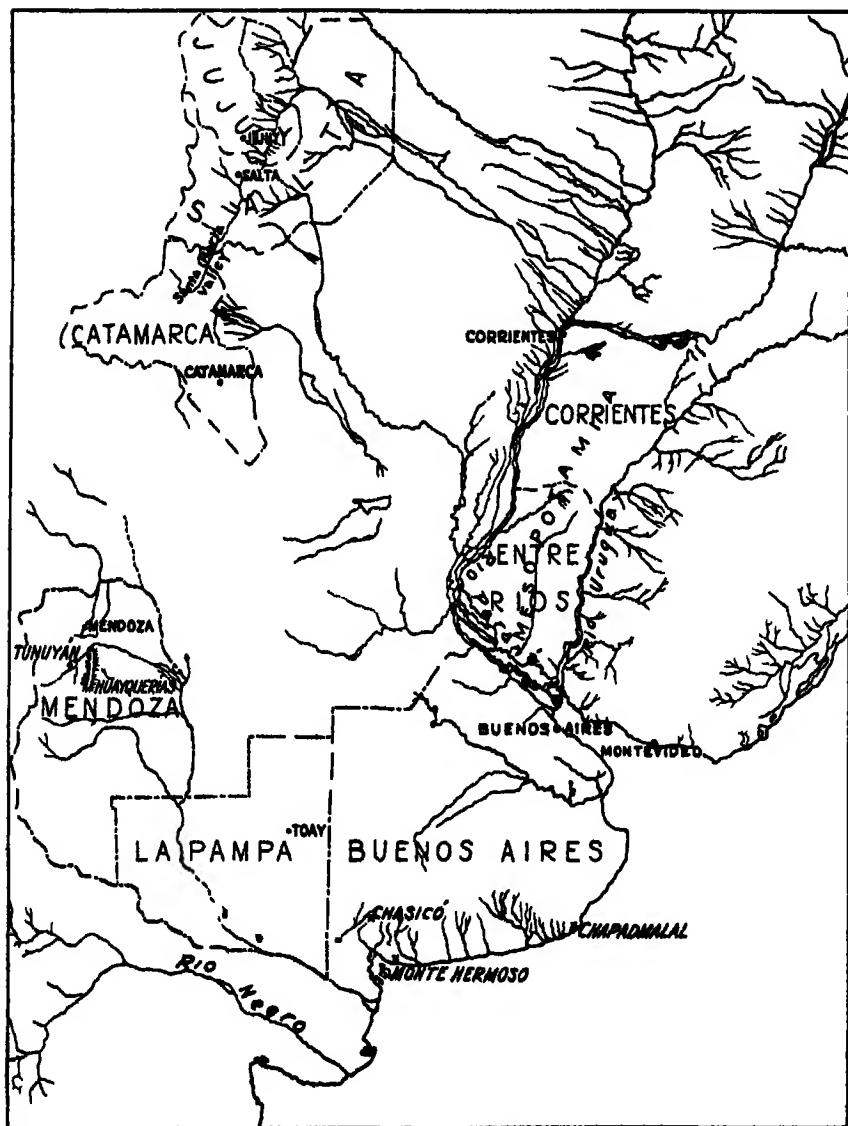


FIG 4 Map of northern Argentina and some adjacent territory, showing type localities of mammal bearing formations and other important localities mentioned in the text

of many geologists to refer to it as a unit, this series is really highly varied in lithology and very complex in internal structure. It covers most or all of two Tertiary epochs and is far from being unified.

In this series the great collector C Ameghino found four distinct faunas. His idea of this sequence has been challenged repeatedly (*e g.* Hatcher, 1897, Loomis, 1914), but it was certainly correct. Indeed at one locality, the high barranca (cliff or steep slope) south of Lake Colhué-Huapi,² all four faunas occur superposed in a single sequence and the type descriptions of three of them were based, for the most part, on material from this locality. F Ameghino (*e g.* 1906) named these faunas for typical genera, in ascending order the *Notostylops*, *Astraponotus*, *Pyrotherium*, and *Colpodon* faunas. The rock units from which these come were named for these genera, "Notostylo-pense," *Astraponotense*," etc, names also used for time units in correlation. These have since given way to geographic names developed by Gaudry (1906, following work by Trounouer, 1903), Kraghevich (1930), Friengueli (1930a), and others (see Simpson, 1933), usually given in Spanish with the suffix *-ense*. I now propose to use the unmodified geographic name for rock units and to suffix *-an* to form the names of standard time units. The sequence is, then, as follows.

Fauna (Ameghino)	Typical Rock Formation	Time Unit (Stage)
<i>Colpodon</i>	Colhué-Huapi	Colhuehuapien
<i>Pyrotherium</i>	Deesado	Deesadan
<i>Astraponotus</i>	Musters	Mustersan
<i>Notostylops</i>	Casamayor	Casamayoran

As they occur south of Lake Colhué-Huapi and at most other localities where they have been clearly identified, there is a well defined plane of erosion between each successive two of these formations. The typical faunas also show unmistakably that there is a marked hiatus in time between each two faunas. It is probable that various local faunas will narrow and perhaps eventually remove these hiatuses. Until now these local faunas have seldom been clearly distinguished as such and their sequence has not been established, almost all publications simply referring material in hand to the one of the four faunas that it most resembles, without further or precise details. The central

² The official spelling, but the only local pronunciations are "Coluapi" or "Coli uapi," pronounced as in Spanish and accented on the *a*, not the terminal vowel.

part of Chubut, rich in fossils but largely unexplored as yet, will probably add greatly to knowledge in this respect. One of the exceptions to this general failure to advance the subject beyond the point reached by Ameghino thirty-five to forty years ago is the recognition of the Trelew formation and fauna in the Chubut Valley, belonging to the Colhuehuapian stage but slightly later than the type Colhué-Huapí formation (Bordas, 1939, Simpson, 1935a). It is hoped that work now in hand at the Field Museum, Chicago, and the American Museum, New York, as well as studies continuing in the Argentine, will greatly advance this important work.

Beds of Casamayoran age are thicker than the other Eocene formations in most sections and also appear to be most widespread, a thick mantle of successive ash falls over all of central Patagonia and probably far beyond. Locally there are fluvial, lacustrine, and other diverse facies. The deposits seem to have rested on an almost flat lowland. The beds of Mustersan age are the most restricted in known extent and usually appear as local channel deposits, frequently incised in the Casamayor. Uplift and erosion were evidently increasingly active, because the Musters was not deposited or was entirely removed in many localities and the Deseado often rests directly on the Casamayor. Deseadan time, or probably more strictly the Deseadan-Colhuehuapian hiatus, represents the climax of this regional uplift, and the Deseado formation, although far more widespread than the Musters, is also often represented by channel facies and was deposited on a surface with considerable erosional relief. The materials of all these formations are volcanic, but in the Casamayoran and Mustersan there is little evidence of local vulcanism in the meseta region. The ash evidently came from volcanoes farther west, in what is now the Andine region, which had been periodically active during the Cretaceous and became more so at the beginning of Casamayoran time. There are, however, numerous ancient, extinct volcanoes in the meseta region, as well as intrusive lavas and fissure flows, and the majority of those that have been dated are of Deseadan age.

The first unequivocal, well marked tectonic phase in the meseta region, with local folding and extensive faulting, is post-Deseadan. I have found no locality where it can surely be dated with respect to Colhuehuapian time, but it is pre-Patagonian

and there is no apparent hiatus between the Colhuehuapian and the beginning of the Patagonian marine invasion, so that this tectonic phase is almost surely to be placed in the Deseadan-Colhuehuapian hiatus. It probably corresponds with the "second tectonic phase" of Groeber (*e.g.* 1927), although this is often assigned a later date, on evidence less definite than that (in part unpublished) on which this paragraph is based. This phase appears to mark the beginning of the definitive uplift and structural formation of the southern Andes, which has continued rhythmically and periodically ever since with occasional climaxes in the later Tertiary.

Despite this important event, the few known deposits of Colhuehuapian age are lithologically similar to the earlier tuffs and are customarily included in the heterogeneous "Sarmiento tuffs". The whole region was widely and deeply eroded after the more important tectonic movements and sank, relative to sea-level. Colhuehuapian deposits were formed locally in depressions on the sinking land and in places can be seen passing without noteworthy unconformity into the overlying marine beds of the Patagonian group.

Outside this central meseta area in Patagonia, Roth (*e.g.* 1908) and others have recorded the presence of tuffs supposed to be pre-Patagonian in age and so perhaps equivalent to part of the Eocene series here discussed, but almost nothing reliable is known either of their stratigraphy or of their faunas so that these occurrences, probable as they are, are still largely hypothetical. This is true of the Tecan, or Teckan ("Tequén," "Tequense") stage inserted by Ameghino, and some others following him, between the Deseadan and the Colhuehuapian. This is based on an isolated discovery by Roth on the Río Teca, or Tecka, in western central Patagonia, the true age and nature of which are still quite unknown.

Near Curuzucuatia, in Corrientes, northeastern Argentina, Podestá³ found a fossil that he named *Ameghinotherium* and that may be a typotherium of Eocene character. On the strength of this, Kraglievich (1934) questionably inserted a "Castillense" stage between Deseadan and Colhuehuapian, but this is even more dubious than the "Tequense".

³ I have not seen Podestá's obscure publications, but his description is summarized in Ameghino, 1899.

The accompanying table summarizes the Eogene physical history of the meseta region of Patagonia.

NEOGENE STRATIGRAPHY

Without insisting on precise equivalence to the Eogene-Neogene division in Europe, it is convenient in Argentina to consider post-Colhuehuapian Tertiary deposits as Neogene

Southern and Western Patagonia

The thick mid-Tertiary marine Patagonian group seems to be divisible into two successive formations, the Patagonian, proper, and the Superpatagonian of Ameghino, with the per-

Typical Formations	Sedimentation	Vulcanism	
Patagonian	Marine		
Colhué-Huapi	Deposits in depressions as sea began invasion	Materials almost entirely volcanic ash	Climax of local vulcanism with numerous small vents, flows local explosive eruptions
(Significant but smaller hiatus)	Pronounced erosion, cutting deeply into underlying beds		
Deseado	Mainly valley and basin deposits on rolling land		
(Greatest hiatus)	Erosion		
Musters	Mainly localized channel deposits		
(Hiatus)	Erosion		
Casamayor	Widespread, fairly uniform deposition on emergent lowland	Materials almost entirely volcanic ash	Intense regional activity, almost continuous falls of ash, probably mostly from the west
Río Chico	Widespread but less uniform than Casamayor, materials from erosion of older strata		
Salamanca	Marine, brackish, to coastal and estuarine		
Chubut	Widespread but highly varied mainly fluvatile		

Typical Formations	Tectonics		Strand-line
Patagonian			Wide transgression westward
Colhué-Huapi	Land sinking		
(Hiatus)	Maximum local uplift, with mild local folding	Rhythmic gentle uplift without definite folding	Far to east of present coast
Deseado			
(Hiatus)	Accentuated uplift		
Musters	Accentuated uplift		
(Hiatus)			
Casamayor			
Río Chico			
Salamanca	Gentle oscillation down, then up, with no marked, tectonic phase at Cretaceous-Tertiary boundary		Limited transgression westward
Chubut			Slightly east of present coast

haps equivalent "Aonikense" A few fragmentary land mammals of Colhuehuapian type have been found in the lowest Patagonian strata. Ameghino (*e.g.* 1906) subdivided the Patagonian group into six parts and, with one exception, considered each equivalent to a non-marine formation. The Patagonian proper was given four subdivisions, of which the first three were said to be equivalent to the Teca beds, the *Colpodon*-beds, now called Colhué-huapi, and the *Astrapothericulus*-beds named "Pinturense" (*i.e.* Pinturas formation) by Castellanos (1937). As mentioned above, the Teca beds are too dubious for consideration. The Colhué-Huapi everywhere underlies the base of the Patagonian, but with no marked hiatus so that it is possible that the earliest Patagonian beds do in part overlap the latest Colhuehuapian in time, although this is not established as a fact.

The Pinturas beds and their *Astrapothericulus* fauna are wholly dubious as to age and relationships and do not yet deserve a definite place in a standard series for the region. They are represented by isolated deposits (or perhaps only one) in western Santa Cruz Territory, southern Patagonia, and according to Ameghino cannot be placed stratigraphically in relation

to any other mammal bearing beds or to the Patagonian marine beds, their correlation with the upper part of the latter being based on indirect and theoretical considerations of doubtful force. The scanty fauna has never been revised and has not been added to in the present century. The list given by Ameghino shows that the beds belong in about this part of the series, but does not prove them to be later than Colhuehuapian or earlier than Karaikenian, the next dubious time stage, so that the insertion here of a "Pinturan" stage by Castellanos (after Ameghino's "Astrapothericuléen") is still premature.

Ameghino correlated the basal Superpatagonian with his *Notohippus* beds, named "Karaikense" (= Karaiken) by Kraglievich (1930). Ameghino's statement (1906) that these beds have been visited only once, in 1889, is still almost true as far as concerns serious stratigraphic study or collecting, although a brief note by Feruglio (1938a) has now been added. The Karaiken strata are said (Ameghino, 1906) to overlie the Patagonian, proper, and to underlie the Santa Cruz in the subandine zone near Lago Argentino in western Santa Cruz Territory. Ameghino believed the Superpatagonian to be lacking here, but Feruglio (1938a) says that it, or at least its basal part, is present, poorly developed. In this case the stratigraphic relationships of the Karaiken beds are not detectably different from those of the lowest Santa Cruz beds in various localities and the Karaiken might be considered simply as a local facies of the early Santa Cruz. That non-marine sedimentation should appear somewhat earlier in the subandine region than to the east is to be expected since the sea appears here to have withdrawn eastward. The *Notohippus* fauna as described by Ameghino (1904, 1906) is also scarcely distinguishable from the Santa Cruz fauna of which it apparently represents an early phase. The distinction of the Karaiken formation and of a Karaikenian stage from the Santa Cruz and the Santacrucian is thus not clear-cut but may be accepted tentatively. Doubtless some mammals described as Santacrucian or, particularly, early Santacrucian are Karaikenian, if the two are distinguishable.

The most widespread and most richly fossiliferous of all the non-marine Tertiary formations of Argentina (and hence of all of South America) is the Santa Cruz. It appears at scattered localities throughout southern Patagonia from near the north-

ern end of the Gulf of San Jorge to northern Tierra del Fuego and from the Atlantic to the Andes. The best exposures and richest fossil beds are in Santa Cruz Territory along the coast from Monte León to Puerto Gallegos and inland in the Chico, Shehuen (or Chalia), Santa Cruz, Coyle, and Gallegos valleys. The lower part of the Santa Cruz is conformable with the Suprapatagonian and even interdigitates with it in places, an observation made by Carlos Ameghino and fully confirmed by later work (*e.g.* Feruglio, 1938a, Hatcher denied this in 1897 but confirmed it in 1900). The bulk of the formation is, however, later than the whole Patagonian group. The Santa Cruz, not unlike the Casamayo at an earlier time, was widely spread over an emergent lowland. It contains enormous quantities of volcanic ash, like the earlier beds, but unlike them also contains much gravel and other detrital material derived from the rising Andes to the west.

The Colhuehuapian and Santacrucian mammals are recognizably different, but the average difference is so slight that they suggest closely successive stages of the same general fauna despite the intercalation of the marine beds between them. The time represented by Patagonian and Suprapatagonian together cannot be very long, speaking geologically. It is shorter than the time represented by any of the three major hiatuses in the Eocene series.

The Santa Cruz is a thick formation, up to 500 meters in places, but its deposition seems to have been relatively rapid. Local facies are sometimes distinguishable but no real faunal sequence has been demonstrated. The fauna evidently was remarkably uniform, although doubtless evolutionary advance will be more clearly demonstrable by the application of more refined field and laboratory methods. The only probable distinction yet made between Santa Cruz faunules, as noted by Scott (1932), is that certain beds at a high elevation in the foothills near Lake Pueyrredón yielded a fauna with several distinctive species, possibly later than the typical Santa Cruz. These beds may prove to be equivalent to the Collón-Curá, mentioned in the next paragraph.

In northwestern Patagonia, Neuquén Territory, just east of the Andes, there is a relatively small area of sediments, the

Collón Curá beds, similar in age and origin to the pre-Andine Santa Cruz of southern Patagonia. Roth (1898, 1920), who found the beds and fauna, and Ameghino (1906) agreed that they are Santacrucian in age. Later students, Groeber (1929), Kraglievich (1930), and others, believe them to be somewhat younger. The evidence for a Colloncuran stage distinct from and intermediate between the Santacrucian and Friasian is not very clear at present and the question can only be settled by revision of the fauna and new collections with better field data. The Chichinales beds (Wichmann, 1924), farther east in the same part of Patagonia, are also more or less of this age but are still more dubious.

Roth (1920, 1925, data gathered in 1896-99 and in part then communicated to the Ameghinos), Ameghino (1904, 1906), and Rovereto (1914) discussed mammals obtained from scattered localities along and east of the Andine front in central Patagonia between the regions of occurrence of the true Santa Cruz and of the Collón Curá, particularly on the rivers Frías, Fémix, Senguerr, Guenguel, Huemules, and Mayo and near the lakes Blanca and Buenos Aires. Some of these deposits were considered Santacrucian, some as "Entrerrian," a considerably later stage, and some as intermediate between these two. Ameghino recognized a Friasian ("Friaséen") stage between the Santacrucian and the oldest "Entrerrian" mammals. Kraglievich (1930) later revised these several small and fragmentary collections and sorted most of them by locality into two groups representing, in his opinion, two successive faunal stages which he called Friasian ("Friasense," after Ameghino) and Mayan ("Mayoense," after Roth, 1920). Even these scanty data do suggest strongly that there is at least one distinctive stage here, and possibly two, but knowledge of these is still very defective. Virtually no stratigraphic data have been published, or apparently preserved with the collections, and the little that is known includes the statement (Roth, 1920, 1925) that the fossils from the most important locality, Río Frías, were found at several different levels and are not all of the same age.

Northern Pre-Andine Region

It is probable that beds of Friasian and earlier ages occur in northwestern Argentina and considerable developments of early and middle Tertiary beds are reported here, but fossils are usually lacking in them or so scarce that correlation is impossible and they have no value for the present review. The important Tertiary mammalian faunas of this region all appear to be post-Friasian.

Rocks of later Tertiary age, about Upper Miocene to Middle Pliocene by my correlations, occur through a very large area in the precordilleran and piedmont zones from Jujuy, in extreme northwestern Argentina, to Mendoza in about the latitude of Buenos Aires. Many studies have been made of the rocks throughout this long region, but few of these include characteristic mammalian faunas with their occurrences adequately recorded, and without these neither the faunal history nor the stratigraphy can be properly unraveled.

Much the most interesting and important sequence, so far as yet known, is that in the Santa María valley in northern Catamarca and southern Salta and in nearby parts of central Catamarca. This region has been the subject of numerous studies for nearly sixty years, among which may be mentioned those of Doering (1882), F. Ameghino (1889, 1906, etc.), C. Ameghino (1918-1919 a and b), Rovereto (1914), Frenguelli (1937), and Riggs and Patterson (1939).

Mammals are not surely known from the earlier strata of this region, including the thick Famatino group, considered Cretaceous by Bodenbender, Tertiary by Groeber and other recent students. The beds above these were called Araucanian by Doering and appear in all the older work, including the classic monograph by Rovereto (1914), as if they were a unit, faunally and stratigraphically, although the total thickness of these strata is sometimes as great as 2000 meters. A lower member, containing a guide horizon with *Corbicula*, was distinguished by Bodenbender (1923-24), who called it lower Calchaquí. Frenguelli confined the name Calchaquí to this member, and this usage is followed by Riggs and Patterson. Only one mammal is known surely to be from this member. Riggs and Patterson, combining their determinations of mammals with stratigraphic

studies by Stahlecker (not yet published), divide the "Araucanian" above the Calchaquí into three formations, and one of these into two members, as follows

Corral Quemado
Araucano (*sensu stricto*)
Chiquimil A
Chiquimil B

Few fossils have been found in the Chiquimil, but they indicate approximate equivalence to the "standard faunas" of Chasicó and Mesopotamia, mentioned on a later page. The Araucanian fauna of previous literature is a mixture of the related but distinct Araucanian and Corralquemadan faunas of Riggs and Patterson.⁴

Most of these sediments are sandstones, with much conglomerate above the Calchaquí and recurrent thin ash beds in the Araucano and Corral Quemado. Predominantly torrential deposition from surrounding highlands is suggested. There is no clear evidence of local tectonic movements in this region during the deposition of Chiquimil, Araucano, and Corral Quemado. Frenguelli believes that there was an important tectonic phase between Calchaquí and Chiquimil, but Stahlecker's observations do not support this opinion. Observations elsewhere (e.g. Groeber, 1929) show that an accentuation of Andine orogeny probably did occur at about this time, but it does not appear to have been exactly and unquestionably dated in the mammalian faunal sequence. The terminal, and probably the major, phase of Andine orogeny occurred after Corralquemadan time and all these beds are now folded.

Another region of much interest and with relatively well identified and recorded faunas is that of the Huayquerías de San Carlos in north central Mendoza, some 700 kilometers south of the Catamarca deposits just discussed. The strata here are broadly similar to those of Catamarca and may represent part of a single mass of sediments laid down in the Pliocene along the Andine front. Here these eroded rocks now form a pre-Cordilleran group of hills, and the history is rather distantly

⁴ The much broader use of the name "Araucanian" by all other authors makes this more exact use somewhat confusing and it would, perhaps, be better to keep "Araucanian" as a name for this general sedimentary cycle and to give a new name to this redefined formation, especially as "Araucanian" is not properly a geographic name and is now defined to exclude the typical strata of its original definition.

analogous, on a smaller scale, to that of the Himalayan front and the Siwaliks in India

De Carles (1911) recognized two divisions in this group in Mendoza, a lower Huayquería and an upper Tunuyán formation. The faunas, described by Rovereto (1914), are small but characteristic. Riggs and Patterson (1939) show that the Huayquería is equivalent in age to the restricted Araucano of Catamarca and the Tunuyán to the Corral Quemado. This general region has also been studied by Groeber (1929) and by Frenguelli (1930b), and mapped by Groeber (1939)

Western and Southern Pampa

The Araucanian group of the pre-Andine zone extends far out into the pampa region, becoming thinner and finer-grained, as would be expected, and here generally horizontal or nearly so. In the central parts of the pampa and along the Plata river it is deeply buried, but it outcrops at many places along the margins of the pampas, especially in a broad bend tending southeast from around Mendoza and San Luis to the Atlantic around Bahía Blanca (see folding map in Tapia, 1935). Mammals have here been found in it, see, for instance, Rovereto (1914) and Tapia (1936), but little exact stratigraphic or faunal study has been made and the records are not important except to show that strata Araucanian in a broad sense do occur here. An important exception is the occurrence in the Arroyo Chasicó, in southern Buenos Aires Province, of a large fauna that can be exactly placed in the sequence and that represents a stage otherwise poorly known (Cabrera and Kraglievich, 1931, Kraglievich, 1934). It is somewhat older than the Mesopotamian, about equivalent to the lower Chiquimil, and younger than Friasian or, probably, Mayan.

In the coastal region of southeastern and southern Buenos Aires Province, the unquestioned Pampean is underlain by beds of somewhat similar lithologic character, including loessoid deposits, but separated from the Pampean by erosional unconformity and with distinctly different mammalian faunas. The most important of these typically "Eopampean" beds are those of Monte Hermoso and of Chapadmalal near Miramar. Equivalent beds with adequately diagnostic mammals are not well

known in a continuous section, but the faunas demonstrate beyond any doubt that the Monte Hermoso beds are older than those of Chapadmalal. This faunal evidence also shows that the Monte Hermoso is later than anything in the typical pre-Andine Araucanian group, *i.e.* post-Corralquemadan, but so little later that an intermediate stage is not definitely known or necessary for an essentially complete sequence. The two stages Montehermosan and Chapadmalalan^a are the latest generally acknowledged to be Tertiary (with at least one dissenting vote, by Frenguelli). There is a very large literature on them, see, among many others mostly cited by these authors, C Ameghino, 1918, F Ameghino, 1906, Castellanos, 1937, Frenguelli, 1921, 1928, Kraglievich, 1934

Kraglievich (1934) has questionably inserted an "Ironense?" and a "Claveroense?" between Montehermosan and Chapadmalalan, but as that author makes clear, these are insufficiently established for inclusion in a standard sequence of stages. Judging from their fauna, the Irene beds are a lateral facies nearly or exactly of Montehermosan age.

Most recently Cabrera (1939) has described two closely successive faunas from Adolfo Alsina in western Buenos Aires Province. The strata from which they come are not named or exactly correlated. The known faunas are small and peculiar, the large proportion of quite new forms making correlation difficult. Cabrera considers the younger fauna as late Pliocene and the other as very little older. What comparison is possible suggests nearest relationship with faunas that I consider early or, at latest, middle Pliocene, but the more exact placing of this important discovery remains a problem for the future.

Mesopotamia

The provinces of Entre Ríos and Corrientes, between the Paraná and Uruguay rivers, comprise the Argentine Mesopotamia. One of the earliest geological discoveries in South America was the fact that fossil shells and bones occur in this region in exposures along the barrancas of the Paraná (d'Orbigny, 1842). Combining observations in Corrientes with

^a The usual spelling of the locality name is "Chapadmalal," but "Chapalmalal" is sometimes used. Argentine authors usually drop the last syllable and call the "piso" and the fauna "Chapadmalense" or "Chapalmalense."

those in two regions of Entre Ríos, d'Orbigny made out a supposed sequence of ten Tertiary stratigraphic units, followed by the Pampean. He noted the presence of fossil bones not only in the Pampean but also in one of the subdivisions of his "tertiare patagonien", the "grès à ossements," d'Orbigny noted three marine horizons in a series predominantly continental, but later students, *e.g.* Burmeister (1876) and Ameghino in his earlier work (1880) considered it predominantly marine. Following Doering, Ameghino later (1889) recognized two marine formations, Parana below and Patagonian above—the name "Patagonian" being based on erroneous correlation with the Patagonian of Patagonia—with the non-marine, mammal-bearing Mesopotamian intercalated between them. Still later (1906), he recognized that both marine formations are later than the Patagonian and concluded that the non-marine Mesopotamian is a lateral facies of the upper marine horizon, extending the name to both. Bonarelli and Nágera (1913) showed that the supposedly successive sections of d'Orbigny really cover about the same period of time and that their distinctions are largely facial, the southern part more and the northern less marine in character; the general sequence along the Paraná in Entre Ríos and Corrientes covers the marginal deposits of a sea that came from the south and that fluctuated at least twice. Most of the mammals come from non-marine beds younger than the Paraná marine but older than the next marine phase, general agreement with Ameghino's opinion of 1889. Disregarding various later horizons not pertinent here, Frenguelli (1920) also confirmed this arrangement, showing in sequence the marine Paraná, fluviatile Mesopotamia with land mammals, and marine Entre Ríos formations. (In Ameghino, *e.g.* 1906, and various other writers, Entre Ríos is an inclusive name for the whole group, not for this particular member of it.)

Ameghino inferred that the mammals from this series represent two successive faunas, one believed to be of the age of the older marine beds, Paraná, and one from the Mesopotamia beds. He noted, however, that the available data did not permit an objective sorting of the collections by horizon and so was obliged to list one supposedly mixed fauna, the "Entrerrian fauna", for the two stages Parana and Mesopotamian. The

situation is practically unchanged today; no one knows what mammals characterized the Paranan. The bulk of the fauna is Mesopotamian and does represent a distinctive faunal and time unit corresponding with that stratigraphic unit. Under these circumstances a "Paranan stage" has no determined place in a standard series for non-marine stratigraphy.

The Mesopotamian is later than Friasian or than Chasican and probably is approximately of the same age as the Chiquimil, or upper Chiquimil, in Catamarca.

Northeastern Patagonia

Along and near the coast in northern Patagonia, around Golfo Nuevo, northern Chubut, northward into Río Negro Territory, and inland in the Río Negro valley, there are numerous exposures of Tertiary beds younger than the Patagonian marine group. These include deposits of at least one marine invasion, and probably two, nearly successive or possibly oscillations of the same marine phase. The group has been described by Ameghino (1906), Wichmann (1918), Windhausen (1921), Rovereto (1914, 1921), Frenguelli (1927), and others. In part this group is formed by blue-gray sands, already known to d'Orbigny and called by him "grès bleu du Río Negro." These are partly marine and partly of dubious but apparently non-marine origin, with a few land mammals.

Ameghino (*e.g.* 1906, Fig. 61) correlated with these beds extensive developments of sandstones in central and sub-Andine Patagonia, and much of the listed "Rionegrense" fauna comes from these and not from the type Río Negro formation. It is now known that some, at least, of these deposits are distinctly older than the true Río Negro and it is doubtful in all cases whether they are really synchronous. The scanty mammalian fauna of the undoubted Río Negro beds has not been sufficiently distinguished and its exact age is uncertain but it is approximately equivalent to the fauna of the Huayquería or of the Araucano, *sensu stricto*. The associated marine beds are approximately equivalent to the later (post-Paraná) marine strata of the Entre Ríos. The existence of a definite "Rionegran" time stage distinct from any other based on better data is not yet established.

Summary of Neogene

The accompanying tables summarize the more important mammal-bearing formations of the Neogene of Argentina and the physical history of this sub-period. The standard time stage scale used is more explicitly discussed on a later page.

SUMMARY OF NEOGENE HISTORY IN ARGENTINA

Stages	Tectonics		Strand
Recent	Near present level	↘	Oscillating invasions
Pampean (Uquian)	Orogenic phase?		
Chapadmalalan			
Montehermosan	Near present level		Near and beyond present position
Tunuyanian			
Huayquerian	Orogenic phase?	↘	Oscillating invasions (Entrerriean-Paranan)
Mesopotamian	Land low		
Chasicuan			
(Mayan)	Land sinking		Oscillating near and
Friasian	Land high		mainly beyond present position
(Colloncuan)	(Orogenic phase?)		
Santacrucian	Land rising	↘	Oscillating invasions (Patagonian)
(Patagonian)			

Stages	Non-marine Sedimentation			
	Mesetas	Sub-Andine Zone		Pampa-Chaco
		Patagonia	North	
Recent Pampean (Uquian)	Fluvio-glacial gravels and terrace deposits	Mainly glacial and fluvio- glacial	Glacial, valley fill, terraces, etc	Widespread molian, la- custrine, and fluvial deposits, loess, loam, sands, etc
Chapadmalalan Montehermosan	Limited develop- ment of sands and high terrace gravels	Poorly develop- ed and not well known or identified	Great intermon- tane and pied- mont series, mostly sands and gravels, detritus from rising Andes	-----
Tunuyanian Huayquerian	-----	-----	-----	-----
Mesopotamian Chasicuan (Mayan)	Poorly developed, now mostly eroded	Sands, tuffs, gravels along mountain front	-----	-----
Friasian (Colloncuan)	-----	-----	(Sediments probably pres- ent but not adequately dated)	-----
Santacrucian (Patagonian)	Widespread silt and detrital deposits from rising Andes	-----	-----	(Deposits buried, known in wells but not ade- quately dated)

FAUNAS

Zoological Characters and History

It is well known that South America had a unique and bizarre assemblage of mammals during the Tertiary. In the middle Tertiary, particularly, these were entirely unlike those of any other known region. No attempt can here be made to abstract or cite the innumerable papers and books in which these strange mammals are described. An excellent and readily available summary of them has been given by Scott (1937). Among more detailed studies those by Ameghino occupy the first rank, and other important works have been written by Owen, Burmeister, Lydekker, Roth, Scott, Sinclair, Gaudry, Loomis, Kraghevich, Cabrera, Castellanos, Rusconi, Riggs, Patterson, Bordas, and many others (This voluminous literature is not cited in the present review)

Taking the faunas as a whole and disregarding elements obviously introduced at relatively late times, their outstanding characteristic is the combination of marsupial carnivores with peculiar native placental ungulates. Associated with these are highly varied xenarthran edentates and hystricomorph rodents.

The earliest faunas, Riochican to Mustersan, best known in the Casamayor, have more varied marsupials than any later formations, including the opossum-like Didelphidæ, a rare omnivorous offshoot of this group, Caroloameghinidæ, the marsupial predaceous carnivores, Borhyænidæ, the insectivore-like Cænolestidæ, and a specialized rodent-like branch from that ancestry, the Polydolopidæ. Caroloameghinidæ and Polydolopidæ are unknown in later faunas. The others survive abundantly in the middle Tertiary but become less numerous and less varied in the late Tertiary, where the borhyænids disappear entirely and the cænolestids become greatly reduced. Only the opossums survive abundantly today, but even these are not the direct descendants of the known South American early Tertiary forms but a group introduced from elsewhere in South America or from North America in the later Tertiary.

The earliest known edentates are armadillos, which survive in great numbers and variety throughout the Tertiary and into the recent. From Deseadan to Chasicuan a peculiar family of horned armadillos, Peltephilidæ, is known. Ground sloths ap-

pear doubtfully and glyptodonts surely in the Mustersan and both groups thrived greatly in the middle and later Tertiary only to become extinct after the Pleistocene. The scanty representation of anteaters and probable absence of tree sloths among Argentine fossils may be caused by zonal distribution, as these animals are now and perhaps always have been characteristic of more northern parts of South America.

In the earliest faunas there are very primitive ungulates, condylarths, allied to those of the Paleocene and early Eocene of North America and Europe. These died out, as such, by Deseadan time, but they appear to be represented by a native order, Litopterna, that evolved from them in South America and survived into the Pleistocene. Most litopterns fall readily into two groups, the proterotheres, which remarkably converged toward the horses in structure and became extinct at about the time when true horses reached South America, and the macrauchenids, very remotely camel-like but developing a small proboscis and not much like anything else known.

Astrapotheres, grotesque, large, tusked animals, were common but not much varied in the early and middle Tertiary but are not known after the Friasian. An even stranger group of tusked and remotely mastodont-like animals, the pyrotheres, ran through their whole history in the early Tertiary and are unknown after the Deseadan.

The most abundant ungulates belonged to the almost exclusively South American order Notoungulata and are manifestly related to each other although they became extremely diversified in size and structure. The early forms are difficult to classify because they were then so nearly alike in basic structure, although even more varied in detail than are the later forms. By Colhuehuapian time, they had been reduced and specialized into seven more readily distinguished families, only four of which survive into the late Tertiary and three into the Pleistocene, where they all became extinct.

The later Tertiary forms are usually classed as toxodonts, large and somewhat hippopotamus-like, and typotheres, smaller and somewhat rodent-like, although the latter is perhaps an unnatural grouping. In the middle Tertiary the notouppids, with grazing, horse-like teeth, and the leontiniids, large browsers, are usually classed as aberrant toxodonts and there is another

distinctive group, entelonychians or homalodotheres, that developed claws like the chalcotheres of the northern hemisphere. Among the most primitive forms some are rather doubtfully classed as ancestral to these later groups and some are placed in a distinct primitive group of notoprogonians

Rodents are absent in the known faunas before the Deseadan, when three families allied to the living caviæ ("guinea pigs"), tree-porcupines, and chinchillas appeared. Rodents of this general porcupine-like or hystricomorph group increased in numbers and diversity throughout the Tertiary and are common today. The latter Tertiary is especially characterized by a highly varied family, Heptaxodontidæ,* now extinct, that included the largest known rodents, as large as big brown bears or small rhinoceroses.

Primates appear doubtfully in the Colhuehuapian and surely in the Santacrucian, primitive forms but manifestly allied to the capuchin monkeys and their allies now living in South America. They are not known in the later Tertiary, doubtless because of the facies of the deposits or because this region was south of the climatic range of primates then, as most of it is now.

Generally speaking, insectivores are lacking in South America, except for a few modern North American forms that have recently extended their range beyond Panama, but in the Santa Cruz there is one animal, *Necrolestes*, of unknown origin or fate, that may be an insectivore.

In the later Tertiary, mammals certainly of North American origin begin to appear in Argentina. At first these are only small carnivores, procyonids, allied to the raccoon and to the kinkajou and coati, which are known first in the Mesopotamian. In the Chapadmalalan such invaders suddenly appear in force: field mice (cricetids), bears, cats, horses, peccaries, and deer. Possibly at about this time or somewhat later (undoubtedly known in the early Pampean) dogs, weasels and their allies (mustelids), mastodonts, tapirs, and camels (i.e. llamas and

* The typical giant rodent of the Argentine Tertiary has usually been called *Megamys* d'Orbigny & Laurillard, 1842. Kraglievich has shown, however, that the type of this genus is really a litoptern and has given the name *Eumegamys*. Kraglievich, 1926, to the rodent long incorrectly called *Megamys*. There are four distinct groups of these great hystricomorphs, including many genera. Kraglievich calls these groups families Heptaxodontidæ Anthony, Eumegamysidæ Kraglievich, Neopiblemidæ Kraglievich, and Potamarchidæ Kraglievich. I consider them as subfamilies and group them under the oldest valid family name, Heptaxodontidæ.

	Riocan	Campanian	Miocene	Pliocene	Colombian	Santacrucian	Frisian	Chicoan	Mesopotamian	Huayquerian	Corralquenean	Montebeltrian	Chapadmalan	Pampean	Recent (surviving)
MARSUPIALIA															
Didelphidae															
Caroloameghinidae															
Borhyaenidae															
Cenolestidae															
Polydolopidae															
? INSECTIVORA															
Necrolestidae															
PRIMATES															
Cebidae					?										
Hapalidae															
EDENTATA															
Megalonychia			?												
Mylodontidae															
Megatheriidae															
Myrmecophagidae *															
Bradypodidae															
Dasypodidae															
Peltephilidae															
Glyptodontidae															
RODENTIA															
Cricetidae															
Erethizontidae															
Cephalomyidae															
Eocardiidae															

* Ameghino's record of anteaters in the Santacrucian has generally been doubted or overlooked, but Patterson (pers. com.) now informs me that an unquestionable myrmecophagid is included among the undescribed Santacrucian materials in the Field Museum.

	Riochican	Casamayoran	Musterian	Deersan	Colhuehuapian	Santa Cruzian	Frisian	Chascoman	Mesopotamian	Huayquerian	Cornalquemanian	Montebeltrian	Chapadmalan	Pampean	Recent (surviving)
Interatheriidae										-----					
Mesotheriidae †															
Hegetotheriidae			?												
Archæohyrcidae															
Acrolodidae															
ASTRAPOTHERIA															
Astrapotheriidae															
Trigonostylipidae	?		?												
PYROTHERIA															
Pyrotheriidae	?														
PROBOSCIDEA															
Gomphotheriidae															
PERISSODACTYLA															
Tapiridae															
Equidae															
ARTIODACTYLA															
Tayassuidae															
Cervidae															
Camelidae															

† "Typotheriidae" of previous studies. The generic name *Typotherium* Gervais ex Bravard is antedated by *Mesotherium* Serres.

their allies) were added to the fauna. All except horses and mastodonts survive today in South America although short-faced bears, peccaries, tapirs, and camels are extinct or greatly restricted in their original North American home. Concomitant with this spread of North American animals, the native South American fauna was much reduced. Carnivorous marsupials (borhyænids), ground sloths, glyptodonts, and all the native ungulates, as well as some lesser groups among the ro-

dents, became extinct. The armadillos, the primates, and most of the native rodents, however, held their own in the face of the invasion.'

The accompanying table shows the known distribution of families of mammals, recent and extinct, in Argentina (including native groups surviving elsewhere in South America). Some students, notably Kraglievich, recognize many more families. This is a matter of personal judgment and a relatively broad criterion makes the general faunal characters clearer to anyone but a specialist. I do not dispute the validity of any of Kraglievich's groups, but I give some of them less than family rank.

In the table some of the doubtful stages with small, inadequately known faunas are omitted without loss of essential information. For instance no family is known from the Uquian that does not also occur both in the Chapadmalalan and in the Pampean, and at least fifteen families that must have been present in Uquian time are not yet known from those beds, so that omission of the Uquian makes the table clearer without being misleading. Dotted lines indicate that the family, known both earlier and later, was necessarily present but is not yet known in this stage. Question marks indicate records dubious either as to age or as to zoological identification.

Each stage listed here has a distinctive fauna by which it may be recognized and in a few cases different relative positions within a stage can be determined; this will be true to greater degree when more detailed work has been done. From a broader point of view, six successive major faunal types can be seen, as shown in the next table.

No previous student has recognized quite these same major faunal types, although they appear to me the most defensible

* Patterson (personal communication) comments, and I agree, that the ground sloths and glyptodonts constitute a special case among the now extinct South American mammals. Unlike the borhyaenids and native ungulates, they did not converge toward North American types and they were able to invade North America. Their subsequent extinction thus cannot be so directly and causally related to the faunal interchange as can the disappearance of southern mammals that were in competition with analogous North American types. The biological implications and results of this great faunal interchange are complex and very important, but cannot be discussed here, nor can the effect on the North American fauna be considered in this review.

Ages and Major Faunal Types	First Appearance	Now Extinct	Dominant Forms and General Characters
Present time (late Holocene) RECENT FAUNAS	Various minor groups all probably present previously but fossils undiscovered	Ground sloths Glyptodonts Heptaxodont rodents All native ungulates (macraucheniids, toxodonts, mesotheres, hegetotheres) Mastodonts Horses	Impoverished mixed fauna Marsupials and edentates greatly reduced Rodents somewhat reduced but still abundant and mostly native, plus erioetidae Carnivores and ungulates all of late Tertiary North American type and origin
Chapadmalalan to early Post-Pampean PAN-PAMPEAN FAUNAS	Cricetid rodents Cats Horses Pecaries Deer In Pampean dogs, mustelids, mastodonts, tapirs, camels, man	Proterotheres Interatheres (Borhyaenids extinct after Chapadmalalan)	Mixture of native and North American mammals Climax of edentates, rodents Native marsupial carnivores replaced by immigrant placental carnivores Much reduced native ungulate fauna North American ungulates becoming dominant
Mesopotamian to Montehermosan ARAUCANIAN FAUNAS	Didelphine opossums Placental carnivores (procyonids) Capybaras Various subfamily and lesser lines emerging from native ancestry	Peltephilid armadillos Astrapotheres No tohippids Homalodothores	Great expansion and rise to dominance of specialised lines of edentates and rodents Only six stereotyped, old native families of ungulates
Colhuehuapian to Chasicuan PAN SANTACRUCIAN FAUNAS	Primates Megatheres Several rodent families	Isotemnids Archosphyraoids Pyrotheres Leontinids (after Colhuehuapian)	Edentates abundant, basic differentiation completed Rodents increasingly varied Native ungulates waning, becoming reduced to few specialised lines
Deseadan DESEADAN FAUNAS	Rodents—three families Advanced families of notoungulates	Polydolopid marsupials Condylarthas Most primitive notoungulates	Marsupials reduced to three long lived families Edentates increasing but still rare, primitive Climax of native ungulates in size and variety, appearance of various specialised types, many hypsodont
Riochicun to Mustersan PAN-CASAMAYORAN FAUNAS	Mammals marsupials, edentates, condylarthas, litopterns, notoungulates, astrapotheres, pyrotheres	Dinosaurs	Marsupials highly varied Edentates relatively rare Ungulates highly varied, small, primitive, poorly differentiated, brachyodont

from a review of the previous work and the original data. Most authoritative of similar recent arrangements is probably that of Kraglievich (1930 and 1934), who includes the Mustersan faunas with the Deseadan, separates a Friasian type from the Santacrucian, refers the Chapadmalalan fauna to the Araucanian (or "Araucoenterrian") type, and inserts an Uquian "cycle." Kraglievich was not personally acquainted with the Mustersan fauna, which I am now revising, and he accepted the common belief that it is nearer to the Deseadan than to the Casamayoran. It seems better interpreted as an advanced Casamayoran fauna, unlike the Deseadan. The following table shows that the Friasian and Santacrucian faunal types are, indeed, distinct, but that the distinction is not well interpreted as of major value:

SUBORDINATE FAUNAL TYPES AMONG PAN-SANTACRUCIAN FAUNAS

Time	First Appearance	Now Extinct	General Characters
Friasian-Chasicooan	Rodents caviids, heptaxodonts	Eocardiid rodents Notohippids (Astrapotheres disappear after Friasian)	No real change in basic faunal type, but visible evolutionary advance in all lines. Continued differentiation of edentates and rodents. Some further reduction in variety of ungulates (Didelphids, cænolestids and primates unknown in these faunas and are markedly different when next they appear).
Colhuehuapian-Santacrucian	Primates Megatheres (in Santacrucian) Rodents eocardiids, octodontids, echimyids	Cephalomyid rodents Isotemnids Archæohyracids Pyrotheres (Leontinids disappear after Colhuehuapian)	First faunas in which rodents and edentates are abundant and show their definitive differentiation. Native ungulates individually abundant but fewer sorts than in earlier faunas.

The most questionable point here is including the Chasicooan fauna either in a Pan-Santacrucian type or in a Pan-Friasian subtype, rather than in the Araucanian type where most students would put it. It is, of course, true that its genera are more Araucanian than Santacrucian, because it is nearer the Araucanian group than the Santa Cruz formation in age. Since, however, some boundary is to be made in a series here essentially continuous, the Chasicó seems to me somewhat more easily grouped with the preceding faunas as to broadest faunal characteristics, above the generic level. Nevertheless its inclusion in the Araucanian is equally "correct" and in that case the Mayan is the last Pan-Santacrucian fauna, if it is separable, and otherwise the Friasian.

It should be noted that Kraglievich (like all students who have previously attempted such a synthesis) took stratigraphic considerations into account and attempted to combine these with the faunal types into one system. The two do not coincide and the attempt to represent both at once is necessarily a poor compromise. It was for this reason that Kraglievich has a separate Uquian cycle, although the Uquian as far as yet known has no families and few lesser groups not also present in Chapadmalalan and Ensenadan (early Pamapean), so that this supposed

cycle is now unrecognizable on objective faunal evidence. As regards the Chapadmalalan, Kraglievich showed by a census of smaller groups, especially genera, that it is probably closer in time to the Montehermosan than to the Ensenadan. This is, however, a different question from that of defining major faunal types. In this respect the Chapadmalalan fauna seems to me more naturally classified as the first of Pampean type than as the last of Araucanian type, although it is, of course, intermediate between the two in many respects.* Among what I have called the Pan-Pampean faunas, the Chapadmalalan is a distinctive subtype as shown in the next table.

SUBORDINATE FAUNAL TYPES AMONG PAN-PAMPEAN FAUNAS

Time	First Appearance	Now Extinct	General Characters
Belgranan, Bonaerian, and early Post-Pampean	No families or larger groups, but <i>Equus</i> , <i>Tayassu</i> , and various other modern genera	Hegetotheres, mesotheres	Similar admixture of South and North American mammals but proportion and variety of latter greater. Native forms more reduced and of typically Pleistocene terminal genera. Marked change in percentages of living and extinct genera and species between Ensenadan and later Pampean
Ensenadan	Canids, mustelids, mastodonts, tapirs, camels	Borhyaenids Perhaps heptaxodontids	
Chapadmalalan	Cricetid rodents, bears, cats, horses, peccaries, deer	Proterotheres, interatheres	First important admixture of mammals of North American origin. Native mammals less reduced than in Pampean, many still of typically Pliocene genera

Origin

Space will not permit much attention here to the problem of the origin of the peculiar South American fauna, a very complex subject that I am reviewing elsewhere (Simpson, 1940) and to which I plan to return in connection with revision of the earlier faunas, now nearly completed. The most popular theories have been (a) Australian origin, in part, (b) African origin, (c) North American origin, and (d) autochthonous origin. The evidence for Australian relationships, long familiar and much belabored in the literature of paleogeography, relates entirely to the marsupials. The South American borhyaenids are supposed to be closely related to the Australian dasyurids and the South American cænolestids to the Australian peramelids or to the diprotodonts in general. There is excellent authority for these beliefs, but a review of the evidence induces me to believe them probably incorrect. This is, I think, a striking example of

* I do not mean by this that the Chapadmalalan is to be considered as of Pampean age or referred to the Pleistocene. I believe it to be Pliocene, and the whole Pampean Pleistocene, as shown on a later page.

parallelism, made more striking by the fact that it is parallelism and not convergence (as some critics of this belief state). Without insisting that this problem is solved, it yet is true that it relates only to a small part of the South American fauna. There is no evidence whatever that the fauna as a whole, or the greater part of it, came from Australia and this is almost outside the bounds of possibility.

Supposed African affinities are still occasionally claimed by geologists not familiar with the anatomical data, but are, as far as I know, abandoned by those most competent to judge. The supposed evidence was mainly belief in relationships between pyrotheres and African proboscideans, between notoungulates and African hyracoids, and between South American and African hystricomorph rodents. The first two resemblances are now generally admitted to be convergent and not evidence of relationship. The last may really indicate relationship. The origin of the rodents of South America is a great mystery and they may perhaps have come, directly or indirectly, from Africa but the evidence is not conclusive.⁹ It is, moreover, noteworthy that rodents do not appear in the earliest South American faunas and that their source and route of entry may have been and probably were different from those of the basic South American fauna.

The evidence is by no means conclusive and many problems remain, but derivation of the whole early South American fauna or the major part of it from North America is at present the most probable hypothesis. North America had in the Cretaceous (and later) a primitive marsupial complex that could be ancestral to South American marsupials, a group (Palæanodonta) in later Paleocene and Eocene that could be a survival of the edentate ancestry, and an archaic ungulate complex in Paleocene and Eocene that could be ancestral to the South American ungulates. Exact lines of descent cannot be traced, but North America has, and other continents do not have, as far as known, possible allies of all the early South American mammals. There are other lines of evidence that show that a

⁹ There are competent students (*eg* Cabrera) who deny any relationship between the African and the South American so-called hystricomorphs, although the consensus seems to be against them. Wood notes some resemblance between the North American Eocene and Oligocene *Ischyromyidae* and some South American rodents. It is, after all, possible that the South American rodents were of North American origin but the evidence now available is so tenuous that this hypothesis cannot be advanced very positively.

North American-South American land connection probably did exist at about the beginning of the Tertiary.

The South American Tertiary mammals strongly, almost conclusively, oppose the existence of any of the various Tertiary southern intercontinental bridges that have been postulated. The theory of continental drift, sometimes adduced as explaining the South American Tertiary land faunas, not only cannot explain them but also is flatly and unquestionably inconsistent with them

It is obvious that almost all the families and genera (indeed most of the higher units) of South American mammals were autochthonous in the sense that they arose there, as such, during the long time in the Tertiary when South America was nearly or quite isolated from the rest of the world. Ameghino's view that mammals originally became differentiated in South America and hence spread to the rest of the world is now opposed by a great body of conclusive evidence and is not, I believe, supported by any recent student.

South American primates are not closely related to any Old World forms but could be and probably were derived from early lemuroids such as are known in the early Tertiary of North America. Their relatively late appearance in Patagonia is not analogous to the case of the rodents and does not suggest that they were really absent in South America, or even in this general part of it, at earlier dates.

The placental carnivores, cricetid rodents, perissodactyls, and artiodactyls, appearing in the late Tertiary and Pleistocene, are surely and obviously of North American origin.¹⁰ Obviously this influx reflects the rise of a land bridge between the two American continents. The dating of this event is very important. It has been argued that the appearance of procyonids in the Mesopotamian sets the date. It could be added, although it usually is not, that the appearance of true didelphine opossums at about the same time gives similar evidence, for these have no known direct ancestry in South America¹¹ but do have possible ancestors in North America. These are, however,

¹⁰ Von Ihering argued that the procyonids and uruids appeared first in South America and so must have come from Asia and have gone from South to North America, not in the reverse direction. This opinion is still occasionally quoted with approval, but aside from all contradictory theoretical considerations and indirect evidence, the data cited are incorrect. Procyonids and uruids did not appear in South America before they appeared in North America.

small, arboreal animals such as can follow and have sometimes traveled an insular, stepping-stone route without a continuous land bridge. Their occurrence here, and the absence of all other North American animals until the much later Chapadmalalan, seems to me strong evidence that the land bridge was not yet in complete, continuous form (see Patterson, 1937). In the Chapadmalalan the appearance of a large contingent of North American animals, including terrestrial carnivores and ungulates, proves that the bridge was complete at that time.

SUMMARY OF ROCK, TIME, AND FAUNAL UNITS

Discussion of the Tertiary of Argentina (and of that of many other parts of the world) has been much confused by the attempt to represent stratigraphy, time stages, and faunas in a single system of nomenclature and taxonomy. All such systems are necessarily conventional, but they can have a natural basis and a pragmatic validity if based on one set of criteria or on essentially coincident sets of criteria. The fact is that in the Argentine (as in most places) the most natural and convenient divisions of strata, of time, and of faunas frequently do not coincide, so that representing them by a single system involves conflict and compromise and is seldom satisfactory.

It is, for instance, evident that a standard time classification based on non-marine rocks and faunas should be single, with one name, and only one, for each time stage within a regional province. Attempting to use the same classification for strata as for time, many studies of Argentine stratigraphy apply the same rule to stratigraphic nomenclature with results sometimes chaotic. Thus a time stage may be called "Rionegran" (or "Rionegrense") and the same name applied to all rocks believed to be of that age. In a given publication, then, rocks called Rionegran may occur hundreds of miles from the true or type Río Negro formation, may be of quite different facies, and are merely implicitly supposed by the author to be of the same age as the Río Negro, a belief that has often proved to be incorrect. Similarly a given "Rionegran" fauna may not be from the Río Negro formation and often proves not to be of that age. It eventually results that no accurate idea of what is actually observed and inferred is conveyed by such terms.

In the preceding stratigraphic summary, rock nomenclature and time nomenclature have been distinguished throughout and

a standard time series has been used and the basis for it largely suggested. In the accompanying table this is compared with some of the previous rock + time + fauna units and names used by several Argentine authorities.

BASIC STANDARD TIME (AND OTHER) UNITS

Ameghino, 1906	Kraglievich, 1930	Frenguelli 1930	Castallanos, 1937	This Paper	
Ensenadén	Ensenadense	Tehuelchiano	Ensenadense	Ensenadan	
Puelchén	Uquiense		Uquiense	(Uquian)	
(Not distinguished)	Chapadmalense		Chapadmalense	Chapadmalalan	
Hermosén	Hermosense	Puelchense	Montehermosense	Montehermosan	
Araucanén	Araucanense		Araucanense	Tunuyanian	{ Tunuyanian
Huonegrén	Rionegrense			Huayquerian	
Mesopotamén	Mesopotamiense	Mesopotamiense		Mesopotamian	
(Not known)	Chasicocense	Araucanense	Chasicocense	Chasicocan	
Paranén	Paranense		Paranense	(Excluded as a stage)	
(Hiatus)	Mayocense		Mayocense	(Mayan)	
Friasén	Friasense	Friasense	Friasense	Friasian	
Magallanén	Colloncurense		Colloncurense	(Colloncuran)	
Santaocrusen	Santaocrusenense		Santaocrusenense	Santaocrucian	
Notohippidén	Karaikense	Santaocrusenense	Karaikense	(Karaikenian)	
Astrapotheriulén }			Pinturense	(Excluded as a stage)	
Colpodonén	Trelewense	Colhuehuapiense	Trelewense	Colhuehuapien	
Tequén	(Excluded as a stage)	(Excluded as a stage)	{ Trelewense	{ (Excluded as a stage)	
			{ Castillense?		
Pyrotherén	Desadense	Desadense	Desadense	Desadan	
Astrapotonén	Mustarense		Mustarense	Mustersan	
	Casamayorens		Casamayorens	Casamayoran	
Notostylopén	Colhuehuapiense	Casamayorens	Colhuehuapiense	{ Casamayoran	
(Not clearly recognised)	(Not clearly recognised)		(Not clearly recognised)		Riochican

In quoting Castellanos I have omitted the "Palmirenses", a Uruguayan formation and fauna, and the "Sehuenses", based only on marine beds and not properly placed in this series

Stages accepted only tentatively in this paper are in parentheses in the last column

Most of the reasons for the present form of this system have been mentioned in dealing with the stratigraphy. Ameghino's idea of the temporal sequence and Kraglievich's emendations seem to me to be essentially correct and are accepted, in the main. I use "Huayquerian" as the time term for the earlier part of Kraglievich's "Araucanense", as later split by Riggs and Patterson, because "Araucanian", in various forms, has so long been used in a far broader sense that it seems preferable to reserve it for such use.

The name "Colhuehuapien" for the *Colpodon* stage of Ameghino has priority and seems preferable to "Trelewense" or "Trelewan".¹¹ "Colhuehuapiense" for the early "Noto-

¹¹ As previously mentioned, the Trelew fauna is slightly later than that of the type Colhuehuapi. It may eventually be justified to recognise both a Colhuehuapien

stylopéen" or Casamayoran has never been defined, stratigraphically, temporally, or faunally, and in fact no such stage can be distinguished on objective data now available. With these few exceptions, my present arrangement is that of Kraghevich. Frenguelli's and Castellanos' systems are generally similar and also doubtless essentially correct, but certain details in them are not now acceptable according to my view of the data.

All these and other students have also used larger units in a similar system, and here the difficulty of attempting to incorporate such distinct categories of data has been particularly great. The large units were intended to apply equally to rocks, time, and faunas, and were based partly on tectonics, especially orogenic phases, partly on sedimentation and geographic distribution of outcrops, and partly on major faunal units. The data from these three sources are markedly in conflict and all proposed compromises seem unsatisfactory in part or in whole.

There is no agreement as to the exact dating, or even as to the reality, of the various orogenic phases as placed against a time scale based on non-marine rocks and their known mammalian faunas. I do not think that it is yet practicable to use them to draw boundaries in such a scale except as they are reflected in changes in sedimentation. It is possible to recognize certain major units or cycles in sedimentation and also in faunal types, but the two nowhere coincide.

Ameghino (*e.g.* 1906) called his major units "formations", using the word for much larger units than is now customary in most parts of the world. Following Ameghino, Kraghevich also called them "formaciones" and specified their composite basis by the alternative name "ciclos faunísticos", and Castellanos accepted Kraghevich's terms. Frenguelli called them "grupos", as appears to me proper in modern stratigraphic (not temporal or faunal) nomenclature. The following table shows the equivalence of several systems proposed.

The faunal types have been discussed on previous pages, as well as many of the data for the recognition of stratigraphic groups. It can be seen in this table that previous arrangements

and a later Trelewian stage. I think, however, that the two are so closely related that they are more conveniently classed as belonging to two phases of a single stage, the Colihue Huapi fauna representing early and the Trelew fauna late Colihuehuapi time. In practical use of such regional stage names, it is necessary to allow them some duration and not to follow the principle that they are the shortest time units that can anywhere be recognized.

Time Stages	Aneghuo, 1906 Formations "	Kragjevich, 1930 Formaciones (series faunísticas)	Frenguelli, 1930 "Grupos "	This Paper	
				Faunal Types	Stratigraphic Groups
Pampean	Pampeenne	Pampeana	Tehuelchano	Pan-Pampean Faunas	Pampean Group
(Uquian)		Uquiana			?
Chapadmalalan	Araucanienne	Araucocentremmana	Entreterriano	Araucanian Faunas	Araucanian Group, in northern pre-Andine zone
Montehermosan					
Tunuyuanan	Entreterrienne				Arauco-enterrrian strata
Huayquerian					
Mesopotamian					Mesopotamian Group, in pampas
Chasicuan					
(Mayan)	Magallanienne	Friescana	Santacruciano	Pan-Santacrucian Faunas	Friasian Group in western Patagonia
Friasan					
(Colloncuran)	Santacrucienne	Santacruciana			Santacrucian Group, in southern Patagonia
Santacrucian					
(Karaitenian)	Patagonienne				
Colhuehuapian					
Deseadan	Guaranienne	Deseadana	Deseadiano	Deseadan Faunas	Sarmientan Group, in central Patagonia
Mustrian		Casamayorana		Pan-Casamayoran Faunas	
Casamayoran		?	?		Riochican Group, in central Patagonia
Riochican					

are compromises between stratigraphic and faunal data and that Ameghino's arrangement, granted this basis, is generally valid and, on the whole, better than those proposed to replace it. It is also evident, however, that separation of the two sorts of arrangements, eliminating the need for compromise, produces greater clarity.

Since the various stratigraphic groups occur in different regions, it is understood that they may overlap extensively and that this summary table does not exactly show their extent in time. Thus the Friasian group of sediments is probably in part of Santacrucian age. Nor is this list of stratigraphic groups exhaustive, even as regards mammal-bearing strata.

The nomenclature of smaller faunal units is properly based on those of time, rocks, and localities. Thus the Deseadan fauna is the conjunction of all believed to be of Deseadan age, the Deseado fauna is that found in the Deseado formation and the Cabeza Blanca (Deseadan) local fauna is that found in rocks of Deseadan age at the locality called Cabeza Blanca. The local faunas are the ultimate units and the only ones with purely objective association, but there are very few cases in which these basic data have been published as such.

CORRELATION AND EPOCH BOUNDARIES

A large proportion of the literature on the subjects of this paper is polemic, but none is more voluminous and more bitter than that relating to the ages of Argentine formations relative to those of North America and Europe. The difficulty arises primarily from the unique nature of South American mammals and the long isolation of that continent, giving few opportunities for valid comparisons of faunas. It is greatly complicated by the emotional bias early injected into the problem and clearly visible in much of the work on it, both Argentine and foreign.

Ameghino believed all the Argentine formations to be older than any more recent student would make them. Recent opinion is still widely divided and is approximately embraced between the extremes of Kraglievich, who, for instance, considered the Deseadan to be late Eocene, and Frenguelli, who considers it to be late Oligocene. My own opinion, although based on re-examination of the data and not on striking a balance, is intermediate between these extremes, as shown in the accompanying table.

Stages	Ameghino, 1906	Kraglievich, 1934	Frenguelli,* 1930-1937	This Paper
Post-Pampean	Recent	Recent	Recent	Recent
	Pleistocene			
Pampean	Pliocene	Pleistocene	Pleistocene	Pleistocene
(Uquian)		Pliocene		?
Chapadmalalan	Pliocene			
Montehermosan	Miocene	Pliocene	Pliocene ?	Pliocene
Tunuyanian				
Huayquerian	Oligocene	Miocene	Pliocene ?	Miocene
Mesopotamian				
Chasicuan				
(Mayan)				
Friasian	Oligocene	Miocene	Pliocene ?	Miocene
(Colloncuan)				
Santacrucian	Eocene	Oligocene	Miocene	Oligocene
(Karaikenian)				
Colhuehuapian	Danian	Eocene	Oligocene	Eocene
Descadan	Senonian	Eocene	Oligocene	Eocene
Mustrian	Cenomanian	Paleocene	Eocene	Paleocene
Casamayoran	Cenomanian	Paleocene	Eocene	Paleocene
Riochican	Cenomanian	(Not recognized)	Eocene	Paleocene

* It is impossible to represent Frenguelli's views fully in this table because they involve not only different age assignments but also different local correlations and regional sequences from those of any other author. Thus, as a single example of many, he considers (or has considered) the Puelchean as latest Pliocene and the Montehermosan as early Pleistocene, but the Puelchean is generally believed to be

Double lines in the standard time scale represent the greatest of the hiatuses in the sequence as now worked out. Together with probable internal hiatuses in the Salamancan-Riochican groups, they explain why the Paleocene and Eocene (and, to a slightly lesser degree, the Oligocene) are so poorly represented in this time scale although those epochs were, together, much longer than the Miocene and Pliocene, which are so well represented in the local time scale.

Although based on positive evidence, these correlations are necessarily very tentative and at best represent rough approximation, not exact equivalence in age

Such data as tectonic phases, thickness of sediments, degrees of consolidation and of fossilization, and the like have been considered, but for various reasons shed little or no light on the problem. The only considerations that have much value at present are (*a*) correlation of the marine formations and establishment of their ages relative to the non-marine sequence, (*b*) judgment of degrees of evolution and of the comparative values of hiatuses in the mammalian faunas between various relatively well established datum planes, and (*c*) comparisons of mammals with those of North America in the rather few cases in which they are at all comparable.

Even in the marine sequence such long-range correlation is by no means exact or undisputed but it is better founded than is non-marine intercontinental correlation. Study of the marine faunas is a very complex problem not to be reviewed here. Opinions are given categorically, but they are based on judgment of the extensive and partly contradictory literature and on some personal study. I believe the Salamanca formation to be at most latest Cretaceous and probably early Paleocene, the Patagonian formation (at least that part below the Superpatagonian) to be about transitional Oligocene-Miocene, not earlier than late Oligocene nor later than middle Miocene, and probably

considerably younger than the Montehermosan and about equivalent to the Uquian, or later Uquian. I have not found in his publications a statement of opinion as to the Mio Pliocene boundary that can be represented clearly against my time scale. On one hand he puts beds that are (in my opinion) as late as Mesopotamian in the Miocene and on the other he puts beds that are (I believe) as early as Mayan or at least Chasican in the Pliocene. It would take far too long here to discuss all differences of opinion between Dr. Frenguelli and other students. Nevertheless his views are an extreme that should appear in this table and they are therefore included with this warning that he would himself express them in quite different terms and the suggestion that the reader consult Dr. Frenguelli's many papers rather than take his opinions here at second hand.

earlier than the latter date; the Paraná to be late Miocene and the overlying Entre Ríos marine beds to be either latest Miocene or probably early Pliocene; the Interensenanadan to be early Pleistocene; and the Querandine to be Holocene.

Among the early mammals, no really good comparison is possible but there are a few Casamayoran mammals of roughly Wasatchian type (Lower Eocene of North America) and the Casamayoran fauna as a whole is still more roughly of about Lower Eocene aspect. In later beds no mammals even distantly comparable appear until the Mesopotamian, where there are procyonids, not at all exactly placeable but consistent with late Miocene or early Pliocene aspect in comparison with North American fossils. In the Chapadmalalan for the first time many mammals of North American origin appear. They have inevitably undergone some change in the long journey but most or all of them could be derived from middle Pliocene forms in North America. These were the first animals surely to use the bridge. In North America animals of South American origin appear very sparingly in the middle Pliocene, then in more variety in the late Pliocene (Blancan stage). The middle Pliocene immigrants in North America are only a very few small ground sloths, which could well have crossed (as did the procyonids in the reverse direction) before the bridge was quite complete and passable for the bulk of the fauna.¹² It was thus complete and passable by Blancan in the North American sequence and by Chapadmalalan in the South American. The evidence is strong for the approximate equivalence of Blancan and Chapadmalalan. The Blancan is now considered the typical late Pliocene in North America.¹³

¹² These earliest known northern ground sloths were ancestral to or belonged to *Megalonyx*, a genus that arose in North America from primitive South American ancestors of Miocene or early Pliocene type. The occurrence of small autochthonous types of ground sloths in the West Indies is also almost certainly due to the adventitious introduction of a primitive South American stock without the existence of a land bridge and its subsequent evolution in isolation. The West Indian rodents of South American affinities doubtless reached the islands in the same way and so, probably at an earlier date and from North America, did the insectivores.

¹³ Some students insist that the earliest beds with any immigrant forms in the two continents should be synchronized, sometimes calling the Chapadmalalan middle Pliocene on this basis. Such a criterion, however, would make the Mesopotamian, not the Chapadmalalan, middle Pliocene, a correlation that is extremely improbable. It is more reasonable and more consistent with all other data to admit that the early stragglers crossed before the bridge was complete and to base synchronization on the beginning of extensive faunal interchange, a point already emphasized by Patterson (1937).

In the early Pampean, Ensenadan, there is another wave of immigration and for the first time some of the mammals are of typically Pleistocene character, in comparison with North America. Similarly another wave of South American animals reached North America in the early Pleistocene. On the basis of the mammals I would, then, consider the whole Pampean, including the Ensenadan, as Pleistocene

This evidence is summarized in the following table.

WORLD CORRELATION OF ARGENTINE STAGES

Non-marine Stages	Principal Marine Stages Involved in Correlation	Correlation of Marine Stages	Comparable North American Mammals and Faunas	General Correlation
Post-Pampean	Querandine	Holocene	Holocene	Holocene
Pampean { Bonaerian Belgranian Ensenadan	Interonsenadan	Early Pleistocene—	Pleistocene <i>E.g.</i> Broadwater	Pleistocene
(Uquian)				Pliocene or Pleistocene
Chapadmalalan Montehermosan Tunuyanian Huayquerian	Enterrian	Early Pliocene	Blancan	Pliocene
Mesopotamian	Paranan	Late Miocene	Procyonids, late Miocene or early Pliocene?	
Chasicuan (Mayan) Friasian (Colloncuran) Santacrucian (Karaikenian)	Patagonian	Earliest Miocene	Not comparable Miocene Cetacea	Miocene
Colhuehuapian Desadnan			Not comparable	Oligocene
Mustersan Casamayoran			Wasatchian?	Eocene
Riocan	Salamanca	Danian or Montian		Paleocene

The old problem of the Mesozoic-Cenozoic boundary is effectively solved by general agreement that beds immediately below the Salamanca are Cretaceous and that others almost immedi-

ately above the Salamanca are Tertiary. There is room for doubt whether the Salamanca itself should be considered as latest Cretaceous or earliest Tertiary, but this is not important for the subject of the present paper.

The even more bitterly disputed question of the Plio-Pleistocene boundary is not yet subject to any general agreement.¹⁴ In this connection two deposits, the Uquia and the Puelche, mentioned only in passing on previous pages, require some further notice. The Uquia beds of Jujuy Province, far northwestern Argentina, were discovered about thirty years ago by de Carles. Small collections of mammals have been made in them, as summarized by Kraglievich (1930, 1934), but their exact stratigraphic occurrence and associations are unknown. Some of these mammals appear to be intermediate between those of the Chapadmalalan and those of the Ensenadan, early Pampean, and on this basis Kraglievich inserted an Uquian stage between those two. He noted, however, that various faunules of more than one age seem to be represented in the collections, so that the fauna belonging explicitly to this intermediate stage has not yet been determined on an objective basis. All reported families occur both in Chapadmalalan and Ensenadan. Of 15 genera recorded by Kraglievich (8 of them doubtfully), 5 are supposedly confined to this stage, 4 occur in both Ensenadan and Chapadmalalan (or earlier), 3 in the Ensenadan but not Chapadmalalan, and 3 in Chapadmalalan or earlier but not Ensenadan. If, as I think proper, the Plio-Pleistocene boundary be drawn between the Chapadmalalan and the Ensenadan, this Uquian fauna does not contain the forms conveniently diagnostic of the boundary and can only be listed as doubtful.

It has been known since at least 1863, when Burmeister published the fact, that in the vicinity of Buenos Aires there is a thick stratum of saturated, semiliquid sand beneath the typical Pampean deposits. This was named the Puelche sand by Doering and has been the subject of great discussion in its long

¹⁴ The same question is, of course, still disputed in North America, Europe, and Asia, so that Argentine correlation means nothing unless given in terms of the disputed stages of those continents. In common with most North American paleontologists, I consider the Blancan as Pliocene and such faunas as that of Broadwater as Pleistocene. In Europe I consider the Astian as Pliocene, the Sicilian as Pleistocene. If the Sicilian be considered Pliocene, as it is by some students, then the Ensenadan should perhaps also be considered Pliocene. I consider the Pontian and Clarendonian as early Pliocene, the Aquitanian and Arikarean as early Miocene.

bibliographic history, summarized by Castellanos (1928). The type Puelche is known only from wells, does not outcrop, and until recently had no known fauna except a few equivocal shells. For many years, and even to the present day, most geologists have applied the name "Puelche" to almost any sands lying below the identified Pampean. The "Puelche" of the literature includes deposits of many different sorts and ages scattered from Bolivia to southern Patagonia and from the Andes to the Atlantic. It is not surprising that there has been bitter dispute as to the origin of this supposed formation and the age of this supposed stage. Ameghino's lists of the "Puelche fauna", for instance, include animals from Tarija in Bolivia, from Córdoba, and from the Playa del Barco in southern Buenos Aires Province, probably of three or more different ages and none known really to be of the same age (still less from the same geologic formation) as the true Puelche.

Recently a fauna has been recovered from the unquestioned Puelche near Buenos Aires (Rusconi, 1937). This fauna appears to contain some admixture of fragments derived from older eroded beds, but it also contains a fairly rich and largely unified mammalian fauna. This fauna is very evidently later than the Montehermosan and almost as surely post-Chapadmalalan. It has, in fact, the aspect of a rather ancient Pampean fauna. It could well be considered as basal Ensenadan, or as a substage below the Ensenadan but in the Pampean, extending the Pampean to include Puelchean, Ensenadan, Belgranan, and Bonaerian substages. Rusconi correlates this true Puelche with the Uquia beds. From the inadequate published data, part, at least, of the Uquia fauna or faunules appears to be slightly older than this Puelche, but this is quite uncertain and the Uquia may include material as late as Puelche, or even later.

In any case the Ensenadan appears to me to be Pleistocene and the Chapadmalal probably Pliocene, so that among the recorded faunas those of the Uquia and Puelche probably lie nearest to the Plio-Pleistocene boundary. Very tentatively, it is convenient to follow Kraglievich in recognizing an Uquian stage between Chapadmalal and Ensenadan and (contrary to Kraglievich who thinks it middle Pliocene on grounds unacceptable to me) to consider it transitional Plio-Pleistocene. The Puelche, with any part of the Uquian that overlaps it, fits best

with the following Pleistocene Whether the Uquian really also includes a distinctive and possibly terminal Pliocene stage remains to be established by publication of the necessary data and by further work.

Ameghino devoted many years to arguing for the presence of Tertiary man in Argentina and the argument still rages unabated. The problem largely involves the Chapadmalalan and Montehermosan, and there are three principal schools: (1) those who believe, with Ameghino, that these stages are Tertiary and that man was contemporaneous with them (*e.g.* Castellanos), (2) those who agree as to the evidence of Chapadmalalan, or of Chapadmalalan and Montehermosan man, but deny that these stages belong in the Pliocene (*e.g.* Frenguelli), and (3) those who deny the validity of the evidence for man during these stages (*e.g.* Hrdlička). Disregarding the problem of man, it seems to me well established that the Chapadmalalan fauna is probably Pliocene and cannot be younger than oldest Pleistocene. The Montehermosan is surely older than the Chapadmalalan. If, then, man was really contemporaneous with either of these faunas, he occurred in Argentina about as early as is surely known to be true anywhere. Even Chapadmalalan man, for instance, would be older than *Pithecanthropus* by the lowest probable estimate. I can offer no new evidence as to the contemporaneity of man with these faunas and a review of the vast and contradictory literature is outside the scope of this paper.

NON-ARGENTINE SOUTH AMERICAN TERTIARY MAMMALS

Discoveries of Tertiary mammals in South America outside the Argentine are few and equivocal, with some geographic importance, but little value in other respects. Most come from Uruguay, faunally and geographically so near Argentina as to add little to the general history. Scattered and very sparse remains have also been found in Brazil, Venezuela, Trinidad, Colombia, Bolivia, and Peru and these will be briefly summarized.

Mammals of Pleistocene type have been found in almost all parts of South America, sometimes in considerable abundance, as in Lagoa Santa, Brazil, or Tarija, Bolivia. As far as yet demonstrated they agree generally with the Argentine Pampean faunas and certainly show no more, possibly less, regional

faunal differentiation than is shown by the living South American mammals. Some of these late fossil faunas, notably that of the Tarija valley, have been ascribed to the Pliocene. Kraglievich has, however, shown that the Tarija fauna is post-Ensenadan, probably Belgranian. If the Ensenadan is considered Pleistocene, as it is by me, then the Tarija is necessarily Pleistocene and it is still Pleistocene even if the Ensenadan be defined as latest Pliocene. Patterson (pers. com.) considers the Tarija fauna middle Pleistocene, on the basis of a magnificent, mostly undescribed collection made by Riggs. Faunas of this type in South America, containing *Equus* and other modern genera, can confidently be assigned to the Pleistocene and in any case are certainly not typically Tertiary. Their occurrences will not be discussed here.

Uruguay

The pioneer work of Kraglievich (1928, 1932, and elsewhere) in the last years of his life showed that there are in Uruguay mammal-bearing equivalents of several of the Argentine formations of middle and late Tertiary age. The collections so far made are scanty, in many cases their stratigraphic occurrence is unknown or poorly known, and the stratigraphic sequence and nomenclature are not yet well worked out.

The oldest described Uruguayan mammal is perhaps *Propachyrucos shaffinoi* Kraglievich, 1932, from the Santa Lucia beds (Santaluciense, Kraglievich) of the Department of Canelones. This single specimen is hardly adequate for age determination but it suggests middle Tertiary age, possibly as old as Eocene and not younger than Eocene.

Although its stratigraphic origin is unknown, a jaw fragment, probably from the Department of Río Negro, named *Uruguaytherium beaujeani* by Kraglievich is important. It is an astrapothere of advanced type, different from and possibly more specialized than those of Argentina, where astrapotheres are last known in the Friasian. A less distinctive scrap of bone from the Department of Soriano was named *Proterotherium berroi* by Kraglievich and is another indication of otherwise unknown probably Miocene beds in western Uruguay.

In the barrancas along the shore of the Plata and the Uruguay, from not far west of Montevideo to near Fray Bentos on

the Uruguay, there are at least three Tertiary formations of some importance for mammalian history. They nowhere surely occur in one section and cannot be followed continuously because of stretches where younger beds occupy the whole exposed section. Near San Gregorio, in the Department of San José (west of Montevideo), the base of the section is formed by mammal-bearing sands, identified by Kraglievich (1932) as "Mesopotamiense." Farther up the river, west and north, the pertinent part of the section is more complex. For instance at Punta Gorda the base is Kraglievich's "Palmirensis" (Palmira formation), a fairly well defined formation of loess-like loam with very scarce mammals. Above this is a greenish clay tentatively correlated by Kraglievich with the "Mesopotamiense" of San Gregorio, but here without fossils. Then follow thick sands with *Lingula* and the top of the section is of argillaceous sands with more numerous marine shells. This upper part, at least, may correspond with the Entre Ríos formation of the Argentine province of that name. The *Lingula* beds may also belong to that formation, as Kraglievich believes, or may be a lateral facies of the Mesopotamian, as suggested by Frenguelli. Above the mouth of the Uruguay, near and above Nueva Palmira, the whole section is formed by the Palmira formation, or the marine beds are absent even when post-Palmira deposits are preserved.

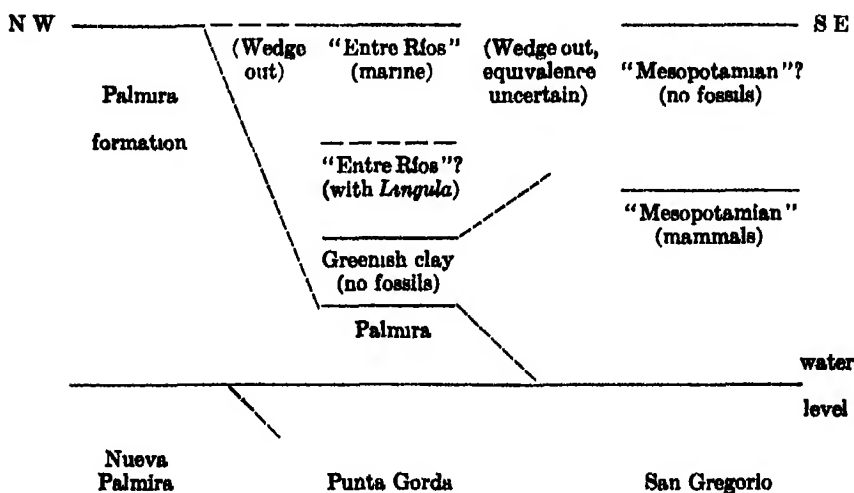
The few fragmentary mammals known from the Palmira suggest that this formation is later than Santacrucian and earlier than Mesopotamian, perhaps very approximately Friasian. Correlation with the marine Paraná beds, suggested by Frenguelli, is possible but is not warranted by the factual data, especially as the facies are so unlike and there is no direct paleontological evidence. The known Palmiran mammals include two typotheres of Miocene type, a rodent, *Palmiramys* (Kraglievich, 1932), allied to Santacrucian genera, fragments of ground sloths of rather indeterminate middle or late Tertiary type, and a few other fragments.

The so-called "Mesopotamian" beds of San Gregorio, which should, I think, be given a distinctive local name, have yielded a small fauna which is, nevertheless, the best yet found in the Uruguayan Tertiary. It includes about a dozen genera and species, not all exactly identifiable. Most of the species were

described by Kraglievich (1932) as new, but many of them are close to those known from the type Mesopotamian and most of the genera are Mesopotamian, so that this age correlation cannot be far wrong. Glyptodonts, ground sloths, toxodonts, and litopterns are present in an assemblage that would be normal in the late Miocene or early Pliocene of Argentina.

In the beds correlated with the Entre Ríos, Kraglievich (1928) reported a rodent, *Gyriabrus*, but he later (1932) suggested that it was really from the "Mesopotamian". The genus occurs in the type Mesopotamian. More surely from this marine horizon is a species of *Megalonychops*, also a Mesopotamian genus but here represented by a species possibly more advanced and not really certain as to genus.

The following diagram, based on Kraglievich's studies, shows the relations of these beds:



Isolated finds not well correlated with stratigraphic data suggest that later Tertiary deposits, perhaps Montehermosan and Chapadamalalan in age, also occur in this general area. There are also scattered deposits with typical Pampean mammals.

These various Uruguayan discoveries add very little of significance to mammalian anatomy, phylogeny, or taxonomy, but they have some geographic interest. Without exception these fossils, from perhaps as early as Oligocene through the Pleisto-

cene, have fairly close allies in the Argentine. In most cases the species are different but most of the genera are the same and all are closely allied to known Argentine genera and unquestionably belong to Argentine families. Uruguay has belonged to the same faunal realm as Argentina since the Oligocene, at least. The differences that existed were not much greater than those of today and may be ascribed to the same cause; the presence between the two countries of a water barrier, varying from a wide river to an estuary and, at times, to an arm of the sea, but a barrier that could with moderate difficulty be circumvented if not directly at least indirectly through the region north of the present Plata

Brazil

Most references to Brazilian "Pliocene" mammals relate to forms of Pampean type, more or less like those of the Ensenadan or Tarijan-Belgranan, here considered Pleistocene and certainly not typically Tertiary. Records of truly Tertiary mammals in Brazil are few and rather dubious. The first were perhaps those found by José Bach some time before 1911 at the first rapids of the Yuruá River in northwestern Brazil. The specimens themselves seem to have been lost long since, but von Ihering had a photograph which he showed to Ameghino and later sent to Schlosser. The former (*vide* Schlosser) declared them pre-Pampean. Schlosser published this history in a brief note (1925) in which he identified the mammals as including a toxodont, otherwise undetermined, and "*Megamys*", i.e. some heptaxodontid rodent. He gave the age as old Pleistocene at most. It is doubtful whether the heptaxodontids survived into the Pleistocene on the South American mainland, so that Ameghino's determination as pre-Pampean is probably correct and is as close an estimate as these very faulty data justify.

That there is a Tertiary mammal deposit in this region was to some extent confirmed by the description of a new genus, *Trigodonops*, by Kraglievich (1931), a toxodont of Pliocene aspect, based on *Toxodon lopei* Roxo, of unknown geologic origin and uncertain geographic position but probably from the Alto Yuruá.

Another discovery, also of inexactly recorded origin but surely Tertiary, is that of a rodent, a primitive ground-sloth,

and the crocodile *Dinosuchus* (= *Brachygnathosuchus*) on the Alto Purús. Patterson (pers. com.) informs me that the rodent, of which he is elsewhere publishing a description, belongs to the heptaxodont rodent genus *Phoberomys*, characteristic of the Mesopotamian, late Miocene, in Argentina.

Venezuela and Trinidad

No real mammalian fauna of Tertiary age has yet been found in Venezuela, but there are several scattered occurrences, as follows:

Gyrinodon quassus Hopwood, 1928, late Miocene or Pliocene, El Mene oil field in western Falcón.

?*Nesodon* sp., Schaub, 1935, probably Miocene, near San Pedro in central Falcón.

Prepothorium venezuelanum Collins, 1934, Miocene, on the Río Tucupido in the Andine Foothills, western Portuguesa.

Xenastrapotherium christi (Stehlin, 1928), see also Kraglievich, 1928, Miocene in the llanos near Zaraza, northeastern Guárico.

Fragmentary remains, insufficient for useful identification, have also been found at various localities, for instance in the same beds as *Xenastrapotherium* elsewhere in the Guárico llanos and in late Pliocene or early Pleistocene beds at Valera, central Trujillo, in the Andes.

From the Springvale beds, late Miocene, of Trinidad, Schaub (1935) has described a femur of a gigantic rodent, not exactly determinable but evidently allied to the giant hystricomorphs of the later Tertiary of Argentina.

Gyrinodon is a toxodont of pre-Pleistocene type, rather close to *Altotoxodon* from the Montehermosan of Argentina. *Nesodon* is the typical Santacrucian Argentine toxodont and *Prepothorium* is also an Argentine genus, a sloth reported in the Karaikenian and Santacrucian. The generic reference of the Venezuelan fossil is very uncertain. *Xenastrapotherium* is an astrapotheres particularly close to *Uruguaytherium* (perhaps only subgenerically distinct) and of possible derivation from the earlier Argentine astrapotheres.

Few as these discoveries are, they are important because

they show unquestionable affinities with fossil mammals of Argentina and none with those of North America. In view of the fact that no South American mammals had reached North America in the Miocene or earliest Pliocene, even this small budget of evidence is enough to prove that South America was then a unified continent with its northern and southern parts united by land and that the northern part, as well as the southern, was then separated from North America by a marine barrier

Colombia

The most definite record of a Colombian Tertiary mammal is very recent, that of a rodent jaw found by H. M. E. Schurmann in the Honda formation near Carmen de Apicalá in the Melgar Basin north of the Girardot-Bogotá line. Stehlin (1939) identifies the specimen as belonging to a new species of *Scleromys*, a Santacrucian genus in Argentina. He notes, however, that it also resembles the living *Proechimys* and would probably be put in a new genus if better known. The Honda formation is believed to be Miocene on other grounds, and this specimen is consistent with that correlation, although it hardly adds to the evidence for it. In any case the fossil is unquestionably Tertiary and like all the Tertiary mammals yet found in central or northern South America it is of a peculiarly South American group, affording no evidence for the connection of this region with North America at that time.

It has several times been reported (*e.g.* in Kraglievich, 1928) that astrapotheres similar to *Uruguaytherium* and to *Xenastropotherium*, the Venezuelan genus, occur in Colombia, but I am not acquainted with any detailed account. Such occurrences are to be expected, and further data would be most welcome.

Bolivia

Douglas (1914) reported the discovery at an elevation of 13,000 feet in the volcanic beds of the Mauri River, western Bolivia, of a jaw fragment identified by C. W. Andrews as *Nesodon*, a Santacrucian genus in Patagonia. The exact identification is uncertain, but mid-Tertiary age is indicated. These deposits have been included in a "Desaguadero Formation,"

but so have other beds surely Pleistocene and the "formation" evidently has not been properly defined or zoned. The abundant fossil mammals of the Tarija Valley, and others of similar aspect, can only be called Pleistocene, as noted elsewhere, and are certainly very much later than the horizon of Douglas' discovery.

Castellanos (1925) described a new genus of armadillo, *Dasypodon*, from an elevation of of 3200 meters, near the Bolivian-Argentine border south of Tupiza, Department of Potosí. The age of this deposit is otherwise unknown, but *Dasypodon* is definitely Tertiary in aspect. Castellanos believes it younger than Santacrucian and older than Huayquerian ("Araucanian").

Pachynodon validus Burmeister probably belongs in and certainly is closely related to *Haplodontherium* Ameghino, a Mesopotamian genus belonging to a peculiar group unknown after the Montehermosan. Burmeister's species is therefore almost surely Tertiary and the type probably was found in Bolivia, but the accompanying data are vague and not clearly trustworthy. As far as I know, the only available information as to provenience is that the specimen was sent to Vaca Guzmán (then Bolivian Minister in Buenos Aires) from Santa Cruz de la Sierra, Santa Cruz, Bolivia. The implication is that the specimen was found at that place, but this is not definitely established.

Peru

The only recorded Peruvian mammal of Tertiary age is *Griphodon peruvianus* Anthony, 1924, based on a lower jaw fragment from beds of early Tertiary but otherwise unknown age at Chococa, near Chepeza, on the Rio Huallaga. It was described as a possible perissodactyl, with expressed doubt and with the suggestion that it might be a pyrothere. If it is a pyrothere, it may be intermediate in evolutionary stage between *Carolozittelia* of the Casamayor and *Pyrotherium* of the Deseado. Patterson is elsewhere publishing a redescription of this fossil and informs me (pers. com.) that it almost surely is a pyrothere. In view of the great rarity of primitive pyrotheres the discovery is highly interesting, but it is annoying in its isolation and as yet is more suggestive than conclusive.

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PHOTOGRAPHIC METEOR STUDIES. III. THE TAURID SHOWER

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(Communicated by Harlow Shapley)

ABSTRACT

The present paper is a study of fourteen meteors photographed at the northern stations of the Harvard Observatory—thirteen photographed in 1937 and one in 1938. The meteors all belong to, or are associated with, the Taurid shower. Four meteors of the 1937 shower and the one meteor in 1938 were photographed simultaneously by the two cameras especially equipped for the determination of meteor heights and velocities. For one meteor, singly photographed by the camera equipped with a rotating shutter, an approximate evaluation of the velocity is made. Another meteor was doubly photographed at the two stations by cameras without rotating shutters, and the heights have been determined. The other seven meteors were photographed at only one station, and give evidence concerning the motion of the shower radiant with time.

The total interval of the shower, as covered by the present observations, is from October 26 to November 22, nearly a month. This unusual persistence of the Taurid shower, coupled with the slow daily motion of the radiant, has led Knopf (1931) and Hoffmeister (1937) to conclude that the shower orbits are hyperbolic about the sun. The present observations show that the orbits are closed and of short period.

There have previously been few data concerning the variation of meteor orbits with position in a stream. Because the Taurid shower provides such data the observations are discussed in some detail, especially with regard to the observational errors inherent in the methods of determining velocities and radiant. An approximate theory of the perturbations by Jupiter is derived and the stream identified in origin with Encke's comet. Rough estimates of the age of the stream are made by two methods. The greatest uncertainties and difficulties, observationally and theoretically, concern possible variations in the semi-major axis and eccentricity among the meteor orbits.

1. *Methods of Reduction*

IN the first paper of this series (1938) the methods used for the reduction of the doubly-photographed meteor trails were described. No essential changes in these methods have been necessary, but some comment should be made concerning details.

The position of the Oak Ridge camera (AI) has been better determined, and the positions of the two cameras have been referred to a common system, the 1927 datum of the U. S. Coast and Geodetic Survey. Bench marks of the Coast and Geodetic Survey are located near each of the two cameras (*Geodetic Bulletin*, May and June 1938). Offsets were made and the geodetic longitudes and latitudes of the cameras were found to be. Cambridge (FA) $\lambda = 71^\circ 7' 45''.45$ and $\varphi = +42^\circ 22' 53''.70$, Oak Ridge (AI)

$\lambda = 71^{\circ} 33' 29'' 82$ and $\varphi = + 42^{\circ} 30' 20'' 72$ Altitudes above sea level for Cambridge and Oak Ridge were taken to be 60 feet and 624 feet, respectively The equatorial rectangular coordinates of the FA camera with respect to the AI camera at Oak Ridge sidereal time zero are now $X = + 9.050$ km, $Y = + 35.331$ km; and $Z = - 10.295$ km. The straight-line distance between the cameras is 37 896 km. The errors in the values of these coordinates as used in the earlier work are of the order expected at that time, about one part in a thousand, and do not appreciably affect the earlier results The present values of the coordinates, although probably in error by a very few meters because of imperfect offsets from the bench marks, are sufficiently accurate for all purposes of the photographic meteor investigations.

In the reductions for the velocity, the deceleration of a meteoroid along the trail was determined by a least-squares solution, on the assumption of an exponential increase of the deceleration with time, the value of the deceleration at the mid-point of the trail was derived and the velocity was corrected for the resistance of the atmosphere by Equation 16a (1938). In correcting for the earth's attraction (zenith attraction), the effective gravity at the mid-point of the trail was used rather than that at the observing station. An additional correction was applied in determining the Taurid radiants; the zenith of the mid-point of the trail was used in calculating the zenith attraction rather than the zenith of the Oak Ridge station This procedure eliminates an error amounting to more than $2'$ in the corrected radiant for one meteor. Since the calculations of the daily motion of the Taurid radiants were made previously to the final correction for the meteors' zeniths, and since the corrections were all small, the path of the Taurid radiants, as presented, is very slightly misplaced because of zenith error The random scatter of the meteor radiants from the mean path of the radiants is not affected appreciably by the neglect.

The radiant of meteor 719, singly photographed with a rotating shutter, was determined from the divergence of the shutter breaks. The deceleration at middle break was assumed to be $0.072 \text{ (sec}^{-1}\text{)}$ times the velocity at this point, on the basis of previous observations of decelerations (1938). The velocity was determined from the height-magnitude-velocity relationship at the point of maximum brightness. The general principles upon which this solution for velocity is based have been discussed by the author (1938,

TABLE I
FUNDAMENTAL DATA AND ORBITAL ELEMENTS FOR TAURID METEORS

Meteor number	697	705	710	710	712	716	719	778
AI plate number	33564	33580	33589		33593a	33594	33608	34257
PA plate number	1422	1459	1480		1491	1494		2310
Sidereal time (Oak Ridge)	6 ^h 15 ^m 7	6 ^h 50 ^m 7	3 ^h 5 ^m	3 ^h 35 ^m	4 ^h 7 ^m 0	7 ^h 43 ^m 5	0 ^h 22 ^m	5 ^h 30 ^m 2
U.T.	1937 Oct 31 3507	1937 Nov 5.3620	1937 Nov 8.1968		1937 Nov 10 23.43	1937 Nov 10 38.42	1937 Nov 22 0.46	1938 Oct 26 3397
Length of trail (degrees)	16	14	5		7	14	15	19
Number of breaks measured	15	15	18		20		21	12
Length of time (sec)	0.90	0.70	0.85		1.10		1.05	0.65
Relative brightness of AI trail	Medium	Bright	Faint		Bright	Medium	Medium	Medium
sin Q	+0.6482	+0.7454	+0.8202		+0.3780	+0.2709		+0.4416
cos Z _z	+0.7114	+0.6605	+0.8866	+0.8957	+0.8969	+0.5710	+0.588	+0.7954
Height at first break (km)	90.85	93.88	93.05	91.44	94.02		89.45	85.74
Height at last break (km)	70.80	79.54	70.46	69.13	65.21		72.32	68.59
Height at middle (km)	80.50	86.68	81.38	79.91	79.41		81.36	77.04
Apparent radiant $\alpha(1937.0)$ $\delta(1937.0)$	52° 33' 9 +15 6 7	54° 42' 4 +15 42 3	55° 36' 3 +15 57 6		56° 50' 6 +16 24.1	58° 18' 7 +16 27 0	62° 20' +17 28	48° 12' 9 +20 56 5

TABLE I—Continued

Meteor number	697	705	710	710	710	712	716	719	778
Corrected radiant α (1937 0) δ (1937 0)	51 5 +14 0	52 56 +14 23	55 12 +15 4	54° 59' +15 1	55 58 +15 25	56 10 +14 49	64 1 +15 56	46 53 +20 3	
Elongation (λ)	76 4	79 14	79 45	80 0	80 58	80 56	84 59	73 8	
V , Apparent rel. vel. (km/sec)	32 087	30 645	30 506	29 836	29 202	29 467	27 03	33 254	
V_{∞} No-atmosphere vel. (km/sec)	32 900	30 791	30 853	30 201	29 467	29 467	27 50	33 946	
V_g Geocentric vel. (km/sec)	31 204	28 892	28 732	28 078	27 328	27 596	24 86	32 281	
V_H Heliocentric vel. (km/sec)	37 720	37 643	37 706	37 399	37 321	37 468	37 35	37 126	
a (a.u.)	2 444	2 390	2 413	2 271	2 232	2 191	2 22	2 191	
q (a.u.)	0 2906	0 3543	0 3637	0 3736	0 3939	0 2394	0 476	0 2394	
τ	0 8311	0 8518	0 8493	0 8355	0 8235	0 8007	0 785	0 8007	
P (years)	3 821	3 696	3 748	3 422	3 335	3 243	3 30	3 243	
Tisserand's Criterion	1 879	2 031	2 048	2 090	2 138	1 789	2 29	1 789	
ω (1937.0)	120° 39'	113° 23'	112° 12'	111° 37'	109° 28'	307° 42'	100° 2	307° 42'	
Ω (1937 0)	37 29	42 31	45 21	45 23	47 24	213 14	59 3	213 14	
λ (1937.0)	6 20	5 27	5 9	4 59	4 33	3 51	4 7	3 51	
τ (1937 0)	158 8	155 54	157 33	157 0	156 52	159 56	159.5	159 56	

1940). The details will be presented in a later paper, where the method will be used more extensively.

2. Velocities and Orbits

The fundamental observational data and the orbital elements for the seven Taurid meteors are given in Table I. Most of the table entries are self-explanatory. The length of the trail refers to the total photographic length, while the length of time refers to the interval from the first to the last measured break. The angle Q is measured between the poles of the meteor trail as observed at the two stations. The angle Z_R is the distance of the corrected radiant from the zenith of the AI camera. Heights are measured from sea level, and corrected for curvature of the earth. The

NOTES TO TABLE I

METEOR 697—The trail has a broad maximum with five irregularities that could be identified in both photographs. The total range in the calculated times from the five irregularities is 78', the uncertainty, therefore, is less than 1".

METEOR 705—The trail is regular with a short but weak maximum and a broad, barely measurable, secondary maximum. The FA trail, fortunately, lies nearly in the line of sight, so the time is probably accurate within 2". Over an interval of duration of 2', thirty-nine trail breaks could be well measured on the AI plate. The first nineteen gave a good solution for deceleration and were used for the velocity determination to avoid a large correction for atmospheric resistance. Results from measures of the other breaks are discussed below.

METEOR 710—Two very difficult irregularities only could be measured on the two trails. The times of apparition differed by 20". Most probably the time falls within the two 30" limits used for calculation in Table I. The solution is not sensitive to the time, so that the resultant velocities and orbital elements are reliable.

METEOR 712—This trail is marked by a short (0.04) but very bright flare at a height of 73.06 km, and a conspicuous but very short (0.02) end flare at 64.74 km. Three other minor irregularities could be well measured. The total range in calculated times is 15', three of the times lying within an interval of 2' 8". The bright flare rises 1" 5 or more above the smoothed maximal brightness.

METEOR 716—This meteor was photographed on the Oak Ridge plates IR 1769 (8 inch Ross-Lundin lens) and AC 33904 (1.5 inch Cooke-Taylor lens), but was exceedingly faint near the edge of the AI plate. The trail also was faint near the edge of the FA plate. The time of apparition could not be well determined from the trail irregularities, but it was limited to a range of 21" by the various exposures. The middle of this interval was used in the calculations for heights. The trail is fairly smooth with a broad maximum which occurs early in the trail and would produce a spurious velocity for the meteor from the height-magnitude-velocity relation. For this reason, the no-atmosphere velocity was assumed to be the same as that for Meteor 712, observed on the same night.

METEOR 719—A short (0.03) weak (0.5) flare occurs at a height of 75.3 km for this singly-photographed meteor. The plate was exposed for 77", the middle of the exposure being used as the time of apparition. Solutions at the beginning and end of the exposure gave only a ten per cent range in the calculated velocity, so that the error arising from the uncertainty in time is less than five per cent.

METEOR 778—Four flares were noticeable with heights, lengths, and brightnesses (compared to the smooth maximum) respectively as follows: 72.02 km, 0.006, 0.0; 68.48 km, 0.007, -0.8; 67.13 km, 0.01, -1.5; and 66.73 km, 0.005, -0.2. The times of apparition calculated from these flares ranged over an interval of only 20".

elongation, λ , is measured from the corrected radiant to the apex of the earth's motion. The orbital elements are indicated by their customary symbols. Tisserand's criterion is the quantity $1/a + 2\sqrt{a(1-e^2)} \cos i$, as commonly used for the identification of comets (see F. R. Moulton, 1923).

3 Errors in the Velocity Determinations

An inspection of Table I shows immediately that the orbital characteristics of the meteors form a continuous sequence with time of year, except for No. 778, the declination of the radiant for 778 is inconsistent by six degrees or more. As the reductions were carefully checked for possible error, it is necessary to assume that the early (October 26) 1938 meteor was not a member of the strong 1937 shower. Evidence will be given below to show that it belonged to an "associated" shower. Meteors No. 716 and No. 719 were incompletely observed and can be used only for qualitative confirmation of shower characteristics. The four accurately observed meteors, Nos. 697, 705, 710, and 712, provide the best data. The observed velocities, after correction for atmospheric resistance, and the heliocentric velocities for these four meteors are plotted as functions of the time of apparition in Fig. 1, both limiting solutions for Meteor 710 being shown. Although the observed velocities vary over a range of nearly three and one-half kilometers per second, the heliocentric velocities are almost constant, with an apparent decrease in value with time of apparition. A variation of the heliocentric velocities in a meteor shower is of great interest theoretically. It is important, therefore, to investigate the observational errors in order to test for the reality of the apparent variation. An assumption that the heliocentric velocities are actually invariant would lead to a probable error of the order of ± 0.13 km/sec. The probable error of a single geocentric velocity would then be about ± 0.27 km/sec, or nine parts in a thousand.

The major sources of error in a velocity determination are given in the following paragraphs.

(a) *Shutter Speed*—Over a long interval of time this error is negligible, but over short intervals a small amount of "hunting" error probably exists. Stroboscopic observations of the AI-shutter speeds made by Dr. F. G. Watson and the author at different times indicated that this error does not exceed one part in a thousand.

(b) *Angular Velocity of the Shutter*—As the shutter does not pass through the second principal point of the lens, its shadow on the plate possesses an angular velocity of the order of 20,000 degrees per second. The resultant error in a measured angular velocity increases with the angular velocity of the meteor and depends upon the direction of motion. The error seldom exceeds one part in a thousand of the measured angular

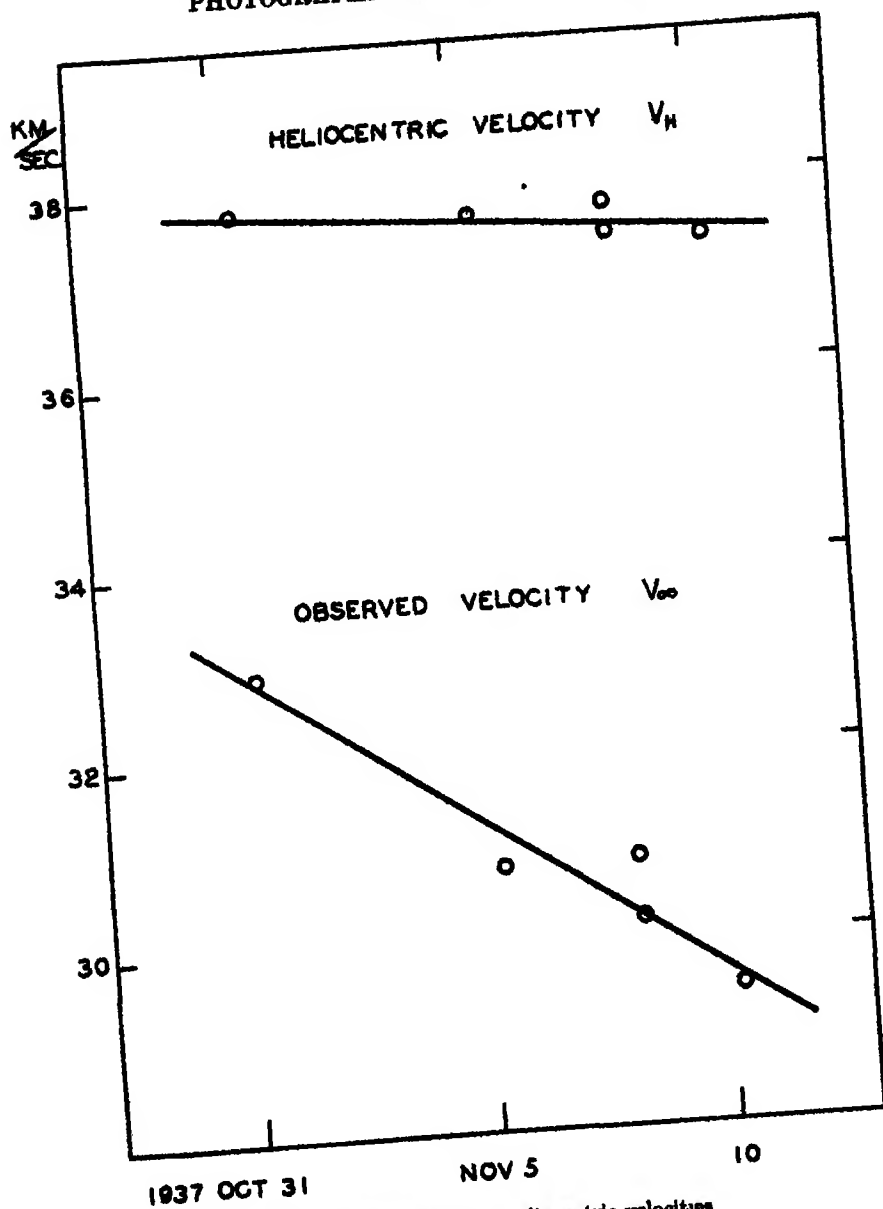


FIG 1 Geocentric and heliocentric velocities

velocity and becomes zero for some directions of motion. Although it is possible to correct for the error in the reductions, no such correction has been made.

(e) *Effect of a Persistent Luminous Train*—Such a train would shorten the apparent shutter break and the preceding distance between breaks by registering on the photograph when the shutter opens. This effect has been discussed by Millman and Hoffleit (1936). At maximum it should not exceed one-half of the interval of break, or one-

eighteenth of the distance between breaks. For an average meteor with sixteen intervals, the direct effect on the velocity should not be more than three parts in a thousand.

(d) *The Lens Astigmatism and Aberrations*—A general optical distortion of the lens is accurately allowed for in the reductions. It is determined, however, from the star images and not directly from the meteor trail. The star images often possess higher surface brightnesses than does the trail, so that a residual effect may exist. Inspection of trailed AI and FA plates shows that, near the edge of the field, the trailed images do not pass through the centers of the untrailed images for stars of moderate brightness, but are shifted towards the edge of the plate. In other words, a strongly exposed star image is displaced radially towards the center of the plate as compared with a weakly exposed image. For a rather bright image the effect amounts to about 0.01 mm at a distance of 8 cm from the plate center. The resultant error in the velocities should not exceed one part in ten thousand, because the faintest AG stars were used for measurement. The error is systematic, however, producing a slight spurious increase in the measured velocities.

(e) *Measuring Error*—From Table II, shown below, the measuring error for the four meteors averages less than three parts in a thousand of the velocity.

(f) *Error in Radiant*—The effect of an error in the radiant is to change the convergence factors used in transforming distances on the plate into distances along the trail. Errors in heights and distances are involved. From the internal evidence of the measures, the errors in radiant should not exceed about three minutes of arc. The observed scatter of the Taurid radiants appears to be larger, and must be ascribed to a cosmic spread in the motions. Detailed data are given below. An error in the radiant affects the convergence factor directly as the error along the trail, and inversely as the angular distance from the midpoint of the trail to the radiant. For a distance of 20° , an error of $3'$ in the radiant becomes about three parts in a thousand of the velocity.

(g) *Error in Time of Apparition*—This error is by far the most serious in the present method of determining meteor velocities. It can sometimes affect the order of magnitude of the velocity when the time is not determinate. In every case, fortunately, it is possible to be certain whether the error is serious or not. For the four meteors under consideration the probable error in velocity, from uncertainty in the time of apparition, is of the order of ± 0.15 km/sec, or five parts in a thousand.

(h) *Error in Correcting for Atmospheric Resistance*—All of the sources of error in the velocity except (g) will affect the value of the deceleration at the middle of the trail. In addition, some small error will arise in correcting for the resistance of the upper strata of the atmosphere. Since the calculation of the deceleration and velocity are combined, most of the sources of error in the velocity will be reflected more strongly in the deceleration, but since the correction for deceleration is generally small, a fair assumption may be that an additive error in velocity is introduced, equal to the effect of the other errors excepting (g), i.e., that the probable error from items (a) to (f) will be doubled to allow for the error (h).

The errors estimated above are mostly maximal values and must be considered as at least twice the probable errors, with the exception of (g). We thus arrive at an estimate of the probable error of a single geocentric velocity, the fractional probable errors being generally less than the following: (a) ± 0.0005 , (b) ± 0.0007 , (c) ± 0.0015 , (d) ± 0.0001 , (e) ± 0.0015 , (f) ± 0.0015 , (g) ± 0.005 , (h) error that doubles the probable error of items (a) to (f). The probable error then becomes ± 0.0063 of the geocentric velocity, slightly smaller than the value ± 0.009 obtained when the heliocentric velocity is assumed to be the same for the four Taurid meteors. It is clear that no certain deduction concerning the reality of the apparent variation of heliocentric velocity with time

can be obtained from the internal evidence concerning the accuracy of velocity measurements.

4. Reality of Variations in the Orbital Elements

Although the apparent changes with time in the heliocentric velocities may not be real, it does not follow that the apparent changes of the other orbital elements are spurious. The observed values of a , e , and q for the four accurately observed meteors are

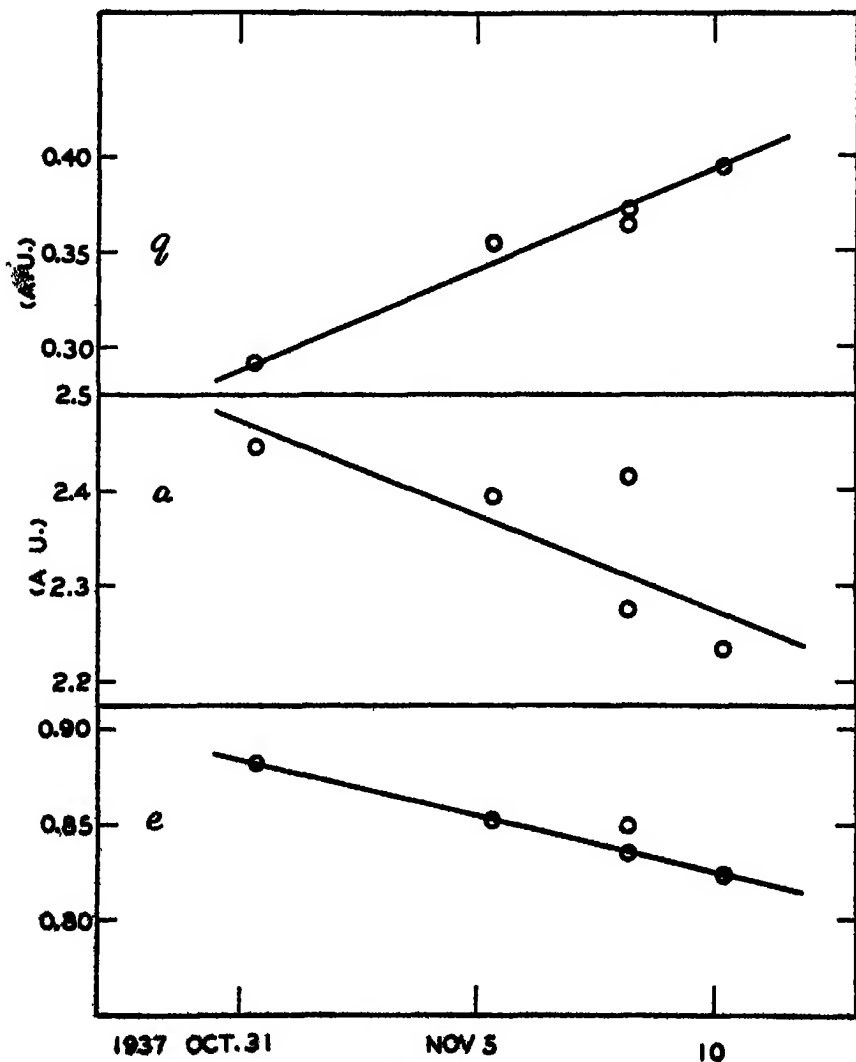


FIG. 2. Variations of a , e and q .

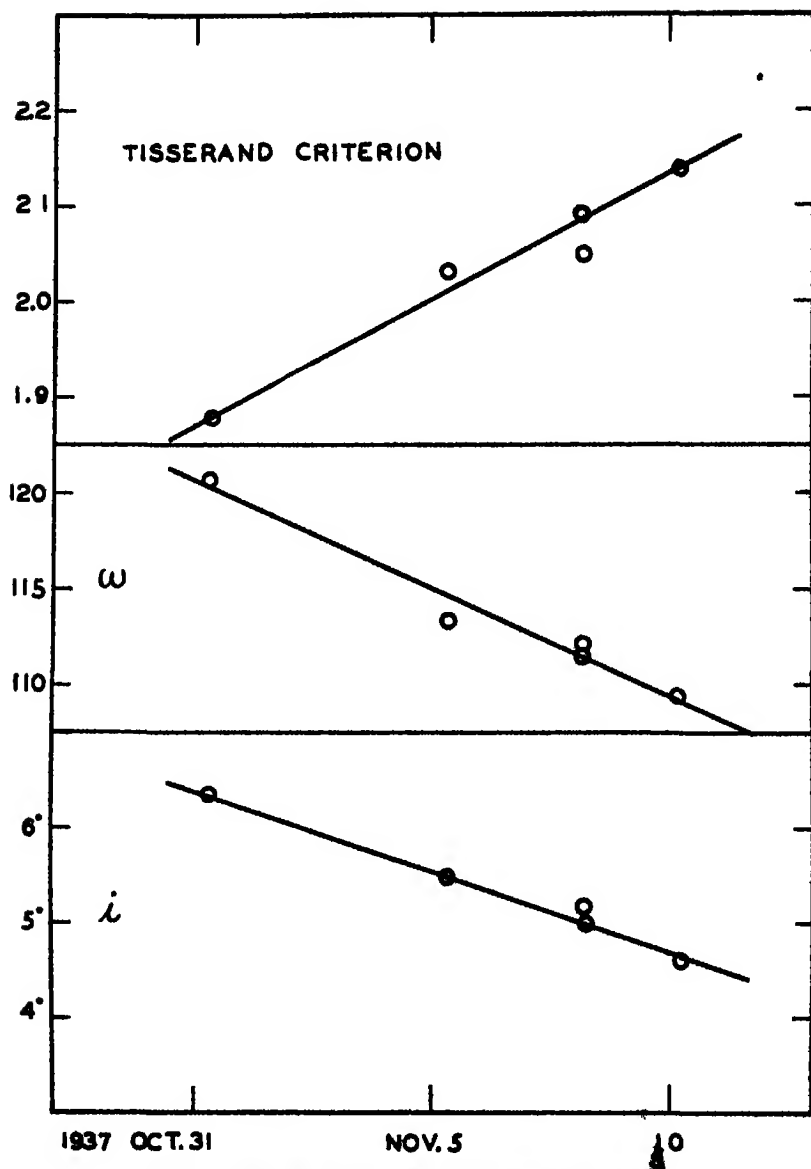


FIG 3 Variations of i , ω and the Tisserand Criterion.

shown in Fig. 2, the values of i , ω , and the Tisserand Criterion in Fig. 3. It is clear that Ω depends entirely upon the date of observation

If the heliocentric velocity is invariant for the members of the shower, then a must be invariant. The mean value of $1/a$, ob-

tained from the four meteors, is $+0.4257$ ($a = 2.349$). To test for the reality of the variations in the other orbital elements, the geocentric velocities for the four meteors were arbitrarily changed to yield this mean value of $1/a$. A new solution was then made for the orbital quantities e , q , aphelion distance, and ω , as shown in Table II. Comparison of Tables I and II shows that the ranges

TABLE II
ORBITAL ELEMENTS FOR A CONSTANT HELIOCENTRIC VELOCITY

Meteor	697	705	710	710	712
e	0.875	0.849	0.844	0.842	0.834
q	0.294	0.356	0.366	0.371	0.389
Aph. dist.	4.40	4.34	4.33	4.33	4.31
Tiss. Crit.	1.90	2.04	2.06	2.07	2.11
p	0.550	0.658	0.675	0.683	0.714
ω	$120^{\circ} 36'$	$113^{\circ} 22'$	$112^{\circ} 11'$	$111^{\circ} 38'$	$109^{\circ} 30'$

of variation for e , q , and the Tisserand Criterion are slightly reduced by the assumption of an invariant heliocentric velocity, and that the values of ω are practically unchanged. It must be concluded that variations in these quantities with position in the shower are real, as are the large variations in the orbital parameter p (Table II) and in the inclination i (Table I).

The orbits of Meteors 697 and 712 are shown in Fig. 4, in projection on the plane of the ecliptic. Part of the orbit of Jupiter and the orbits of the terrestrial planets are shown for comparison. The orbits of Meteors 705 and 710 lie between those of Meteors 697 and 712, and are not plotted in order to avoid confusion. Except possibly for the considerable difference in aphelion distance, the orbital characteristics shown in Fig. 4 are real and represent the changes with date for members of the Taurid shower. It is most interesting to note that the direction of the line of apsides remains essentially constant with date, but shows rather large fluctuations for individual meteors (Table I). This result is independent of small errors in the measured geocentric velocities, since ω and Ω are both unaffected by the arbitrary changes listed in Table II.

5. Orientation of the Taurid Orbits

The planes of the orbits of the Taurid meteors lie very close to the mean plane of the orbits of the terrestrial planets. This

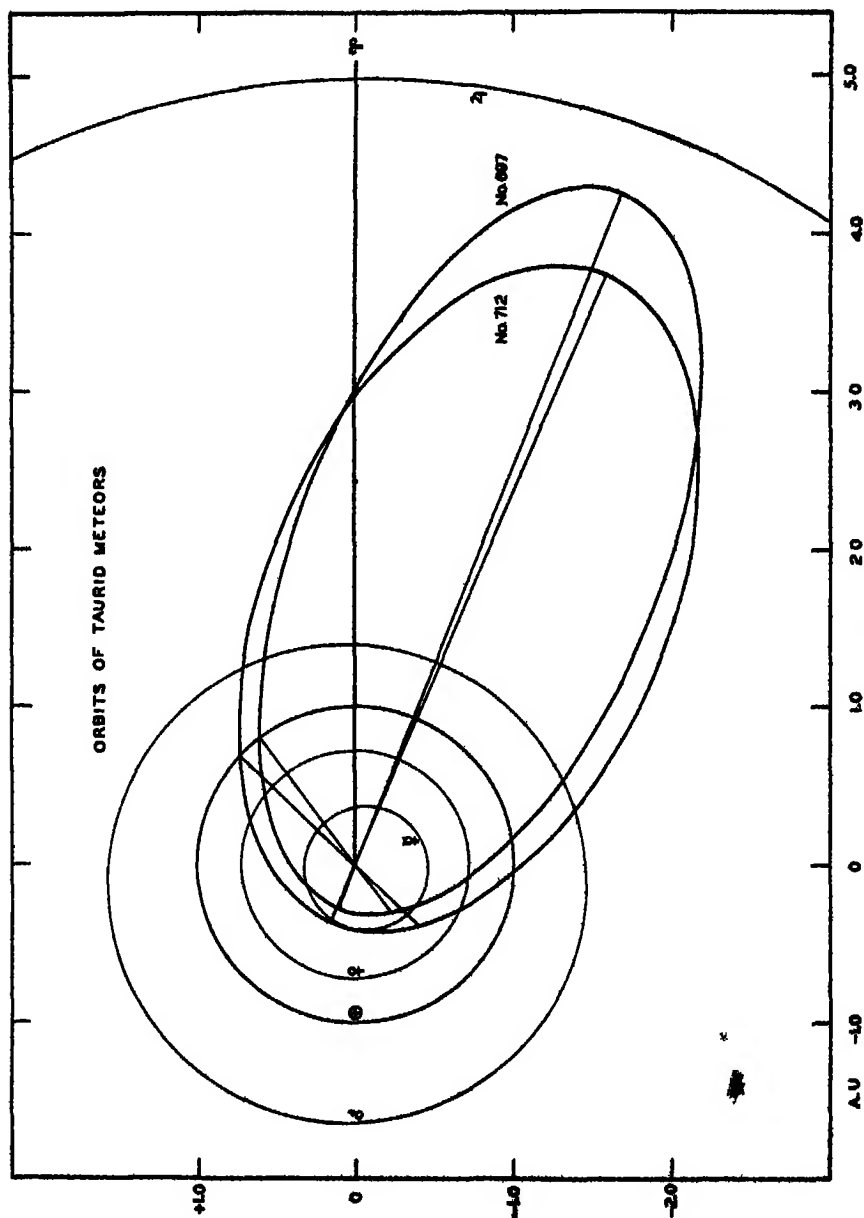


FIG. 4. Orbits of two Taurid meteors.
 No. 697 Oct. 31, 1937 $e = 0.88$ $i = 6^\circ.3$
 No. 712 Nov. 10, 1937 $e = 0.82$ $i = 4^\circ.6$

fact becomes apparent when we consider that the orbital inclinations of Mercury, Venus, and Mars are, $7^{\circ}.0$, $3^{\circ}.4$, and $1^{\circ}.9$, and the longitudes of their ascending nodes 47° , 76° and 49° , respectively. Close approaches between members of the shower and these planets can occur. Values of ΔZ , the distance perpendicular to the ecliptic between meteor and planet orbits, at the time of equal radius vector and heliocentric longitude, are given in Table III for the four terrestrial planets and three of the meteors. The second column contains the sign of the true anomaly for the meteor orbit, and the succeeding columns give the values of ΔZ in a.u. A positive sign for ΔZ indicates that the meteor orbit passes north of the planetary orbit.

Since the stream is spread over at least 0.2 a.u. along the plane of the ecliptic, and since the values of ΔZ are so small, it is likely that the shower is active for all four planets before and possibly after perihelion passage. In the case of the earth the

TABLE III
MINIMUM DISTANCES OF METEOR FROM PLANET ORBITS

Meteor No		597 $\Delta Z(\text{a.u.})$	705 $\Delta Z(\text{a.u.})$	712 $\Delta Z(\text{a.u.})$
Planet	T A			
Mercury	-	-0.002	-0.010	0.022
	+	-0.016	-0.008	
Venus	-	+0.038	+0.032	+0.026
	+	+0.052	-0.037	-0.026
Earth	-	0.000	0.000	0.000
	+	-0.099	-0.072	-0.052
Mars	-	-0.018	-0.020	-0.019
	+	-0.110	-0.082	-0.082

radiant of the post-perihelion shower would be in the general direction of the sun, producing only daytime meteors unlikely to be observed except as fireballs in late June and early July.*

The fact that the plane of the Taurid meteors is close to the mean plane of the terrestrial planets and Jupiter, and the fact of a high frequency of meteor orbits with low inclination (Whipple 1938) led the author (1939a) to suggest that meteors tend to frequent this mean plane. The observational result would be a higher frequency of meteors in the northern hemisphere during the

* Dr. Nininger has mentioned to me in conversation that daylight fireballs appear usually to come from the general direction of the sun. This observation would be expected, if the orbits are largely of low inclination.

fall months, when the earth crosses the mean plane. Since the observed orientation of the Taurid meteor orbit will be shown to be only temporary, the general conclusion must depend upon other evidence than that of the Taurid shower. This subject will be discussed in more detail in a later paper.

The close approaches and collisions of stream members with the terrestrial planets will serve, of course, to accelerate the disruption of the stream. The masses of these planets are so small, however, that the process is slow. We shall see below that the perturbations by Jupiter have a more important and somewhat different effect.

6 *Similarity Between the Orbits of the Taurids and Encke's Comet*

The projection of the orbit of Encke's comet upon the plane of the ecliptic is almost identical with that of the orbit of Meteor 712. If, from the orbits of the thirty-nine observed apparitions of Encke's comet through 1934, we choose the mean of the seven centered about 1855 in determining mean elements, we find $a = 2.217$ a.u., $e = 0.847$, $q = 0.339$ a.u., and aphelion distance = 4.09 a.u. (A. S. Yamamoto, 1936). The longitude of perihelion is about 159° (1937.0). Comparison of these orbital elements with those of Meteor 712 (Table I) are so striking as to make further study imperative. The similarity also includes the discordant Taurid, Meteor 778 of the present discussion, and Meteor 642 previously measured (1938). For purposes of comparison, and as a basis for a study of the effects of Jupiter's perturbations, the orbital elements of Meteors 642, 710 (Sid. T 3^h05^m), 778, and of Encke's comet, have been reduced to the plane of Jupiter's orbit.

TABLE IV
ELEMENTS REFERRED TO JUPITER'S ORBIT (1920.0)

Object	C Encke	M 642	M 710*	M 778
ω'	188.0	134.8	125.9	337.9
Ω'	331.0	15.0	31.5	227.5
π	158.8	149.8	157.3	159.7
t'	13.9	5.7	4.5	4.5
e'	0.0	10.6	12.3	15.6
a (a.u.)	2.217	1.910	2.349	2.191
q	0.847	0.845	0.844	0.891
Aph Dist (a.u.)	4.09	3.52	4.33	4.14

* Sidereal time 3^h05^m

These transformed elements are given in Table III for the 1920 equinox and orbit of Jupiter (Russell, Dugan, and Stewart, 1926). The quantities ω' , Ω' , and i' in the new reference plane have the customary significance of ω , Ω , and i , respectively, the origin being at heliocentric longitude zero. The quantity ϵ' is the dihedral angle between the orbit planes of the meteors and Encke's comet (mean 1855 orbit, as described above).

The most striking feature of Table IV is the large value of ϵ' for the meteor orbits. Although very similar to the orbit of Encke's comet with respect to a , e , and π , the meteor orbits lie in planes differing by 10° to 15° from the plane of Encke's comet. This discrepancy seems, at first glance, to remove all possibility of a common origin or connection between the Taurid meteors and Encke's comet. A study of the perturbing effect of Jupiter will show, however, that a common origin is probable.

7 The Observed Perturbations in the Orbit of Encke's Comet

An inspection of the elements of Encke's comet at the thirty-nine apparitions observed to 1936, shows that relatively large changes occur at every seventh apparition (in one case at the fourth apparition). This effect arises from the near commensurability of the period (3.3 yr) with that of Jupiter (11.86 yr), in the ratio of two to seven. At aphelion a near approach to Jupiter occurs every seven revolutions, except occasionally when the deviation from strict commensurability amounts to a half-period of the comet. Then a four-period separation occurs. The changes in the angular elements are systematic with time whereas the changes in a and e are essentially random in nature, except for the abnormal decrease in a , which was studied by Encke (1866). He ascribed this decrease in a to a resisting medium in space. The researches of Backlund (1911) show that the rate of decrease in a was greatly reduced at perihelion passages subsequent to Encke's work, the rate has since dropped to approximately one-tenth of its former value. For the purposes of the present discussion, we shall consider the elements a and e as statistically constant with time, affected by no secular perturbations, but possibly exhibiting small unexplained fluctuations.

The perturbations in the angular elements of Encke's comet are seen to be produced largely by the attraction of Jupiter when close approaches (about 1 a.u. minimum) occur at the aphelion of the comet. In order to obtain an approximate theory of the secular perturbations in the angular elements of the comet, we shall assume

that all of the observed perturbations arise from this cause. The error so introduced is appreciable, but should not affect the order of magnitude of the numerical results. Values of the perturbations in ω , Ω , and ι , arising from each close approach, are given in Table V. The observed elements were reduced to a common equinox, means taken over the number of apparitions indicated in parentheses in the first two columns of Table V, and the differences of the mean elements obtained to give the entries of the last three columns. The mean perturbation is the average for a close approach, and the standard deviation refers to single, observed

TABLE V
OBSERVED PERTURBATIONS FOR ENCKE'S COMET

Diff in mean date	$\Delta\omega$	$\Delta\Omega$	$\Delta\iota$
	°	°	°
1812(2) to 1832(7)	+0 38	-0 19	-0 24
1832(7) to 1855(7)	+0 65	-0 43	-0 25
1855(7) to 1873(4)	+0 20	-0 10	+0 01
1873(4) to 1891(7)	+0 30	-0 16	-0 22
1891(7) to 1914(7)	+0 71	-0 49	-0 35
1914(7) to 1931(3)	+0 28	-0 00	-0 00
Mean perturbation	+0 42	-0 25	-0 18
Stand deviation	$\pm 0 21$	$\pm 0 16$	$\pm 0 14$

values. The ordinary ecliptic elements were used in these calculations, but, because of the small inclination of Jupiter's orbit, the perturbations apply quite accurately to the comet elements when referred to the plane of Jupiter's orbit.

The perturbations in ω , Ω , and π are so closely correlated that the standard deviation in $\Delta\pi$ is numerically equal to the difference of the absolute values of the standard deviations in $\Delta\omega$ and $\Delta\Omega$, ($\pm 0^{\circ}.05$). The mean perturbation in π is, of course, $\Delta\pi = +0^{\circ}.42 - 0^{\circ}.25 = +0^{\circ}.17$. The perturbations in ι are very closely correlated with those of ω , Ω , and π .

8. An Approximate Theory of the Secular Perturbations for Encke's Comet

It is possible to obtain an approximate theory of these perturbations by the variation-of-elements method on the basis of the assumption made above, namely, that the perturbations are all produced at the aphelion of the comet's orbit by close approaches to Jupiter. Expressions are first obtained for the three mutually perpendicular forces of attraction on the comet, S along the radius

vector away from the sun, T normal to the radius vector in the orbit plane toward the direction of motion, and W normal to the orbit plane. If the coordinates of Jupiter are taken as ξ , η , and ζ with origin at the sun and oriented with respect to the comet's orbit in the same sense as S , T , and W , respectively, if the radii vector of the comet and planet are r and r_1 respectively, and if the distance between the comet and Jupiter is ρ , then the perturbing forces on a unit mass become

$$S = k^2 m \left(\frac{\xi - r}{\rho^3} - \frac{\xi}{r_1^3} \right), \quad (1a)$$

$$T = k^2 m \left(\frac{\eta}{\rho^3} - \frac{\eta}{r_1^3} \right), \quad (1b)$$

and

$$W = k^2 m \left(\frac{\zeta}{\rho^3} - \frac{\zeta}{r_1^3} \right), \quad (1c)$$

where k^2 is the Gaussian constant and m is the mass of Jupiter. The mean value of the tangential force T can, within the limits of the accuracy desired, be taken as zero. Because the constants are to be determined from the observed values of the perturbations, and because the attractions of Jupiter on the sun are small compared to the perturbing forces, the solar attractions ξ/r_1^3 and ζ/r_1^3 can be absorbed in the constant k_0 to be substituted for $k^2 m$ times other constants. Under these conditions, a mean value of $1/\rho^3$ can be found so that the value of ζ exactly at aphelion of the comet and opposition of Jupiter may be used. The longitude of Jupiter then becomes $\pi + 180^\circ$ and

$$\zeta = r_1 \sin \omega' \sin i'. \quad (2)$$

The approximate values of the perturbing forces then become

$$S = k_0 \left(\frac{\xi - r}{\rho^3} \right), \quad T = 0, \quad \text{and} \quad W = k_0 \frac{r_1}{\rho^3} \sin \omega' \sin i'. \quad (3)$$

The general expressions for the perturbations in π , Ω' , and i' are (F. R. Moulton, 1923)

$$na^{\frac{1}{2}} \sqrt{p} \frac{\Delta \pi}{\Delta t} = -\frac{p}{e} \cos \nu S + \frac{p+r}{e} \sin \nu T + r \sin u \tan i' / 2W, \quad (4a)$$

$$na^{\frac{1}{2}} \sqrt{p} \frac{\Delta \Omega'}{\Delta t} = r \frac{\sin u}{\sin i'} W, \quad (4b)$$

and

$$na^{\frac{1}{2}} \sqrt{p} \frac{\Delta i'}{\Delta t} = r \cos u W, \quad (4c)$$

where v is the true anomaly of the comet, u is its longitude from the ascending node, n is the mean motion, and Δt is the effective time that the forces act

Under the prevailing assumption that the perturbations occur at aphelion of the comet, $v = 180^\circ$ and $u = \omega' + 180^\circ$. Upon substituting in Equations (4) these values of v and u and the expressions for the forces from Equations (3), we find

$$\frac{\Delta\pi}{\Delta t} = -k_0 \frac{p}{e} \left(\frac{\xi - r}{\rho^3} \right) - k_0 \frac{rr_1}{\rho^3} \sin^2 \omega' \sin v' \tan v'/2, \quad (5a)$$

$$\frac{\Delta\Omega'}{\Delta t} = -k_0 \frac{rr_1}{\rho^3} \sin^2 \omega', \quad (5b)$$

and

$$\frac{\Delta i'}{\Delta t} = -k_0 \frac{rr_1}{\rho^3} \sin \omega' \cos \omega' \sin v', \quad (5c)$$

where k_0 absorbs the orbital constants

Because of the moderate values of v' , the second term of $\Delta\pi/\Delta t$ is small numerically in comparison with the first term and may be neglected. In obtaining expressions for the average values of the perturbations per close approach, we see that Equations (5a) and (5c) may be used in their present form, but that the $\sin^2 \omega'$ term in (5b) will be inaccurate when $\sin \omega'$ is near zero, because the perturbations are produced over a range in longitude both for the comet and Jupiter. If the average of $\sin^2 \omega'$ is taken over a range, l , in longitude on each side of aphelion, the quantity $\sin^2 \omega'$ may be replaced by $1/2(1 - k_3 \cos 2\omega')$, where $k_3 = (\sin 2l)/2l$, and may be determined from the observed mean ratio of $\Delta\Omega'/\Delta i'$ per close approach.

If we now adopt the average interval between close approaches as the unit of time, combine as constants (k_1 etc.) all quantities that are not angular orbital elements, and use the derivative form for the perturbations over long time intervals, we find the following as the fundamental differential equations for the perturbations:

$$\frac{d\pi}{dt} = k_1, \quad (6a)$$

$$\frac{d\Omega'}{dt} = -k_2(1 - k_3 \cos 2\omega'), \quad (6b)$$

$$\frac{d\omega'}{dt} = k_4 - k_3 \cos 2\omega', \quad (6c)$$

and

$$\frac{di'}{dt} = -k_3 \sin \omega' \cos \omega' \sin i', \quad (6d)$$

where

$$k_4 = k_1 + k_2 \quad \text{and} \quad k_5 = k_2 k_3.$$

Equations (6) integrate directly into the following expressions:

$$\pi = \pi_0 + k_1 t, \quad (7a)$$

$$\cos 2\omega' = \frac{k_5 - k_4 \sin k_7(t - t_0)}{k_4 - k_5 \sin k_7(t - t_0)}, \quad (7b)$$

$$\Omega' = \pi - \omega' \quad (7c)$$

and

$$\log \tan i'/2 = k_8 - k_9 \log (k_4 - k_5 \cos 2\omega'), \quad (7d)$$

where π_0 , ω_0' , and i_0' are values at $t = 0$ (1855). The constants are given by the expressions

$$k_7 = 2\sqrt{k_4^2 - k_5^2},$$

$$\sin k_7 t_0 = \frac{-k_5 + k_4 \cos 2\omega_0'}{k_4 - k_5 \cos 2\omega_0'},$$

$$k_8 = \log \tan i_0'/2 + k_9 \log (k_4 - k_5 \cos 2\omega_0'),$$

and

$$k_9 = k_6/4k_5$$

The period of revolution of ω' is $4\pi/k_7$.

If we adopt the mean values of the observed perturbations, as given in Table V, and the initial values of the elements of Encke's comet with respect to Jupiter's orbital plane as given in Table IV, we obtain the values of the constants as given in Table

TABLE VI
VALUES OF THE CONSTANTS

k_1	0 00290	k_7	0 0456
k_2	0 0468	$k_7 t_0$	30° 6
k_3	0 942	t_0	11 7
k_4	0 0492	k_8	-2 047
k_5	0 0436	k_9	0 530
k_6	0 0924		

VI. The unit of time is the average interval between close approaches with Jupiter, roughly 21 years, and the zero of time is 1855. The equinox is that of 1920.0. The calculated value of k_8 corresponds to an average of the attraction in $\Delta\Omega'$ over $\pm 18^\circ$ in longitude, a reasonable value. The period of one revolution of

ω' is 276 intervals or about 5,800 years. A comparison of Equations (7b) and (7d) shows that the period of variation of i' is half that of ω' , or about 2,900 years. The range is from $i' = 16^\circ.0$ maximum to $i' = 3^\circ.6$ minimum.

The perturbed elements for Encke's comet, as calculated from Equations (7) with the constants of Table VI, are shown graphically in Fig 5 as functions of the number of close approaches with Jupiter. Calendar dates are given to fix the order of magnitude, but it must be remembered that the length of the period between

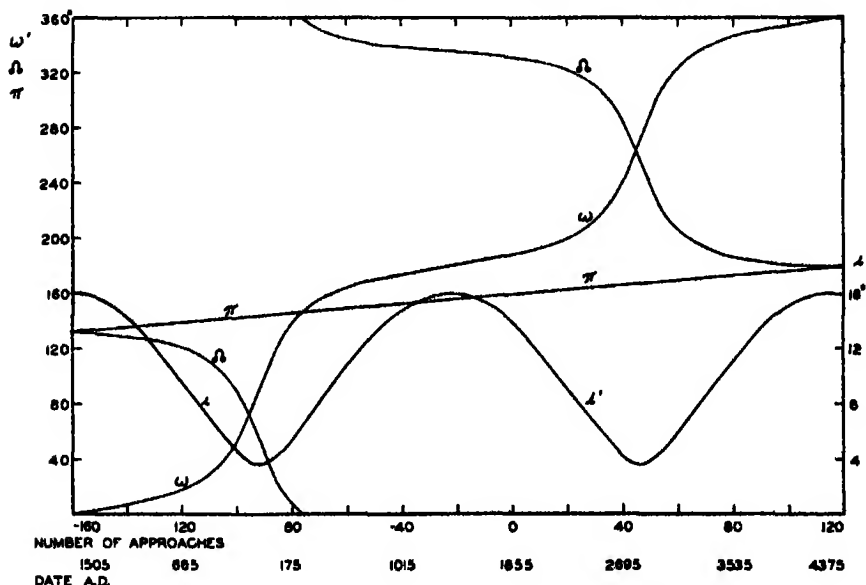


FIG 5 Perturbed orbital elements for Encke's comet

close approaches may change greatly with small changes in the semi-major axis of the orbit. Thus, unpredictable changes in a , such as have occurred in the past, may greatly change the predicted dates without necessarily changing relative values in Fig 5 by large amounts

9. Identification of the Taurid Meteors with Encke's Comet

The close orbital similarity in shape, size, and direction of the apses between the Taurids and Encke's comet has been pointed out above. The discrepancy in orbital plane can now be largely explained by a consideration of the calculated perturbations affecting Encke's comet. If we suppose that, in the past, a large

comet disintegrated into smaller ones, either at one time or successively, and that the cause of the disintegration was not violent (such as a near collision with Jupiter that might have changed the orbital elements vitally), then the various resultant components of the original comet would separate with only slightly different orbital elements. In the course of time, as a result of different perturbation forces on the separated comets, and possibly as a result of unpredictable perturbations, the semi-major axes of the various comets would differ appreciably. If the original orbit resembled that of Encke's comet with respect to aphelion distance, the rates of variation of the angular elements would depend largely upon the exact distance of aphelion from Jupiter's orbit, and upon the order of commensurability between the periods of revolution. The general relations between the values of the angular elements, however, would not be greatly affected, as has been shown above. If, then, the Taurid meteors are the disintegration products of one or more of the components of the original comet that also produced Encke's comet, the relation between ω' and i' for Encke's comet should also apply to the Taurid orbits. If, for Meteors 642, 710, and 778, we substitute the values of ω' from Table IV in Equation (7d), using the constants of Table VI for Encke's comet, we obtain the calculated values $5^{\circ} 1$, $3^{\circ} 9$ and $4^{\circ} 5$, respectively, for the inclination i' . The corresponding observed values are $5^{\circ} 7$, $4^{\circ} 5$, and $4^{\circ} 5$, an agreement that is better than might have been expected.

The values of π and Ω' cannot be calculated so directly. We must know whether the node of each of the meteor orbits has regressed more rapidly or less rapidly than the node of Encke's comet, and whether the known difference of a fraction of a revolution has been augmented by any complete revolutions. It is also necessary to take into account the different dependence upon aphelion distance of the radial force S compared to the normal force W , and to investigate the correlated dependences of the perturbations.

To proceed with a study of the perturbing forces, we note from Equation (5) that $\Delta\pi$ per close approach depends upon the mean value of $(\xi - r/\rho^2)\Delta t$, and that $\Delta\Omega'$ and $\Delta\omega'$ depend upon the mean value of $(1/\rho^3)\Delta t$. If the aphelion distance of a meteor stream changes slightly from that of Encke's comet, these mean values will change in a different manner. Very roughly, a small change of

+ $\epsilon\%$ in ρ at aphelion of the stream with Jupiter at opposition might be expected to produce a change of $-3\epsilon\%$ in $(1/\rho^3)$, $-2\epsilon\%$ in $(\xi - r/\rho^3)$, and $+\epsilon\%$ in Δt . Thus, the rate of change of $\Delta\pi$ would be numerically $-\epsilon\%$, and that of $\Delta\Omega'$ and $\Delta\omega'$, $-2\epsilon\%$. Numerical integrations, approximating the conditions of the problem, confirm the order of magnitude of these estimates. Somewhat better values are $-\epsilon\%$ for $\Delta\pi$ per average close approach, and $-2.5\epsilon\%$ for $\Delta\Omega'$ and $\Delta\omega'$. Such changes will, of course, affect the numerical values of the constants k_1 , k_2 , and k_3 directly (see Equations 6). The relation between ι' and ω' will, however, remain constant so long as the aphelion distance remains constant, the numerical relation depending only upon the initial conditions and the constants, because the time does not explicitly enter Equation (7d). The values of ι' , calculated above for the meteor streams, may be slightly but not grossly in error as a result of the different perturbations incurred in the past, and as a result of the present differences in aphelion distance from that of Encke's comet.

10 "Age" of the Taurid Stream

Because the perturbations depend upon the aphelion distance of the orbit, it is possible to estimate the times elapsed since the separations of the various meteor streams (or their original comets) from Encke's comet by comparison of the present values of ω' or Ω' for the various orbits. For this calculation, accurate values of the aphelion distances at present are necessary as is also some assumption concerning the past values. If we accepted the widely different values of aphelion distance indicated for the four principal Taurids in Table I, we would arrive at four widely different time intervals since the separation. A more probable solution is from the mean of the four aphelion distances given in Table I (4.34 a.u.), for a constant heliocentric velocity. The aphelion distance of Encke's comet is 4.09 a.u. Thus, the perturbations should be greater for the Taurid stream than for Encke's comet; the gain in ω' is 298° (from Table IV), and that in Ω' , 300° . Since the gain in π is observed to be negligible, it is likely that no complete revolutions in ω' or Ω' have occurred. If we further assume that the values of the aphelion distance have remained relatively unchanged since the time of separation, the relative value of ρ at a close approach becomes $(5.0 - 4.09)/(5.0 - 4.34)$ or 1.38, $\epsilon = 38$. Accordingly, the motion in ω' for the meteor

stream should be nearly twice that for Encke's comet ($1 + 2.5 \times 0.38$), so that the gain of 298° would have been acquired in about two hundred and forty close approaches or 5,000 years. This estimate is a minimum value, since the present difference in aphelion distance was probably acquired slowly

The relative motion in π would be increased by $1/2.5$ as much as that in ω' , or $0.38 \times 240 \times k_1 = +15^\circ$. The observed difference in π is negligible. This discrepancy cannot be removed by a change in the relative perturbations of the Taurid stream compared with those of Encke's comet because the perturbations in π and ω' are directly related by the theory. Only a change in the direction of relative motion could bring about an agreement. If the aphelion distance for the Taurids is actually less than for Encke's comet, it would follow that ω' for the comet has gained only 62° and π would be calculated to have gained only 3° , a satisfactory agreement except for the large change in a required. The systematic error in the observed geocentric velocities must exceed 1.6 per cent, or roughly five times the probable error of the mean, for the aphelion of the Taurids to fall within that of Encke's comet. A systematic error of that magnitude might of course be possible.

The above analysis when applied to Meteor 778 (Table IV) would lead to an age of about 20,000 years, calculated by the gain of 150° in ω' . The discrepancy in π is about $+8^\circ$. For Meteor 642 (Table IV) the loss in ω' would be 307° , corresponding to an age of over 5,000 years and a discrepancy of about -8° in π . In all cases the observed values of π are nearer the value for Encke's comet than the theory would predict.

It is unfortunate that the accuracy of the velocity determinations is not great enough to establish the age of the Taurid showers by this method, but it is instructive to note that the suggested order of magnitude of the age is not excessive, only a few thousand years. The discrepancies in the predicted values of π are not large enough to challenge seriously the identification of the Taurid meteors with Encke's comet.

Another method can be used for estimating the period of time required for the disintegration of an original comet to the present main Taurid meteor stream. If the individual bodies separated from the comet with slightly different mean motions, they would soon be widely distributed along the original orbit, but would

deviate from the orbit only slightly. The perturbing force of Jupiter (and other planets) would act on each body individually and differently, however, the deviation from the average perturbations producing finite differences in the orbital elements. Unless a is perturbed considerably, these random perturbations would tend towards the same average value of the total perturbation because of the repetition of the natural periodicities arising from the near commensurabilities of the mean motion with that of Jupiter. It will not be proper to use the standard deviation of the perturbations per close approach (Table V) to calculate directly the time required for the elements to acquire the observed standard deviations. Such a method completely neglects the periodic character of the perturbations

A better method is to estimate the random effects on the elements by the use of the observed perturbations for Encke's comet, in order to determine the deviations from perfect correlations between the perturbations of the various elements. This procedure tends to reduce the effects of periodicities in the Jupiter perturbations, and includes perturbations other than those of Jupiter. In choosing the elements to be used for this calculation, we note that Ω (or ω) is the running coordinate of observation affected by the largest perturbations, and that i is directly correlated with it. The element π is less perturbed theoretically, shows considerable variation from meteor to meteor, and is less influenced by a selection effect in observation. From the perturbations of Table V we find that $\Delta\pi = +0.37\Delta\omega$, the probable error in $\Delta\pi$ by this equation being $\pm 0^{\circ}.024$. If we assume that this probable error is constant for all $\Delta\omega$ (or $\Delta\Omega$, there being little difference for this discussion) in 650 close approaches, the probable error in π would increase to $\sqrt{650}(\pm 0^{\circ}.024)$ or $\pm 0^{\circ}.62$, the observed value from the four principal Taurid meteors of Table I. The corresponding age of the stream is approximately 14,000 years.

The above age is probably a lower limit under the assumption that a has not changed during the interval. Periodicities in the ratio $\Delta\pi/\Delta\omega$ will almost certainly tend to reduce the effective probable error in $\Delta\pi$, because the two quantities $\Delta\pi$ and $\Delta\omega$ depend mainly upon the S and W forces respectively, which will be affected differently at different close approaches with Jupiter. If, however, a undergoes unpredictable perturbations for the various small bodies, the ratio S/W will also change unpredictably, as has been

shown above. Thus, the effective probable error of $\Delta\pi$ when calculated from $\Delta\omega$ will increase, and the age will be reduced. The effects of collisions, as calculated by Öpik (1936), can be neglected. The age of 14,000 years appears to be the best estimate that can be made from the present material and analysis. It may be of some interest that this period is of the same order of magnitude as the time required, by Encke's and Backlund's calculations (in the case of a resisting medium) for the aphelion distance of Encke's comet to have decreased from the heliocentric distance of Jupiter's orbit

11. *Correlations Among the Elements of the Taurids*

In Figs 1-3 definite correlations between the orbital elements of the four Taurid meteors are shown, and the reality of certain of them have been established. With increasing values of Ω (or date of apparition), q and Tisserand's criterion increase, while ω , i , a , e , and the aphelion distance decrease.

Of the six orbital elements, only five can be considered here since the date of perihelion passage alone, without a knowledge of the differential number of complete revolutions for single meteors, is of no interest. Of the five elements, a is too uncertain for its correlations to be of value. The effect of selection is to impose the condition that the radius vector of the orbit be approximately unity at the node, or that

$$\cos(-\omega) = (p - 1)/e. \quad (8)$$

Between four elements, there can be three independent correlations, one of which is specified by Equation (8). Two independent correlations, only, are under consideration.

The theory of the perturbations for Encke's comet shows that π should change slowly with respect to Ω . The small observed correlation between π and Ω is seen from Table I, and has been used to estimate the age of the stream. The strong correlation of ω with Ω is therefore explained ($\pi = \omega + \Omega$), leaving but one certain correlation for discussion.

The inclinations of the orbits are strongly correlated with Ω . Such a correlation would be expected from the fact that both elements are perturbed by the W force alone. Over relatively short intervals of time the peculiar effects of close approaches with Jupiter should average out, to maintain a fairly precise relation between Ω and i . We have seen above that the value of i' for

Meteor 710 is in very close agreement with the value predicted on the basis of an identification in origin with Encke's comet. Using ω' as the argument for calculating i' for all four meteors, we find the predicted range of i to be $0^\circ.7$ compared to $1^\circ.7$, the observed value (Table I), in the correct sense, the mean difference in i ($O - C$) is $+0^\circ.2$. Since there is no theory for perturbations in a or e , and since the criterion of selection involves these elements, we cannot hope to better this agreement by means of the present analysis.

12 Daily Motion of the Taurid Radiant

The apparent radiants of the first five meteors of Table I were used in the determination of the apparent path of the Taurid radiant. A great circle was fitted to the five apparent radiants by least squares, the direction cosines of the pole are (1937.0): $\lambda = +0.0553$, $\mu = -0.3601$, and $\nu = +0.9314$. The observed radiants fit the great circle with a probable error of $\pm 5'.0$, but are not spaced along it uniformly with the time to this accuracy. A least squares solution for the motion in α from Table I gives

$$\alpha(1937.0) = 52^\circ.2 + 0^\circ.50(\pm 0^\circ.06)T', \quad (9)$$

where the time T' is measured from Nov. 0.0 (longitude of the sun $217^\circ 14$, 1937.0). The probable error of a single observed α is $\pm 30'$.

Since the apparent radiants are subject to terrestrial effects, a similar solution was made for the corrected radiants of the first five meteors in Table I with the result that

$$\alpha = 50^\circ.68 + 0^\circ.52(\pm 0^\circ.03)T'. \quad (10)$$

The probable error of a single observed α is reduced to $\pm 14'$, although the corrected radiants do not fall on a great circle as accurately as before.

The seven singly observed meteors of the 1937 Taurid shower are not included in the above calculations. No precise method is available for correcting single trails for terrestrial effects, since neither velocities nor apparent radiants are known. In the present reduction the observed values of V_∞ for the first four meteors of Table I were used in a least squares solution to solve for V_∞ (km/sec) as a linear function of the time of apparition, as follows:

$$V_\infty = 32.91 - 0.329(\pm 0.032)T'. \quad (11)$$

The radiant of a singly observed meteor was taken as the intersection of its trail with the path of the apparent radiants as given by the direction cosines above. With aid of Equation (11) these radiants were corrected for terrestrial effects in the usual manner. The corrected trails were passed through the corrected radiants at the original angles with the path of the apparent radiants. Because only small distances were involved, a rectangular system was set up about the path of the apparent radiants and the various calculations were made in this system.

The final least-squares solution for the radiant motion was made from all of the material available. For a doubly-observed meteor one equation was used for uniform motion along the radiant path (assumed to be a great circle), and another for the deviation from the path. For singly observed meteors one equation was used to derive the minimum distance of the trail from a point moving uniformly with time along the radiant path. Each equation was given unit weight in the solution, so that the doubly-observed meteors affect the solution with a double weight.

The pole of the path of the corrected radiants has direction cosines (1937 0) $\lambda = -0.0624$, $\mu = -0.2666$, and $\nu = +0.9618$. On Nov. 0.0, 1937, the calculated radiant was $\alpha = 50^\circ.52$, $\delta + 14^\circ.32$. The daily motion was $0^\circ.536$. In Table VII data are given for the

TABLE VII
DEVIATIONS OF RADIANTS

Meteor number	Singly-observed meteors							
	699	700	701	704	708	714	715	719
Plate number	IR 1730	AC 33873	AC 33873	RH 7847	IR 1764	AI 33893	RH 7884	AI 33906
U T 1937	Oct. 31.338	Oct. 31.348	Oct. 31.318	Nov. 5.251	Nov. 8.179	Nov. 10.275	Nov. 10.379	Nov. 22.046
Pole of λ	+0.06273	+0.48742	+0.54834	+0.48217	+0.76330	+0.80526	-0.86491	+0.13376
trail μ	-0.39475	-0.08067	-0.14067	-0.04931	-0.34728	-0.57339	+0.47645	-0.36319
1937 0 ν	+0.91410	-0.86375	-0.82434	-0.87469	-0.84476	+0.15090	+0.16780	+0.90068
Slope	-0.0539	-0.8577	-1.0885	-0.8281	-2.765	+2.350	+13.778	+0.0881
Corrected λ	+0.6068	+0.6042	+0.6062	+0.5806	+0.5584	+0.5418	+0.5238	+0.4140
radiant μ	-0.7536	+0.7585	+0.7554	+0.7748	+0.7890	+0.7996	+0.8096	+0.8690
1937 0 ν	+0.2528	+0.2444	+0.2439	+0.2518	+0.2584	+0.2619	+0.2673	+0.2711
Residual	9".8	9".8	3".5	10".3	12".6	22".1	60".2	11".3

Meteor number	Doubly-observed meteors				
	697	705	710	713	716
U T 1937	Oct. 31.35	Nov. 5.26	Nov. 8.21	Nov. 10.28	Nov. 10.28
Residual along path	+18".2	-25".2	-1".4	-4".1	-8".7
Residual perpendicular	-25".2	-9".2	+11".7	+39".0	-5".4
Total residual	30".3	30".5	11".8	29".3	7".8

eight meteors singly observed, and for the five doubly observed. Most of the entries are self explanatory. Direction cosines are given for the poles of the uncorrected trails and for the corrected radiant points. The slope measures the direction of the original trail and is positive for increasing values along, and perpendicular to, the uncorrected radiant path. The residual is the perpendicular distance from the corrected trail to the calculated radiant for a singly observed meteor. The average value is $17'.6$. The total residual for a doubly-observed meteor is the distance from the observed to the calculated radiant. Its average value is $23'.1$. We should expect the average residual for a singly observed meteor to be $1/\sqrt{2}$ of this quantity, or $16'.3$, in close agreement with the observed value.

The accuracy of measurement and correction for the trails is much higher than these residuals would suggest. We must conclude, therefore, that the residuals arise from an intrinsic spread in the directions of motion of the meteors in space *plus* random deviations occurring in the earth's atmosphere. A probable error of about $20'$ in the radiant point is to be accounted for. If the error arises from deviations in the atmosphere, very large deviations from linearity should be observed in the trails. An angle of $20'$ would be about one hundred microns in two centimeters along the plate, a tremendous angle compared to the few microns of irregularity observed uncertainly over the entire lengths of the measured trails. For a discussion, with references, on the linearity of meteor trails, see Arend and Hunaerts (1939) and Whipple (1940). It seems impossible that any significant deviations could occur in the higher (low-density) regions of the atmosphere before the meteors become visible. The direct deceleration caused by atmospheric resistance is small even at the middle of the trail. Possible deviations arising from the action of the earth's magnetic field on charged meteors are several orders of magnitude below the observed deviations. There appears to be no escape from the conclusion that the probable error of $20'$ in the observed radiants of the Taurids is almost entirely inherent in the stream itself.

The extended trails and radiants of the thirteen meteors of Table VII are shown in Fig. 6. The x -axis is the finally-corrected path of the radiants with $x = 0$ at the Nov. 0.0 position. The coordinate x increases with α (and time) and y with δ . The residuals are indicated by short perpendicular lines. The zero point is

4°.0 south of the ecliptic, and the path of the radiants is inclined 8°.7 to the ecliptic in the sense that with increasing date the radiants move farther east and south.

13. Comparison of the Radiants with Previous Observations

In a compilation of Taurid and Arietid radiants, W. F. Denning (1928) listed thirteen that he considered as well established in the region along the ecliptic from $\alpha = 21^\circ.6$ to $\alpha = 80^\circ.5$. The most frequently observed shower he associated with λ Tauri, the mean visual radiant being at $\alpha = 55^\circ.7$, $\delta = +14^\circ.2$. This shower is clearly the principal one of the present discussion (see

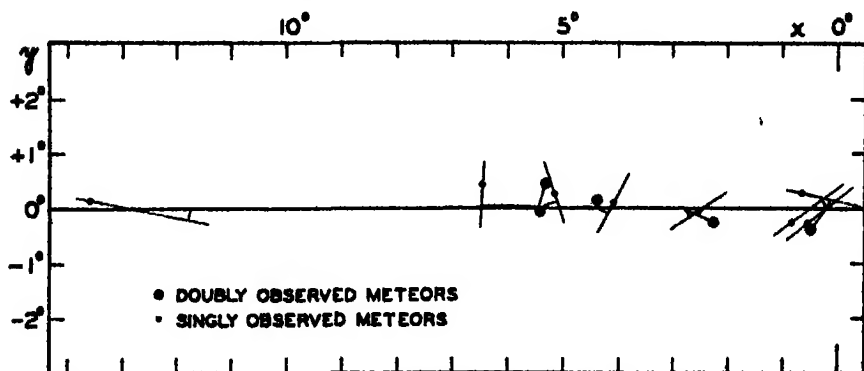


FIG 6 Corrected trails and radiants

Table I). It was observed by the Arizona Meteor Expedition on Nov. 3, 1931 at $\alpha = 54^\circ$, $\delta = +15^\circ$, but only on that one night in 1931 and not at all in 1932. Öpik (1934) lists its single occurrence as one of the most certain showers in his compilation. Olivier (1921) lists several radiants that probably belong to this shower but usually only a few members were observed, so that certain identification is more difficult.

Meteor 778 possibly belongs to Denning's second most frequent Arietid shower (ϵ Arietis) at $\alpha = 41^\circ.9$, $\delta = 21^\circ.4$, although the right ascension for Meteor 778 is somewhat high ($48^\circ.2$). Öpik lists a similar radiant observed on three nights: 1931 Oct. 30–Nov. 1, $\alpha = 42^\circ$, $\delta = +20^\circ$; 1931 Nov. 2, $\alpha = 44^\circ$, $\delta = +14^\circ$; and 1932 Nov. 3, $\alpha = 49^\circ$, $\delta = +20^\circ$. There is some question as to whether these radiants all belong to the same stream, and more uncertainty as to whether Meteor 778 is strictly a member. There is no doubt that it is an associated Taurid and probably these other

radiants, whether really the same as the radiant of Meteor 778 or not, also represent associated Taurids.

Meteor 642 of a previous study (1938) is probably not sporadic but is associated with the Taurid shower. Its radiant ($\alpha = 43^{\circ}.0$, $\delta = +12^{\circ}.5$) is very close to a less frequently observed radiant in Denning's list (σ Arietis) at $\alpha = 41^{\circ}.9$, $\delta = +13^{\circ}.7$. Öpik lists two radiants, not too certain, that are similar 1932 Oct. 25, $\alpha = 48^{\circ}$, $\delta = +12^{\circ}$, and 1932 Oct 31, $\alpha = +40^{\circ}$, $\delta = +15^{\circ}$. If Denning's radiant near σ Arietis is accepted as real, there can be little question of the identification of Meteor 642 with the shower

Since two, and possibly three, recognized showers are represented in the present discussion, and since they are all associated in origin, it is probable that other recognized October–November showers are also associated with these and, therefore, with Encke's comet.

Knopf (1931), from a study of the catalogue of the orbits of fireballs as compiled by v Niessl and Hoffmeister (1925), has concluded that there exist at least two extra-solar streams of meteors. The demonstration is based chiefly upon the small daily motions of the radiants. One of these streams, which he calls the Taurus stream, has thirty members in the catalogue of fireballs, twenty of which were observed in the interval from Oct. 14 to Nov 25. The mean latitude of these twenty observed radiants is $+0^{\circ}.4$, the mean longitude $58^{\circ}.0$, the mean date Nov 10, and the mean daily motion about $+0^{\circ}.3$. The λ Tauri radiant is at latitude $-5^{\circ}.4$, longitude $56^{\circ}.7$, and has, from the present study, a mean motion of $+0^{\circ}.5$. The agreement is rather close except for a difference of nearly six degrees in latitude. The twenty catalogue fireballs selected by Knopf as representatives of the Taurid extra-solar stream are identical with my selection for representatives of the Taurid short-period stream on the assumption that there are several sub-streams

Another method of identifying the fireball radiants is by comparison with the thirteen Arietid and Taurid radiants as listed by Denning. If the frequencies are taken proportionately to the number of radiants observed in intervals as given by Denning, it is possible to obtain a mean radiant and motion at any date. Such a summation leads to a mean latitude of $-1^{\circ}.4$, longitude $56^{\circ}.4$, and a mean motion of $0^{\circ}.35$ on Nov. 10, in somewhat better

agreement with the data for the twenty fireballs selected from the v. Niessl-Hoffmeister Catalogue. There can be little doubt that the same streams are represented in the two groups of observations. Since the major streams of Denning's list have been shown to have elliptical orbits about the sun, the Taurid stream studied by Knopf must also be of solar-system origin. There is no difficulty in accounting for the remaining ten fireballs observed at other times of the year as accidental inclusions.

14. Physical Results

For the sake of completeness and for reference purposes, the chief physical observations of seven Taurid meteors are given in Table VIII. The notation of previous papers is preserved (1938)

TABLE VIII
PHYSICAL OBSERVATIONS

Meteor No	697	705	705	705	705	710	712	716	719	778
<i>B</i>	33 007	40 798	30 721	32 88	31 95	30 324	29 463		20 086	33 908
<i>p e</i>	± 0.119	± 0.029	± 0.058	± 0.27	± 0.92	± 0.014	± 0.020		± 0.062	± 0.123
<i>C</i>	-0.034	-0.0045	-0.0102	-0.287	-0.051	-0.0464	-0.0064		$+2.587$	-0.045
<i>p e</i>	± 0.003	± 0.0006	± 0.0010	± 0.018	± 0.006	± 0.0006	± 0.0002		± 0.006	± 0.006
<i>d</i>	4.14	3.19	2.99	1.61	2.30	4.95	4.37			4.00
No breaks to (sec)	15 0.450	15 0.550	15 0.950	11 1.400	7 1.850	18 0.425	20 0.550		21 0.525	12 0.325
<i>V_s</i> (km/sec)	32.09	30.65	30.20	28.47	23.64	30.17	29.20	29.20	27.03	33.25
± 0.14	± 0.04	± 0.08	± 0.37	± 1.32	± 0.05	± 0.02				± 0.15
$-V'_{s}$ (km/sec ²)	3.82	0.60	1.66	7.11	19.12	1.75	1.14			2.62
± 0.33	± 0.08	± 0.15	± 0.40	± 2.18	± 0.01	± 0.04				± 0.36
$-V'_{s}/V_{s}$ (sec ⁻¹)	0.119	0.020	0.052	0.250	0.800	0.058	0.039		0.072	0.079
<i>h_s</i> (km)	90.86	92.88	85.66	75.55	64.21	92.24	94.02		89.45	85.74
<i>h₀</i> (km)	80.50	86.68	78.54	69.66	61.69	80.64	79.41		81.46	77.04
<i>h₁</i> (km)	76.81	83.16				71.90	73.06	81.50	75.26	72.02
<i>h₂</i> (km)	70.80	79.54	71.55	64.21	59.49	69.80	65.21		72.32	66.73
<i>M_s</i>	-0.82	-0.89				-1.18	-1.79		-2.51	-1.32
<i>M₀</i>	-1.74	-3.20	-3.37	-2.78	-1.49	-1.58	-3.72			-3.50
<i>M₁</i>	-2.85	-3.78				-1.94	-4.60	-4.65	-4.41	-3.79
<i>M</i>	-2.05	-3.72				-1.70	-4.00		-4.01	-3.08
$\log p b$	-8.24	-8.52				-7.85	-8.16		-7.71	-8.35
$\log p o$	-7.23	-7.51	-7.19	-6.74	-5.36	-7.40	-7.18			-7.16
$\log p m$	-7.20	-6.92				-7.28	-6.89	-6.81	-6.90	-7.20
$\Delta \log p b$	-0.13	-0.17				+0.37	+0.20		+0.28	-0.67
$\Delta \log p o$	+0.01	+0.25	-0.07	+0.03	+0.13	-0.15	-0.01			-0.12
$\Delta \log p m$	-0.19	+0.55				-0.42	-0.02	+0.52	+0.04	-0.36
<i>m₀</i>	0.5	14	6.6	1.1	0.3	1.1	15			3.1
<i>m₁₀</i>	3.4	18				2.8	27		33	7.5
<i>r₀₀</i>	0.6	1.1				0.6	1.2		1.3	0.8

The quantities *B* and *C* are the coefficients of *t* and *t*², respectively, in a least-squares solution for distance as a function of time along the trail (km and sec), except for the singly-observed Meteor 719 where the solution is made in 1/24 in along the plate. The deceleration is taken proportional to the function exp (*dt*), where

d is assumed from the data. The number of breaks applies to the interval over which the deceleration, $-V'$, is derived. The subscripts b , o , m , and e refer to the first break measured, the middle of the breaks, the point of maximum light, and the last break measured, respectively. Height above sea level, absolute visual magnitude, atmospheric density, mass of the meteoroid, and its radius, are designated by H , M , ρ , m , and r_m , respectively. For ρ , m , and r_m cgs-units are used.

For Meteor 705 it was possible to measure the deceleration with reasonable precision at four points along the trail (by a slight overlapping of the four sets of breaks). The deceleration is seen to have increased from 0.6 km/sec^2 at a height of 93.9 km to 19 km/sec^2 at 64.2 km , near the end. This large increase in deceleration is in good agreement with the theory, and strongly supports the method of correcting the velocity by a relatively small amount for the deceleration incurred at heights above the first observable break.

The velocity, V_o , for Meteor 716 was assumed, while H_m and M_m were measured. The density ρ_m is thus not an independently observed quantity.

For Meteor 719, singly-observed, the mean height-density relation was used in solving for V_o , H_m , and ρ_m . The ratio $-V_o'/V_o$ was also assumed, so that the effect of deceleration could be included in the solution for the radiant from measures of the breaks.

The methods of determining atmospheric density have been described elsewhere (1938, 1939). The residuals, $\Delta \log \rho$, were obtained from the adopted curve of $\log \rho$ as a function of height (1938), after correction for the seasonal term with a double amplitude of 0.48 in $\log \rho$. The values of ρ given in Table VIII are represented in the figure published elsewhere (1939), including all of the measured values of atmospheric density so far derived from the photographic meteor data.

It may be stated that the earlier conclusions concerning the distribution of temperature in the earth's upper atmosphere are generally confirmed by the additional observational material.

15. General Discussion

The multiplicity of radiants, the uniformity and the long endurance of the Taurid stream of meteors have disguised its character as one of the more important known showers. Photographically, from a search of the Harvard plates, the shower is

as well represented as any. Visually, too, it is well represented, although one gains the impression that it may not be relatively as strong visually as photographically. The tendency, however, of visual observers to note members of better known showers may seriously affect the tabulated frequencies. It is still unsafe to attempt to evaluate the ratio of large to small meteoroids in the shower, for comparison with the ratio for other showers.

One fact is of considerable importance in attempting to estimate the total population of the stream. The value of ω is such that $d\Omega/dt$ is large (see Equations 5 or Fig 5). Thus the individual orbits are suffering relatively large perturbations in Ω , approximately four times the minimum value. In only two or three hundred years the node of a single meteoroid retrogrades forty degrees, or through most of the present extent of the shower. If a considerable number of streams associated with Encke's comet are not in a position to produce meteor showers, as seems most probable, the correction factor involved in the calculation of the total population of the associated streams must be quite large. Without attempting a numerical calculation from the uncertain material, the author believes that the total population of the associated Taurid streams is comparable with that of any of the generally recognized streams, except possibly the Perseids and Leonids.

Without stressing the point, I should like to suggest the possibility that the maximum intensity of the morning zodiacal light in October–November as observed by Elvey (1937) may be produced by the meteoric material of the greater Taurid stream. The circumstances are suitable if the zodiacal light is produced by sunlight reflected and scattered from meteoric material. This explanation suffers from the major defect that no maximum of the evening zodiacal light is observed in June–July, when the earth recrosses the projected orbit plane. Such a maximum may actually exist but not have been observed because of the unfavorable position of the evening zodiac at this season of the year. The alternative, however, casts doubt on the reality of the morning maximum in October–November when the zodiac is very well placed. A comparison of Fig. 3 in Elvey's paper with Fig. 4 of this paper will show the geometry of the problem.

The theory of the secular perturbation of Encke's comet as presented in the present paper is only a rough approximation.

The author hopes that someone will attack the problem with a much more complete theory, in order to throw more light on the history of this comet and its large family of meteor streams. The weight of more observations *plus* an improved perturbation theory should eventually provide a much better idea of the life time and possible origin of the whole system.

Two important processes that bear on the development of a meteor stream have not been discussed, an electro-magnetic process and the Poynting-Robertson effect. The electro-magnetic process arises from the fact that meteoroids must carry a positive electric charge as a result of the photo-electric effect produced by the sun's radiation. Under the action of the sun's magnetic field a motion of the line of apsides must result, a greater motion is to be expected in the case of smaller meteoroids. Preliminary calculations suggest that the effect is not inappreciable. In addition, the simple electrostatic effects would tend to disrupt a compact stream.

That a tangential drag, equivalent to a resisting medium, should affect the motion of small particles moving about the sun as a consequence of absorption and re-emission of solar radiation, was shown by Poynting (1903) and re-investigated by Robertson (1937). This process tends to decrease the period of revolution, and, over long intervals of time, is quite appreciable for meteoroids of the size studied here. Since the effect varies inversely as the diameter of the body, one would expect that the less massive meteoroids of the present study would have shorter periods, and therefore lower heliocentric velocities, than the more massive ones, if the effect has contributed appreciably to a shortening of the period of the stream as a whole. A comparison of the heliocentric velocities with the calculated masses showed no significant correlation. We must conclude that the observational material is not sufficiently accurate for a crucial test of the theory. Plummer (1905) has shown that an inconsistency is introduced if the effect is used to account for the anomaly in the motion of Encke's comet. The effects of the electro-magnetic and the Poynting-Robertson processes on the structure of meteor streams are to be studied further.

The author wishes to express his great appreciation to the J. Lawrence Smith fund for a grant that provided for the major part of the multifarious calculations involved in the present study, and to acknowledge the able assistance of Mr. M. Hurahata in carrying out these calculations.

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